

**THE RESEARCH PROCESS : THEORIES AND METHODS
IN AN ETHNOGRAPHIC STUDY OF INFANT SCHOOLS**

FOUR VOLUMES : VOLUME 2

by

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CHAPTER ONE : THEORETICAL AND METHODOLOGICAL ISSUES

INTRODUCTION

The principal aim of this chapter is to present the research approach adopted for this study and to detail the methods used for the collection and analysis of data, the results of which form the main body of the thesis. However, the issue of an alternative approach is considered in some detail for reasons given below.

The first section of the chapter sets out the chief concerns of the research at its commencement. Principally, these were to identify those features which seemed essential for an 'understanding' of infant schools, to give a picture of the life in these in some detail, as lived by those involved, in order to add something to other accounts if possible and also to compare accounts. These concerns were noted in the previous chapter as arising from three sources; a reading of two sociological studies of infant schools, from interest in the then 'New' Sociology of Education, and from teaching experience in an infant school. The section ends by briefly stating the approach chosen and the reasons for this at the time.

The second section of this chapter discusses 'two' apparently different 'traditions', 'approaches', 'styles' or 'perspectives' (all terms used in the literature) associated with sociological research. The approach chosen is one of these. This section is in the nature of a reflection on a learning process which mainly, though not entirely, took place after the fieldwork. The section contains two subsections.

The first concentrates on that form of research which, among other things, is often termed 'positivistic' or 'normative' and which is usually associated with 'the scientific method' or 'quantitative methods'. The subsection notes some problems of definition and the reasons for these. It then traces the development of this approach and ideas associated with it. It discusses the epistemological debates on the nature of 'scientific'

reasoning which have taken place in the course of this development. It discusses whether there is one model of 'the scientific method', and the question of the assignment of 'quantitative' methods to this approach. There is some comparison with certain features associated with the other approach, where this seemed relevant, although details of this are given in the following subsection.

The next part of this section, then, outlines the other research tradition, which can be overall classed as 'interpretive', and which is usually associated with 'qualitative' methods. It is noted first that there are problems in discussing this approach. These arise first because there are various theoretical ideas subsumed under the heading of 'interpretive' which have rather different origins, and secondly because the theoretical and methodological aspects are often described by the same terms. Secondly, therefore, this subsection briefly notes the different origins. It then discusses the 'methods', both in terms of strategies for data collection and the reasoning applied to these, which are associated with the overall approach, referring back to the previous subsection where relevant.

The third section of the chapter presents an over-view of the research stages, giving a time scale.

The fourth section of the chapter discusses the issue of the choice of particular schools in which to carry out the research, and the degree to which the term 'choice' is an accurate description of this process. The question of why an infant school rather than a primary school was the focus of research was discussed in the Review chapter. This section, therefore, is concerned with how the actual 'choice' was made of which particular schools to look at, and the processes involved.

The fifth section deals with the establishment of 'Field Relations'. These include the issue of access, the establishment of the research 'role'

and the development of 'rapport'. These three processes are inter-linked, but are partially discussed separately, especially the problem of 'access'.

The first part of this section is devoted accordingly to the question of access. This is related to the 'choice' of schools, since the final selection of a research site depends on gaining admittance to the site 'chosen'.

This part of the section discusses how initial access was gained both to the pilot study schools and the main research schools.

Related to the problem of initial access are various issues, such as 'the presentation of self', involving 'impression management', which are referred to by other writers, and are therefore discussed.

It is noted that the problem of access, once 'in', is not a once for all event in the research process, and that there are no fixed rules.

The second part of this fifth section discusses the issue of the establishment of a 'research role'. It discusses the various roles which can be 'taken' within the overall approach adopted, and notes some problems in distinguishing between these. It then notes the roles 'taken' by some researchers in other areas of sociology or anthropology, and then those 'taken' by some researchers in schools, and finally, that 'taken' in the present research.

The establishment or not of 'rapport' or 'friendly relations' is noted in this part of the section at various points. It is pointed out that this has a strong influence on the actual research role possible, and thus may affect the collection of data. The establishment of 'rapport' is shown to be linked with questions of both the 'presentation of self' involved in gaining and sustaining access and with perceptions of the

researcher and the research which are possessed by the subjects of this. Thus, the issue of 'rapport' is very closely related to the establishment of the research role, and is therefore not discussed separately in this section.

The sixth section of this chapter discusses in turn the methods of data collection utilised during the research. These were respectively observations, interviews, which ranged from unstructured to semi-structured 'conversations' of different lengths, and the use of various documents. These include school based ones such as notes for teachers, record books and also a questionnaire. In this section, a brief note is also made about the documentary sources for the historical chapters. This part of the section briefly notes the reason why an historical dimension was added to the study. It partly arose from the research and was seen as a means of complementing the empirical work by placing it in a wider context, and also partly from previous reading such as Sharp and Green. The importance of the context in which these methods were used is noted.

The section then discusses the methods of recording the data.

Finally, the section discusses the issue of validity, or the question of accuracy of the account presented by the researcher, and factors which can affect this. This involves the concept of triangulation.

The seventh section discusses the analysis of the data collected, and the stages of this process. It discusses the initial establishment of categories, and the subsequent attempts to develop these by 'progressive' focusing. It indicates the attempt to let these categories emerge from the data and not be imposed by the researcher. The section emphasises that analysis is continuous throughout the research process. It therefore considers both analysis done during the 'field work' and that carried out afterwards. It is noted that this last continues up to and includes the final 'writing up' stage.

The final section of this chapter is a reflection on the research. It brings together some of the more general problems associated with this form of research which have been noted at various points in this chapter. It also discusses the personal problems experienced by this researcher, including the feelings engendered. Personal issues are considered because they can have consequences for the quality of the data collected. Personal reflection is also necessary in order to give a 'true picture' of the research process, as many of the most recent texts on research methodology on fieldwork indicate (e.g. Burgess, 1984b, p. 4; Hammersley, 1984, p. 1; Hargreaves, 1987, p. 17).

SECTION ONE THE CONCERNS OF THE RESEARCH

The review of the literature, particularly of that which related to infant schools, together with personal experience as an infant teacher, gave rise to the view that certain issues were important for an 'understanding' of 'the infant school' world. This chapter begins, therefore, by attempting to set out these issues. It attempts to set these out separately, but they are in fact interlinked.

The basic question which was posed was an ontological one, as to the nature of an 'infant school'.

A second question, linked to this first, was whether there was a distinctive approach in 'the infant school', with a set of shared ideas and practices among teachers.

The issues with which this research was concerned arose from these two questions.

The first question seemed to require an attempt to delineate what features might constitute an 'infant school'. Thus, a major concern of the research was overall with the everyday life of an infant school, in order to produce a 'living picture' of this environment.

As stated in the review of the literature, there was a lack of data in Sharp and Green's account of an infant school of what people actually did. They seemed, unlike King, to concentrate more on what teachers said they did.

As part of this attempt to portray the 'infant school', it was considered that it was necessary to take account of how the various actors within it defined and described their world.

The first direct issue arising from this first concern with presenting a picture of 'the infant school' was the position of the head in relation to staff, pupils and parents. Both personal experience and the reading of Sharp and Green and King indicated that this position was an important feature. Given the small size of most infant schools, a head seemed very likely to be an influential actor because of the closer contact with other teachers. This was not initially a major focus of the research but was an area of interest which developed strongly as the research proceeded.

The second issue was what was the daily routine of the infant school, and what kind of interaction took place within the classroom between teachers and pupils.

The third issue, as part of discovering the meaning that teachers attached to their activities, was a concern with whether teachers, including heads, could articulate their teaching beliefs, and if so, what these were, and whether they were linked to practice.

This issue is directly connected to the second question above. As indicated in the literature review, King held that 'child-centred' and 'progressive' ideas were axiomatic in the infant school. The more recent research of Hartley, for example, has presented a rather different picture. (King, 1978; Hartley, 1985).

The content ascribed to these 'ideologies', the problems of definition, and possible conflict in ideas in schools is explored in Chapter Three, which is a reference point for discussion of terms such as 'ideology', 'child-centred' and 'progressive'.

A fourth issue, arising from the third, was whether there were any constraints which limited what teachers were actually able to do in a school, as distinct from what they might want to do, and whether teachers were aware of these. As noted in the Review, one criticism of Sharp and Green's claim that a phenomenological approach could not deal with structural issues was that they had not asked the teachers directly about any such awareness. (Hargreaves, D., 1978).

A fifth issue, directly related to the fourth, was Sharp and Green's view that there was a need to place:

"... teachers' world views and practices within the context of social and physical resources and constraints which they may or may not perceive, but which structure their situation"

(Sharp and Green, 1978, p. 30)

It was pointed out in the Review of the literature that if this was necessary to 'understand' an infant school, then such contextualisation should include an historical dimension. Therefore, attention should be paid to the development of infant schools. But it is further argued in this thesis that as part of this 'historical aspect' of context, attention also needs to be paid to the circumstances in which ideas, such as a 'progressive ideology' develop, and the reasons for such development. Sharp and Green did not apply their own comments on the need for consideration of a wider structural context to the 'child-centred' approach. They just gave one or two references, but otherwise seemed to take it for granted. (Sharp and Green, 1978, p. 40).

The initial concern, however, was with what ideas concerning infant practice did actually exist in the schools. There was an

initial interest in the history of state education because of previous study, but the concern with infant education in particular was developed as the research progressed in the main school, and as Sharp and Green was re-read during the research. It consequently appeared to be important to trace the ideas governing the origin of infant schools and 'the infant tradition' which was claimed by Silberman to exist. (Silberman, C., 1970). A concern with history, however, did not imply ideas of determination.

Although these issues had been identified prior to the start of the fieldwork, it has to be emphasised that the researcher did not begin with a clearly formulated list of pre-set questions. There was a concern to wait and see what the schools were like, and the idea in general was that there should be as little pre-conception as possible in relation to the issues.

Having established that there were certain issues with which the research would be concerned, the problem then was to decide which seemed to be the most suitable method for exploring these. As noted in the Review of the literature, research about schools had utilised different methods. The choice of a particular method appeared to depend on such factors as the purpose of the research and the resources available, but also to some extent on the climate of opinion on the nature of research, and the attitude of researchers to this.

The 'choice' in this present research was not essentially a cut and dried matter of a prior careful rational consideration of alternatives although there was an element of this. Rather, it was a matter of initial interest leading to a preference. This was perhaps influenced by being at college and later university when the then 'New' Sociology of Education was becoming more popular, as noted in review of the literature.

The approach chosen, the 'interpretive' was selected less on the basis

of a detailed comparison with others but rather because of its expressed concern with social interactions and the subjective meanings attached by actors to these. As noted, one of the central concerns of this research was to see how teachers in an infant school made sense of their 'situation'. Participant observation seemed the best strategy for discovering this, and this was associated with the interpretive approach. From a reading of the available methodological texts which were concerned with this approach, it seemed more relevant for the purposes of this research.

Also the studies of Sharp and Green and King, whatever their possible gaps, and their theoretical differences, had seemed more interesting than some of the survey research read, because they were actually talking to teachers and pupils, to a greater or lesser extent, and describing classrooms. Having been an infant teacher, there was a real interest in seeing how teachers in other infant schools worked, and particularly in directly observing them in the classroom and talking to them about their work. Bulmer noted the influence of 'subjective appeal' on methodological choice. (Bulmer, M., 1977, p. 29).

Some knowledge had been gained from earlier work about what is still sometimes referred to as 'the scientific method'. However, this knowledge was not particularly detailed.

Some gaps had been noticed in studies which had used a survey approach, which it was thought was linked to this method. It was considered that such studies had not generally been very concerned with the meanings of teachers, and this view influenced the choice at the time. It was not that the 'other' method was rejected out of hand, but it was not seen as enabling all the issues to be explored in sufficient detail.

In terms of a theoretical position, the researcher was not committed to any particular theory, particularly in relation to 'society'. This last was in fact seen as both socially constructed and in a sense as also

existing 'out there' at any one moment.

The next section of this chapter deals with theoretical and methodological concerns.

SECTION TWO THEORETICAL TRADITIONS AND ASSOCIATED METHODS

Introduction

This section of the chapter discusses two research traditions, of which the one used is placed second.

They are placed in the order they are because this section is partly a consequence of a process of learning which was extended after the fieldwork.

As noted in the Review and in Section One of this chapter, one of the concerns of this research was what teaching in an 'infant school' meant to the teachers themselves, and another was to look at classroom life. With these concerns, and in the light of initial reading and subjective interests, the 'interpretive' approach had seemed in general more useful and was therefore followed.

It was suggested after the fieldwork that to write about the methods used for a methodology chapter some comparison with other methods should be made, partly to justify the method chosen. It was found to be impossible to discuss 'methods' alone. This was because this term had a number of uses. For example it included 'the scientific method', the 'participant observation method', 'quantitative' and 'qualitative' methods, the analytic method, the methods of data collection, the 'survey method', the 'experimental method' and so on, quite apart from 'methodology' used either as a general term to refer to the study of methods or interchangeably with 'methods'. The various usages seemed to refer to different levels of meaning, either referring to the whole research process or to various stages within it, each of which had a baggage of ideas connected with it.

So the process of comparison led to a more detailed consideration of theoretical and methodological issues. Thus, as part of a form of 'progressive focusing' on theories and methods, and as part of the process of 'reflexivity' in relation to the research, the 'positivistic' tradition was reconsidered and compared with the approach used. The issues had to be gone into in more detail because although the arguments were 'known' in a general way, they had not been explored by the researcher in any depth before or during the fieldwork. As explained in the next section, there were some important immediate decisions to be made under the pressures of time, and these had to be taken in the light of available knowledge. Only in the light of later reading, particularly when following up Hammersley and Atkinson's references, (a book not available during the fieldwork) was it fully realised how complex the debate really was.

In an interpretive study, the researcher is as much a subject of the research as the other people being researched. He/she has thus to be self-reflecting throughout. Part of this reflective process is becoming aware of gaps in theoretical and methodological understanding. In presenting both approaches it is inevitable that further gaps will be revealed, but it is the intention in this section (and indeed in the whole thesis) to present an honest account of current knowledge, within the limitations of space. The act of writing is the process of reflection as well as the result of it. As reflection, this is necessarily a long section. The ideas involved in the different traditions and in the various debates are complicated, whether referring to the fields of philosophy or sociology. Hence over-simplification can occur in the reception of ideas and in the writing about them, particularly when several texts in either field are not unambiguous. But even summarising complicated arguments involves some length. The main point, though, in considering in more detail than originally envisaged the 'tradition' not

originally chosen was to go beyond 'taken for granted' views both in some interpretive texts and in the researchers' own mind. In the same way, it seemed necessary to indicate what the 'interpretive' approach was thought to comprise. If, in writing about these traditions, it seems as if the conclusions reached are 'what everybody knows' anyway, the researcher can only comment that it was not like that for her, but was gone through as a process, and that is what is being presented.

In terms of order, it seemed more sensible to set out first what is known of the 'alternative' approach, to provide a backcloth for discussing the interpretive. The ordering of the section also to some extent follows the historical order of research in education, and the discussion therefore of studies in the Review of the literature.

1. 'Positivist' Sociology and 'The' Scientific Method'

At the time the research began it was noticed that texts dealing with 'interpretive' or 'ethnographic' sociology frequently referred to 'positivism' as an opposing tradition. The references to this often amounted to little more than statements that it incorporated a particular view of 'society' and used 'the scientific method', or 'quantitative methods'. It was not entirely clear whether these two meant the same thing. In any case, the methods were said to take no account of the subjective meanings which social actors attached to situations and actions. It was also held that researchers, in being 'objective' stood apart from the situation, or thought that they could. These methods were then rejected as inappropriate for the sociological enterprise. As Marsh stated:

"Much of the scorn against positivism is in fact an attack on the idea of a social science."

(Marsh, 1982, p. 49)

Like Giddens and Hammersley and Atkinson, Marsh also pointed out that 'positivism': "... has become a term of abuse first and foremost". (p. 49)

(cf Giddens, 1979; Hammersley and Atkinson, 1983, p. 4). Hence, the initial approach was to try to discover what the term referred to exactly, beyond being a 'term of abuse'. This was found to be a difficult task, because of the lack of one clear meaning. Outhwaite commented that the term was: "... notoriously ambiguous". (Outhwaite, 1987a, p. 161). Halfpenny in fact identified twelve 'positivisms'. (Halfpenny, 1982). Hammersley and Atkinson similarly noted that the term was: "... used in a confusing variety of ways". (Hammersley and Atkinson, 1983, p. 4). Kolakowski acknowledged the difficulty and advanced one reason for it. He stated that while:

"... the existence of a 'positivist' current in nineteenth and twentieth century philosophy is universally acknowledged"

there was difficulty.

"When we try to define this current and to formulate rigorous criteria setting it off from other currents."

(Kolakowski, 1972, p. 10)

He added that this was a normal situation with ideas because:

"... the interpretation of ideas, the way in which one current influences another or reacts against it, not to mention ambiguities in the texts themselves, mean that there is always room for more than one interpretation."

(Kolakowski, 1972, p. 10)

A similar problem with ideas had been discovered already by the researcher with certain concepts used in connection with teachers' beliefs, a matter discussed in Chapter Three.

Giddens also offered an explanation for the lack of: "... an accepted and standard meaning" (p. 2) for 'positivism'. He said that this was because of its use as a "critical weapon" in philosophy and sociology. (Giddens, 1974, p. 2). Elsewhere he pointed out the:

"... indiscriminate way in which the term has been used in a number of polemical exchanges in the last few years."

(Giddens, 1979, p. 237)

He was referring generally to debates in philosophy and sociology, as noted, but the same comments might be made in relation to terms like

'traditional' or 'progressive', in educational debates, particularly when these have been used in connection with disputes in the sociology of education.

Another reason for the lack of a clear meaning seemed to be that 'positivism' is used with reference to different periods. Giddens, who distinguished three ways in which the term could be taken, said that the first of these, in a "more restricted sense", could be applied to:

"... the writers who have actually called themselves positivist or at least have been prepared to accept the appellation."

(p. 237)

He added that in this sense it stood for two phases, "social theory" from Comte and one, from the "logical positivists" which was: "... concerned more specifically with epistemology". (p. 237). This statement indicates that 'positivism' is used not just to refer to different 'phases' but also to different levels of discourse. This seems clear in Giddens' second usage, where he said that the term could be used:

"... to refer to the writings of philosophers who have adopted most or all of a series of connected perspectives; phenomenism, ... the thesis that 'reality' consists of sense impressions ... the thesis that empirical knowledge is logically discrepant from the pursuit of moral aims, and the notion of the 'unity of science', the idea that the natural and social sciences share a common logical and perhaps even a methodological foundation."

(Giddens, 1979, p. 237)

This usage seems epistemological, defining what it means to 'know', and what counts as knowledge. It seems similar to the view of knowledge in Kolakowski's definition of positivism as: "... a collection of rules and evaluative criteria referring to human knowledge", (p. 10) although Kolakowski is more specific. These rules, according to Kolakowski, also distinguished between "... philosophic and scientific disputes", or those which might be settled and those which might not. He specified these rules as, first, "the rule of phenomenism", which meant that there was

no distinction between 'essence' and 'phenomenon', (or any 'reality' apart from the appearance of a thing. (Keat and Urry, 1975, p. 42)). Any idea of such distinction was said to be misleading and not part of science, for:

"We are entitled to record only that which is actually manifested in experience."

The second rule was that of "nominalism". This was said to be a consequence of the first. Kolakowski said that it meant that:

"... we may not assume that any insight formulated in general terms can have any real referents other than individual concrete objects."

A third rule was said to be that "value judgements" and "normative statements" were not knowledge.

These three rules present 'positivist' knowledge as objective, and based on empirical observation.

Kolakowski said that a fourth fundamental idea was: "... the essential unity of the scientific method". This idea was also present in Giddens's second usage. According to Kolakowski, this idea:

"... expresses the belief that the methods for acquiring valid knowledge, and the main stages in elaborating experience through theoretical reflection, are essentially the same in all spheres of experience."

(Kolakowski, 1972, pp. 13-17)

Thus, 'positivism' is used methodologically, to refer to the use of 'the scientific method', as well as epistemologically, where it refers here to a process of "theoretical reflection".

A third level of discourse is the ontological. This is clear from Giddens' third usage of 'positivism'. This was the category of "positivistic sociology", which was the idea of sociology:

"... as a "natural science of society" which can hope to reproduce a system of laws directly similar to those achieved in the natural sciences."

(Giddens, 1979, p. 238)

Giddens pointed out elsewhere that this did depend on a view of the nature

of science. (Giddens, 1974, p. 3). It also seemed to relate to a view of social science.

Thus, in trying to find what the term 'positivism' referred to, it was found to be a matter of debate both in philosophy and sociology. The various debates have a history, which definitions partly reflect. 'Positivism' was used to refer to different periods, and also to different levels of meaning, the ontological, the epistemological and the methodological, with debates existing at all these levels.

It seemed both useful and necessary, in writing about positivism, to put it in context by tracing its development, in order to discuss the problems associated with the different levels. This was also thought to be useful for the comparison of ideas, particularly those relating to 'methods', not just between periods of 'positivism' but also between this and the 'interpretive' approach where it seemed relevant. This in turn was seen as a means of connecting the two parts of the section without at the same time going into full details of the latter approach in a point by point comparison.

The origin of the term 'positivism', as well as the coining of the term 'sociology' for the study of society, is attributed to Comte. However, many of the ideas behind his 'positivism', or rather 'positivist philosophy', had historical antecedents in conservative and radical debates about the nature of society, and debates on epistemological concerns such as those over deductive logic derived from Aristotle and induction developed by Bacon (and later Mill) and Hume's empiricism. (Bierstedt, 1979; Beck, 1979; Cohen and Mannion, 1980; Giddens, 1974, 1979; Hamlyn, 1987; Keat and Urry, 1972; Kolakowski, 1972, Nisbet, 1979).

Comte was particularly concerned with the relationship of individuals and society, which he considered as something more than just a collection of individuals. He was concerned with the question of social order, and

one reason for this was that he was living in a period of considerable political difficulty and social unrest after the French Revolution. (Zeldin, 1977). He concluded from observing the progress of science that 'scientific thought' could form the basis of a new 'humanist' religion which could be the basis of a new social order.

His version of the 'Law of the Three Stages' distinguished a historical development from 'theological' through 'metaphysical' to 'scientific' or 'rational' forms of knowledge. The latter Comte termed 'positive', as a reaction to attempts to go beyond the empirically observed to speculation about 'essences'. Giddens quoted Comte on:

"... the "essential attributes ... summed up in the word positive" ... "an orientation to "reality" and to "utility", the term also implies ... "certainty" and "precision" ... also suggested by the term are an "organic tendency" and a "relativist outlook"." The former of these refers to the constructive character of the positivist spirit ... the latter to the rejection of absolutism ... the laws that govern the covariance of phenomena always retain a provisional character, since they are induced on the basis of empirical observation, rather than being posited as "absolute essences."

(Giddens, 1979, p. 240)

Comte considered that all 'positive' human knowledge was to be found in the sciences. He considered that the 'phenomena' of the natural world were seen in science as subject to laws of development which could be discovered. Comte saw the social world as similarly having laws which could also be discovered in the same way by the application of 'positive' or 'scientific' knowledge. Comte's view of the sciences was said to be that they were: "... hierarchical, in both an analytic and historical sense". (Giddens, 1979, p. 240). This view meant that:

"Analytically ... the sciences form a hierarchy of decreasing generality but increasing complexity, each particular science logically depends upon the ones below it in the hierarchy, yet at the same time deals with an emergent order of properties that cannot be reduced to those with which the other sciences are concerned."

(p. 240)

Comte regarded the study of human society as a science which was "... at the

apex of the hierarchy of sciences", and so depended on the laws of those sciences below it, while having its own set of properties or "subject matter". (p. 240).

Sociology, according to Comte as summarised by Giddens: "... relies on three methodological elements ... observation, experiment and comparison". (p. 241). Empirical observation meant not empiricism as such, but that which was directed in some way. Giddens summarised Comte again. He said that:

"Scientific and popular observation" Comte says, "embrace the same facts" but ... the former is guided by theory Theories direct our attention towards certain facts rather than others."

(p. 242)

Laboratory experiment could be replaced by "... "natural experiments" whose consequences can be analysed". However, it was: "... the comparative method, which is the crucial foundation of sociological research". (Giddens, 1979, p. 242). Three of the named methodological elements of Comte's 'positivism', empirical observation, comparison and the idea of "covariance" between phenomena being "inductively derived from observation are briefly defined here because they are not only retained in versions of 'scientific methods' but are also featured in descriptions of the 'interpretive' approach. They are thus discussed again, but because they are mentioned later it seemed useful to give a preliminary definition as a 'marker'.

What "empirical observation" seems to mean, both for Comte and later 'positivists' is that in accordance with Kolakowski's first two 'rules', there must be first hand study by them of phenomena which have some root in 'real' things, whether these be objects, the actions of people or the ideas expressed by them in some way at some time. 'Interpretive' sociologists also have this objective as part of their approach.

The idea of Comte that 'theory' guides observation is also found later, both in views of science and in certain versions of the 'interpretive' approach. But, as Denzin observed, the term 'theory' has several meanings.

(Denzin, 1978, p. 47). Comte could have meant what both later scientists and some in the 'interpretive' field call a 'working hypothesis', which at its simplest is a hunch or idea that something is important to observe. (Lundberg, 1942, p. 9; Geer, 1969, pp. 152-3; Popper, 1963/9, p. 45).

Comparison as used by Comte and also later by Durkheim meant some historical reference and cross-comparison in and between 'societies', as well as classifying phenomena, whereas for the 'interpretive' approach it tends to refer more to comparison of observed categories and their properties. There is still some reference to historical methods, however. Also Weber, who contributed one strand to the 'interpretive' approach, was very much a comparer of historical situations.

Induction very broadly means deriving from empirical observation of particular events some generalisation of future probabilities. It can take two forms. In one, the statement that this swan is white and this swan is white, etc., leads to "it is probable that all swans are white". In the other, there is a statement that this phenomenon is found regularly where these conditions exist and not where they do not, so that where these conditions are found in future it is likely that the phenomena will be also. (Robinson, 1969, pp. 197-201).

Induction is seen as opposed to deduction, which is said to argue from general laws to an explanation of observed particular events, as exemplified by Hempel's explanation of why mercury in a thermometer first falls then rises when placed in hot water, which refers to 'laws' about the thermal properties of glass and mercury and the conductivity of glass, which are not themselves being observed at the time. (Keat and Urry, 1975, pp. 9-10). These ideas of induction and deduction are referred to again and also the idea that they may be less oppositional than different aspects of the same process of explanation, the 'up and down staircase' of Whewell. (Medawar, 1967, p. 134).

Having briefly considered these methodological elements of Comte's positivism the theme of its development is returned to.

Durkheim in one sense advanced the ideas of Comte in relation to 'positivism' although rejecting the latter's view of history and not being impressed with the 'Three Stages' of thought. His view was that in some respects Comte was metaphysical, so he himself preferred the term "rationalist" to 'positivist'. (Lukes, 1975, p. 72). Giddens also noted the term "naturalism". (Giddens, 1979, p. 244). Yet he agreed with Comte on the subject matter of sociology and on the method of study. Durkheim's concern was to place sociology on a sound footing by giving its methods a firm form. Yet Durkheim did not follow in Comte's footsteps. In the view of Lukes, the latter's influence was: "... very much a formative rather than a continuing one". (Lukes, 1975, p. 68). Durkheim developed his own ideas, and moreover Comte was not his only source. Like Comte, he drew upon a number in the course of developing his concepts of society and sociology, both in France and abroad. ((Lukes, 1975, pp. 54-63, pp. 79-83; Tyriakin, 1979, pp. 202-213). Lukes stated that according to Durkheim himself the most significant of these influences were Comte, Spencer and Espinas, the latter a professor and Dean, later Durkheim's colleague at the University of Bordeaux. (Lukes, 1975, p. 79). Tyriakin considered that two major influences on Durkheim's views on ethics and morality were Saint Simon and Kant, particularly the latter, and that Kant also influenced his views on the development of 'consciousness'. (Tyriakin, 1979, pp. 208-213). Like Comte, Durkheim lived through a period of political upheaval and sharp political divisions following the 1870 defeat of France by Germany. (Zeldin, 1977; Lukes, 1975). Like Comte, therefore, he was concerned with the nature of 'society' and the social order which made it possible.

Durkheim's view of the nature of 'society' was that it was more than a collection of individuals, than the 'sum of its parts'. Individuals and 'society' were "inseparable". While not all of an "individual" could be

explained by the "social", neither could the "social" be explained by the "individual". The 'social' had to be explained by the social. (Lukes, 1975, p. 638). In developing his idea of 'society', Durkheim was partly reacting against Spencer's idea of it as a biological organism, following Darwin's theory of evolution, though he drew on the analogy to some extent in developing the concept of 'function'.

Much of Durkheim's work was concerned, through comparisons between earlier forms of society and industrial forms, with an analysis of those features which bound individuals together and encouraged the growth of a common 'morale' or 'morality', a collective sense which made social order possible.

In the Division of Labour, Durkheim used the concept of society as an organic whole. 'Primitive' societies were postulated as held together by 'mechanical solidarity' or their similarities, being supposedly undifferentiated by separate 'functions'. Industrial societies, by 'comparison', were seen as linked through their differences, created by the division of labour, and thus by 'organic solidarity', because these differences led to mutual need out of self-interest, and this mutual need helped to engender a 'collective consciousness'. (Durkheim, 1933). It was this 'collective sense' that made 'society' more than individuals. Durkheim wrote that while:

" 'It is very true that society comprises no active forces other than those of individuals', but individuals as they join together form a psychological entity of a new species"

(Durkheim, 1982, p. 251)

Lukes stated that Durkheim moved on from early views on 'collective consciousness' and came to:

"... stress what he saw as the crucial role of collective beliefs and sentiments and especially of morality and religion in all societies."

(Lukes, 1975, p. 5)

From a comparative study of 'primitive religion' based on studies by

others of aboriginal religious forms in Australia, Durkheim conceptualised religion as the source of "the ties that bind" individuals to the group. (Tykriakin, 1979, p. 219). Religion was seen as the basis of 'moral' behaviour but not because of the explanation offered by religion itself, the actual existence of a deity, but because around the 'totems of the tribe' there developed rituals around the rites which were symbolic representatives of the 'sacred'. These rites, bringing people together, gave them the idea of being members of something outside themselves to which they owed devotion and obligation, and hence the idea of moral rules. For Durkheim, the 'reality' behind the 'sacred' to which this idea referred was not God but 'society'. The rituals had a unifying effect, creating and sustaining a sense of 'community'. Thus, rites were: "... above all, means by which the social group reaffirms itself periodically". (Durkheim, 1915, p. 553). This reaffirmation included ideas of morality, or obligatory behaviour. Thus, in Lukes' words, religion was seen as:

"... performing social functions ... as a system of communication of ideas and sentiments, and as a means of specifying and regulating social relationships."

(Lukes, 1975, p. 471)

Such regulations, based on moral rules, held individuals together and thus made for social order. Tyriakin observed that:

"... at the heart of the notion of social order is a moral or normative ordering of interpersonal conduct."

(Tyriakin, 1979, p. 217)

In "Suicide", which can be seen as a study of 'social illness', or a breakdown of such 'normative ordering', Durkheim recognised that in modern industrial societies, religion performed less of this unifying factor, not only because of the rise of secular beliefs and an anti-religious element, but also because there were different religions, which varied as to the degree to which they penetrated the lives of their adherents. Also he argued that those religions which did this most successfully, so reducing "the inclination to suicide", only did so because they prevented freedom of thought. Durkheim stated that:

"This seizure of possession of human intelligence is difficult at present and will become more and more so. It offends our dearest sentiments. We increasingly refuse to admit that limits may be set to reason"

(p. 375)

The progress of reason was an "irresistible" current. Therefore, unless societies "crumble", returning to their "starting point";

"Religions will no longer be able to exert very deep or wide sway on consciences."

(p. 375)

In his view:

"... religions can socialise us only in so far as they refuse us the right of free examination. They no longer have, and probably never again will have, enough authority to wring such a sacrifice from us."

(Durkheim, 1970, pp. 375-6)

Thus, they could not be the main source of morality. This last was perhaps the central concern for Durkheim in relation to 'society', and one on which his ideas were being developed throughout his career. Lukes stated that "by the end of the Bordeaux period", that is, by 1902, Durkheim had a clear concept of morality as having "three elements". These were "the spirit of discipline" which referred to the obligatory nature of moral rules. Second was "attachment to social groups" or "the collective ideal" and the third was "autonomy" which meant that in order to act morally it was necessary to: "... have as clear and complete an awareness as possible of the reasons for our conduct". (Durkheim, in Lukes, 1975, p. 115). Durkheim considered that it was necessary to 'socialise' the younger generation into the 'moral' or 'normative' ways of behaving, to attach them to the social group. This was the function of education, which he stated was: "... a methodical socialisation of the young generation". (Durkheim, 1956, p. 71). The earlier translation spoke of "systematic" socialisation. (Lukes, 1975, p. 111). (During the research the need for 'socialisation' for school was referred to by some of the teachers, particularly at Moorland, where families were seen as inadequate in preparing children for this. The views expressed seemed very 'Durkheimian').

Durkheim defined education as:

"... the influence exerted by adult generations on those that are not yet ready for social life. Its object is to arouse and to develop in the child a certain number of physical, intellectual and moral states which are demanded of him by both the political society as a whole and the special milieu for which he is specifically destined."

(Durkheim, 1956, p. 71)

He also regarded education as a "characteristic experience". He said that:

"... it is patently obvious that all education consists of a continued effort to impose on the child ways of seeing, thinking and acting which he himself would not have arrived at spontaneously."

(Durkheim, 1982, p. 53)

The family was seen by Durkheim as the earliest source of this education. In comparison with pre-industrial families, however, the modern 'conjugal' family gave a reduced sense of "common life". It had also lost many of its former functions. In "Suicide" Durkheim also observed that this smaller unit was also a source of "an aggravating influence" which could lead to homicide, even though its loss through divorce, and the consequent lack of its 'normative influence' could lead to suicide. He noted that divorce, where possible, was becoming more frequent because, in part, marriage did not reward both parties equally. (Durkheim, 1970, p. 202, 260, 377 and 384-5). However, in spite of the loss of many of its former functions, the family still retained that of being.

"... an important centre of morality, a basis for moral education, a centre of moral security and a source of attachment and regulation for the individual."

(Lukes, 1975, p. 185)

However, because of its reduced functions, there was a need for more formal organisation of education, hence the development and importance of the school. This had to:

"... serve as an 'intermediary between the affective morality of the family and the more severe morality of civil life'" and "the schoolmaster was the secular successor to the priest."

(Lukes, 1975, pp. 115-6)

But apart from 'morality', intellectual development was also the function of education, which should be secular. Durkheim considered that there was a pattern of development in stages. This meant that in "the 'childhood' stage", the teacher should know:

"'...at each moment of this period, what precisely are the needs which correspond to it, what are the child's powers, and the exact degree and true extent of his faculties'. The 'first law of pedagogy is to adapt with maximum precision the education one gives to the child as he develops'."

(Durkheim, in Lukes, 1975, p. 123)

This was an interesting statement for the present research because of the concept of child development in the schools visited, and because of the association of this concept with 'progressive' education.

So far, it has been shown that at the ontological level Durkheim conceived of 'society' as in some sense an organic entity, with different institutions such as religion, education and the family as having 'functions', concerned with the development of a sense of collectivity and the normative regulation of behaviour. Because he stressed that society was more than the individual, a different entity, he has been regarded as reifying it, and of supporting a conservative view. However, Lukes noted that in some respects he tended towards being a "radical social reformer" as well as being also a "moralistic conservative" in his concern for social order and the need for "limits" to individual behaviour and for "moral discipline". (Lukes, 1975, p. 546), Durkheim's view of the relationship between an individual and 'society' shaped his epistemological views. He considered that the principles of classification, including the categories of time, space, class, number and so on were neither derived from individual experience, as Hume considered, nor were they 'a priori', outside nature, which was Kant's view. Durkheim saw them as in a sense 'a priori', but only in the sense that for any individual such conceptual thinking and classification came to him from outside, from 'society', because they were 'collective representations', through which minds could communicate. Durkheim also claimed that the "social order" and "the conceptual order" were

causally linked, that specific classifications were caused by a specific form of society, that such classifications were "structurally similar" to specific forms of society. He also held that the classifications and categories were thus 'functional' for any given society.

He also considered that forms of primitive classification were cosmologies or attempts to explain the universe, attempting to make understandable the relationships between things, and so to unify knowledge amongst groups. From this, Durkheim developed the view that primitive religions were the source of the fundamental ideas of science, that the underlying logic was the same, a view which Lukes described as both "challenging and fruitful". (Lukes, 1975, pp. 435-445).

Durkheim's view of the sociology of knowledge has perhaps links with phenomenology, one of the strands of the 'interpretive' approach. This was Tyriakin's view. He stated that Durkheim's treatment of the idea that classification was rooted in social organisation was:

"... a major innovation and anticipation of phenomenological sociology, for if the modes of organising and classifying objective reality are collective representations, then it follows that those a priori structures (similar to Husserl's noemata) are constitutive of the social world we seek to understand. To understand the structure of a collectivity's classification of the world is, in effect, to understand its rules or principles of social organisation."

(Tyriakin, 1979, p. 212)

He added that this could be considered as one of Durkheim's most important "insights and discoveries". (p. 212).

Lukes also regarded the theoretical ideas as important, and that the idea that there were: "... structural correspondences between symbolic classification and social organisation" (p. 460) had been fruitful. However, he also argued that the empirical evidence on which the ideas were based was not wholly accurate. No correspondence had actually been established between a particular form of society, and a particular society. The evidence had not been "rigorously" tested by "concomitant variation",

(one of Durkheim's 'Rules'), and he thought that it was a wrong assumption that at any one stage a particular society only had one form of classification and set of categories. Also, Durkheim and Mauss took for granted alleged "evolutionary sequences", from totems to clans to regions, for which evidence was lacking. Also, they had not considered either the different ways that totems might be attached to groups, nor different groupings, nor any other "sociological explanation" of classification. Also, in Lukes' view, Durkheim went "too far" in assuming that 'society' could account for all "operations of the mind and the laws of logic", (p. 447) because he did not distinguish between the contents of the categories and the existence of the ability to think spatially and temporally and so on. Lukes argued that it would be impossible to hypothesise a situation where people did not think in these forms, because it was the definition of thinking. (Lukes, 1975, pp. 445-449).

Another epistemological consideration for Durkheim was the kind of philosophical question social science could deal with. He considered that there were:

"... meaningful philosophical questions (in particular those of epistemology and ethics [which] were answerable and the conclusions of science were relevant to the answers"

but there were:

"... those which could not be answered"

(those which looked beyond experience) and which:

"... were not meaningful, or at least not important."

(Lukes, 1975, p. 107)

In terms of methods, Durkheim, as stated, wished to give sociology a firmer base. In the "Rules" he attempted to do this. His first aim was to clearly identify the 'subject matter' of sociology. To this end, he stated that what he termed "social facts" should be treated as things.

By a "social fact" he meant:

"... any way of acting, whether fixed or not, capable of exerting over the individual an external constraint, or,

which is general over the whole of a given society, whilst having an existence of its own, independent of its individual manifestations."

(Durkheim, 1982, p. 59)

Such 'facts' were "ways of functioning" which were "physiological", or "ways of being", which were "anatomical" or "morphological", which were collective. (p. 57). Thus, 'social facts' could include communication systems, demographic patterns, birth or suicide rates, laws, customs, and , eventually, as Lukes pointed out, 'collective representations' or ideas and beliefs. (Lukes, 1975, p. 230). Lukes was critical of Durkheim's view of social facts on two counts. He argued first that their supposed "characteristics" of "exteriority" and "constraint" were "highly ambiguous". Secondly, he claimed that Durkheim had not clearly identified those facts which should have "explanatory priority". (Lukes, 1975, pp. 228-9; cf Lukes in Durkheim, 1982, pp. 4-6).

Durkheim also viewed 'social facts' as either "normal" or "pathological", thus again indicating his concern for 'social health' or order. "Normal" facts were those which were in some way functional, and were defined as those which:

"... for a given social type, viewed at a given phase in its development ... occurs in the average society of that species, considered at the corresponding phase of its evolution."

(p. 97)

Thus, at a particular stage, even certain levels of crime could be "normal".

In Durkheim's view, once 'social facts' had been identified, it was necessary to study them 'objectively'. To observe them thus a sociologist should first: "... systematically discard all pre-conceptions". (p. 72). Then the facts to be observed had to be exactly defined. For Durkheim,

"... the definition, clearly must express the phenomena as a function, not of an idea of the mind, but of their inherent properties."

(p. 75)

Subjective impressions, he considered, had to be set aside, so that:

"The subject matter of research must only include a group of phenomena defined beforehand by certain common external characteristics, and all phenomena which correspond to this definition must be so included."

(p. 75)

The idea that social phenomena could be defined by external features substantiates Lukes' statement that Durkheim followed Descartes in believing that there was a "reality" which had an independent existence, apart from thought. (Lukes in Durkheim, 1982, p. 11). Lukes was critical of this "absolute conception of knowledge" and the "pursuit of objectivity" it required. His argument was that it was "unrealistic" to seek to explain the world without reference to subjective meanings because:

"... such intersubjective meanings are essential to the very identification of social facts."

(p. 12)

Lukes therefore considered that Durkheim's pursuit of 'objectivity' was the most mistaken aspect of his view of sociology and method (and it is certainly the heart of the 'interpretive' critique of 'positivism'). Lukes pointed out that Durkheim, in rejecting a concern with subjective interpretations, either of other individuals or his own, avoided having to concern himself with the problem of relativity, whether a 'real' explanation is possible. Lukes also noted that Durkheim never considered how to discover the interpretations of others, and that this was "odd", given his own later concern with 'representations'. (p. 15).

These could have been considered in terms of social psychology, or phenomenology, as Tyriakin observed. But Durkheim was concerned to establish sociology as a discipline distinct from psychology and so set himself against explanation at the individual level.

Hence, as Lukes argued, he did not bring:

"... his own actual interpretive practices to the level of self-conscious reflection."

(p. 15)

Consequently, his work lacked "an explicit micro-theory". His methodological position prevented him from considering:

"... the problems of interpretation and thus to incorporate hermeneutic inquiry into the rules of sociological method."

(p. 15)

This, in Lukes' view, had "limiting and disturbing effects" on Durkheim's practice, so that he was "insufficiently critical" of his own interpretations. (p. 15).

Apart from defining 'social facts' and stating the need for 'objectivity', Durkheim held as one of his 'Rules' that 'classification' of societies was necessary, in order to distinguish what was 'normal' for a given society. His idea was that this would be on the basis of their stage of evolution, for Durkheim held that social species evolved as did biological ones, following Spencer. (p. 112). Durkheim considered that any society was just a matter of: "... different combinations of one and the same original society". (p. 116), differing according to the degree of organisation. Accordingly, societies would be classified in terms of their manifested organisation:

"... taking as a base the perfectly simple society or the single segment society. Within these classes different varieties will be distinguished, according to whether a complete coalescence of the initial segments takes place."

(Durkheim, 1982, p. 115)

Hence, in classification, Durkheim was using a form of historical method. This was also the case in his Rule that the 'comparative method' must be used. This was seen as a form of "indirect experiment", like Comte. It was seen as necessary, in order to establish causal relationships between phenomena, to compare:

"... the cases where they are both simultaneously present or absent, so as to discover whether the variations they display in these different combinations of circumstance provide evidence that one depends on the other."

(p. 147)

This method of "concomitant variation", seeing where phenomena regularly

varied together, was for Durkheim: "... the supreme instrument for sociological research". It was unnecessary, where it was used, to make numerous observations. (p. 153). However, it had to be practiced "with rigour".

What had to be compared was not:

"... isolated variations, but series of variations, systematically constituted, whose terms are correlated with each other in as continuous a gradation as possible and which moreover cover an adequate range."

(p. 155)

Such a range was necessary, in Durkheim's view, in order to extend comparison by showing how phenomena evolved, in relation to conditions, for:

"... one cannot explain a social fact of any complexity save on condition that one follows its entire development throughout all social species."

(p. 157)

In terms of causal explanations, Durkheim absolutely rejected the idea of "the plurality of causes". He held that in order to use:

"... the comparative method scientifically, i.e., in conformity with the principle of causality as it arises in science itself we shall have to take as the basis of comparisons established the following proposition: To the same effect there always corresponds the same cause."

(p. 150)

However, in using concomitant variation to try and establish causal relationships, there is the problem, noted by Lofland, among others, that because variables regularly occur together, this does not necessarily prove that one is the cause of the other. As Lofland stated:

"Some other unknown factor may be the cause, or among the causes. Some known, but unmeasured factor may be the cause, or among the causes."

(Lofland, 1971, p. 60)

Durkheim himself recognised that sometimes concomitance could be found:

"... not because one of the phenomena is the cause of the other, but because they are both effects of the same cause, or indeed because there exists between them a third phenomena, interposed but unnoticed, which is the effect of the first phenomenon and the cause of the second."

(p. 152)

Hence, results might need to be "interpreted", and new comparisons made.

However, he believed that with this further comparison, it was possible to find one cause for one effect. Thus, if there was more than one cause for suicide, for example, it was because there were different forms of suicide. Lukes observed that, in arguing thus, Durkheim was begging the question of the causes of suicide by beginning with a causal theory which he "assumed to be true", leading him to restrict consideration of other possible explanations of the data. (Lukes, 1975, p. 202).

Becker, who discussed the use of concomitant variation in causal propositions in "participant observation" (associated with 'interpretive' approaches), stated that it was important to try and eliminate rival hypotheses, and the larger the proportion of these that could be discredited the greater the validity of the proposition. Nevertheless he also stated that:

"It will seldom be possible in participant observation, or in any other social research, to muster strong evidence discounting all possible rival hypotheses." -

(Becker, 1969, p. 257)

For Durkheim to insist on single causes, and on his ability to exclude alternative explanations seemed another weakness in his Rules, like his insistence on the exclusion of all subjective interpretations. Nevertheless, his stress on the importance of classification, comparison and the use of concomitant variation in sociological research seems as relevant for the 'interpretive' approach as for 'positivism', since the concepts are used in that approach, although what is classified and compared is rather different, nor is there an insistence on universal explanations.

A final point about Durkheim's "Rules" was his view that analysis of a 'social fact' had to be in terms of functions as well as causes. The latter should be investigated first, but then it was necessary to determine, for a full explanation:

"... whether there is a correspondence between the fact being considered and the general needs of the social organisation, and in what this correspondence consists ...".

Intentionality was not important for this. Such questions were "too subjective". (Durkheim, 1982, p. 123).

As noted, the idea of social phenomena such as 'education' or religion having 'functions' for society was an important feature of Durkheim's views. It was picked up later, especially in social anthropology, as noted below.

In terms of reasoning to arrive at explanations, Durkheim is stated by Giddens to use "a Baconian version of scientific method" even more than Comte, that is, induction. Giddens stated that it was Durkheim's view that science:

"... including sociology, advances only slowly and cautiously, through patient inductive generalisation based on observed regularities in social facts."

(Giddens, 1979, p. 244)

However, he did not give a reference for this statement. Denzin, on the other hand, stated that Durkheim's theory of suicide conformed with the concept of theory as:

"... a set of propositions that furnish an explanation by means of a deductive system."

(Denzin, 1978, p. 47)

He considered that it did so because of Durkheim's proposition that the suicide rate varied with the degree of individualism in a society, and that the latter varied with the degree of Protestantism, leading to the deduction, as Homans saw it, that the suicide rate varied with the degree of Protestantism.

But Durkheim stated in "Suicide" that he developed the categories of types of suicide by looking at various factors which might have influenced what he saw as rises in the basic suicide rate, such as age, sex, race, and finally the "... states of the various social environments". (p. 151). That is, he used a form of observation, using statistics, to develop his categories, by a comparison with such environments, before making the

generalisations. This seems a form of induction, and Durkheim, in the preface, discussing the hope of discovering "real laws", added that errors would have been made, and that: "... we ... must have over extended the facts observed in our inductions". (Durkheim, 1970, p. 37). Elsewhere, in the "Rules", Durkheim mentioned both deduction and induction. For example, discussing causality, he argued that this principle was:

"For the scientist", not problematic; it is assumed by the very method of science. How can one otherwise explain both the role of deduction, so important in experimental reasoning ..."

(Durkheim, 1982, p. 149)

In one very 'clear' sentence later, he wrote, also referring to causality:

"First we shall discover with the help of deduction how one of the two terms was capable of producing the other, then we shall attempt to verify the result of this induction."

(p. 152)

On the following page, writing of the use of 'concomitant variation', he stated that the sociologist: "... must, take as the chief material for his inductions ..." (p. 153). Elsewhere, writing of the need to compare 'series of variations' he stated that: "... variations of a phenomena only allow a law to be induced ..." (p. 155). Finally, he argued that:

"All that "(sociology)" asks to be granted it, is that the principle of causality should be applicable to social phenomena ... as an empirical postulate, the product of a legitimate induction."

(Durkheim, 1982, p. 159)

Thus, it seems clear that Durkheim did use inductive logic in deriving his generalisations from 'observations', but it is not certain that he denied the role of deduction. This seemed interesting in view of one aspect of twentieth century debates on one form of 'positivism' discussed later.

To sum up, Durkheim sought to define sociology as the study of the social, not the individual, being, and to give it a firm method. This was to be 'objective', as he considered science to be, and starting from the empirical basis of sense perceptions of "the external nature of things".

("Rules", p. 81). It was to use classification and comparison, as Comte had advocated, with the use of 'concomitant variation', to arrive at inductive generalisation, or 'real' explanations, though perhaps also making deductions. He thus made sociology more 'positive' or 'scientific', as Comte had tried to do, though himself eschewing the actual name 'positivist' for 'rationalist', or perhaps even 'realist' as well, according to Lukes, or perhaps 'naturalist'. (Lukes, 1982, p. 11). Both these latter terms occur again in the 20th century debates.

Before going on to these debates, a word on Marx seems in order. Griffin noted that Marx was not a positivist. (Griffin, 1985). However, in certain respects he might be considered as 'positivistic'. Like Durkheim, and to some extent Comte, Marx believed in historical laws of development, although in his case these were to do with changes in the economic base of 'society', on which, in his view, everything also depended. In his concept of dialectical materialism, developed from Hegel, he put forward the view that each historical stage had its own form of production and the relationships associated with it, such as feudalism and later capitalism. Through thesis, anti-thesis and synthesis, each stage led (or evolved) into the other and so, he predicted, capitalism would give way to socialism. Each of the earlier stages was seen as characterised by forms of class struggle. But the point, for Marx, in trying to understand 'historical' laws and so 'society', was to predict the inevitability of change in the latter.

Durkheim was critical of Marx's concept of 'historical materialism' and the associated view that economic factors were the source of social institutions, since he thought that these, like others, had developed out of religion rather than the other way round. (Durkheim, 1982).

Like Durkheim, Marx thus took a 'holistic' view of society. He also considered that social institutions had functions, and were in some sense external to individuals. He also used the concept of 'consciousness'

as something which was social, not individual, but in Marx's view this was 'false' rather than simply 'collective', or shared in terms of morality. It was seen as the consequence of ideological oppression, which was itself the consequence of the economic oppression of one part of 'society' by a ruling class. For Marx all social institutions, including the educational system, were part of the 'superstructure' of society, raised on the economic base. Each served the interests of the ruling group, and thus reflected the class struggle, or the potential for this.

Like Durkheim, Marx was interested in the concept of social order and how this was achieved. In contrast to views later developed by Durkheim, however, he saw social order not as functional for society, *maintaining it* in the interests of all, but rather as functional for the ruling group, maintained through 'false consciousness' or ideology, and if necessary by coercion. Hence, conflict between groups was likely, and social change possible, if this 'false consciousness' of the oppressed group would be changed to an awareness of their real position. Hence conflict, not consensus, was Marx's concern in relation to social order.

Thus, although Marx might be classed as 'positivistic' in certain respects, in seeing consensus and order as oppression he differed strongly from the views of both Comte and Durkheim.

Thus 'positivism' in the nineteenth century and in the earlier part of the twentieth generally means the ideas first expressed as a theory by Comte, and developed into a modern and firmer form by Durkheim. The latter in particular seemed to give it some of the characteristics most criticised by opponents, such as its 'objectivity' and a particular view of science.

The twentieth century, after Durkheim, was characterised in terms of 'positivism' by several features. One was the influence of Durkheim on American sociology (and later British) in particular in the work of

Talcott Parsons and others. This showed a continuation and adaptation of the ontological views of Durkheim. There was also, as noted, a development of positivism which was mainly epistemological, although having methodological aspects. This was 'logical positivism', or the second stage. This led to a debate within the philosophy of social science, and science about what counted as 'scientific' knowledge, which had two aspects. One was concerned with the merits of inductive or deductive reasoning. The other concerned the idea of science as hard 'objective' and 'value free'.

Somewhat apart from these, although obviously connected with the ontological and epistemological aspects of 'positivism', was the increasing equation of 'positivism' with 'quantitative' methods. This is itself a matter of debate.

Because of the range of ideas, and because it is no longer one person being considered, it is difficult to deal with all these aspects together, even though they are connected. Therefore, they are to some extent separated below. Consequently the 'ontological' continuation is discussed first, then the 'epistemological', and the ensuing philosophic debates, and then the question of the use of 'quantitative' methods is part of 'positivism', and what exactly these methods were thought to be, in terms of practical techniques of data collection and analysis, in the twentieth century.

Keat and Urry noted the influence of Durkheim from the 1930's in American sociology in the work of Talcott Parsons and then Merton. They stated that:

"Parsons is concerned to establish universal laws of social life which will explain any particular empirical facts"

and also noted his concern with "empirical verification". (Keats and Urry, 1985, p. 91).

Parsons was 'functionalist' in a slightly different sense to Durkheim, however. He considered that sociology was the study of social relations.

These were not directly observable, but were complex theoretical constructs which enabled human behaviour to be predicted and explained. But social relations pre-supposed a more fundamental construct of a hypothetical 'actor', who had 'goals', and gave his world meaning in terms of these, and who had expectations of the behaviour of others. In this Parsons differed from Durkheim. He took the idea of a 'social actor' from Weber, who is briefly discussed in the next sub-section, as part of the 'interpretive' approach. However, Parsons made a different use of Weber's idea. He used the term 'action frame of reference', against the Durkheimian tradition. This 'action frame' explained action by considering the place it had in the orientation to action, the purpose and meanings, of a hypothetical actor. But Parsons, after saying that he was using this frame, called himself a functionalist, though not using the organic analogy of Durkheim but rather that of a 'system'. 'The' social system was society, but this was seen as having sub-systems, which could themselves be considered as systems. As Parsons used it, the notion of a system seemed to have the essential features of an organic model, since similar types of explanation to those of Durkheim were used. Given this central concept of a system, something was explained if it was shown to be system maintaining. Parsons listed four of what he termed 'functional imperatives' for the maintenance of a system or sub-system. These were; adaptation to the external environment, a goal and the idea of its attainment, and the allocation of resources to attain external goals, integration, the idea of harmony and co-ordination of internal units, and latency, (pattern maintenance and tension management) maintaining internal patterns of shared values, but 'latent', that is, not immediately apparent. Each of these 'imperatives' or problems would be dealt with by 'the' system's sub-systems. Thus, the educational sub-system could, by allocating individuals to particular positions, help with adaptation and also integration, by its 'socialising' function. (Parsons, T., 1957,). This seemed a very Durkheimian view of education.

Merton developed Parsons' theory by making a distinction between 'manifest' and 'latent' functions. The former were the intended and recognised consequences of actions, while the latter were the opposite. He also developed the concept of 'dysfunction' to indicate that some consequences of social action could be 'functional' or system maintaining for one system but not so for another.

Both Parsons and Merton were major figures in American sociology, and the 'functionalist' model was the dominant paradigm there, although not the only one. Keat and Urry summarised sociology in America as "largely positivist" from the 1930's to the 1960's. (Keat and Urry, 1985, p. 90).

Bernstein similarly noted that in England during the 50's the "... major theoretical approach was that of structural-functionalism". (Bernstein, 1975, p. 151). However, Rex noted also the other British tradition, the 'Fabian' or: "... book-keeping of social reform". (Rex, 1978, p. 295). Eggleston argued that in order for sociological research to become established in Britain it had needed academic recognition. He stated that the existence of a 'pure science' model in psychology had helped this discipline to achieve such recognition, and so sociology followed the same path. He also pointed out that the standing of social anthropology helped sociology to avoid "... some of the traumas" that psychology had had to go through in gaining status. (Eggleston, 1975, p. 2).

Floud and Halsey similarly pointed to the influence of social anthropology in the sociology of education, and they stated that:

"... 'structural-functionalism' is par excellence the anthropological approach to social analysis."

(Floud and Halsey, 1958, p. 171)

Social anthropologists such as Malinowski and Radcliffe-Brown were very much influenced by Durkheim. Both, but particularly the latter, had used the functionalism of Durkheim and added to it the notion of structure, and hence 'structural-functionalism'. This was the main form of analysis

in social anthropology, but Fletcher pointed out that even so it was only one component of this discipline. (Fletcher, 1972, p. 250). This is why it is also discussed later in the 'interpretive' sub-section.

Thus, in both England and America, until the 1970's, the 'positivist' tradition of Comte and Durkheim, adapted to 'functionalism' or 'structural functionalism' was the principal model of society for sociology.

It was noted in the Review of the literature that much of the work on schools had been 'structural-functionalist' before the 70's. It had also been pointed out that Floud and Halsey had argued that attention needed to be paid to the internal processes of the schools.

Floud and Halsey also pointed out the limitations of structural-functionalist notions of consensus and integration, and equilibrium for industrialised societies, for these:

"... are dominated by social change, and "consensus" and "integration" can be only very loosely conceived in regard to them."

(Floud and Halsey, 1958, p. 171)

Banks stated that what she termed the " ... traditionalist sociology of education" which, like one form of mainstream sociology was then being criticised as "... macro, structural-functionalist and determinist" (p. 4) had been:

"... very much concerned with the relationship between the educational system, the economy and the division of labour."

(p. 6)

These might be thought of as akin to Durkheim's interests, but Banks noted Floud and Halsey's concern:

"... with the increasing subordination of the educational system to the economy."

(p. 6)

That is, it was a critical look, not a study in the maintenance of norms.

Banks pointed to her own work (1955), which:

"... sought to demonstrate the dependent relationship of secondary education to social stratification and the hierarchy of occupations."

(Banks, 1974, pp. 4-6)

It was meant to show these links, not suggest that they were beneficial for the maintenance of 'society'.

So even if in Britain and America the major and 'traditional' model was structural-functionalism and thus 'positivistic', perhaps for status reasons until the late 60's, it did not necessarily follow that all those using it shared exactly the same concerns as Comte and Durkheim in social order. Some may not have been uncritical of 'social institutions', nor have been unconcerned with issues later taken up by the 'new' sociology of education. That was quite apart from the Marxist view of conflict, which would very well have used structural functionalism as an approach, with the analysis serving to show that institutions functioned to maintain a dominant group.

However, 'positivism' and thus structural functionalism, were being increasingly criticised within sociology as 'new' ideas came to the fore in America and Britain in the 70's. As shown in the next sub-section, these 'new ideas', like 'positivism', had a long history. 'Positivism' had, in fact, been criticised almost from the beginning within the philosophy of social science. It was said earlier that positivism in the twentieth century had, besides ontological aspects, epistemological concerns. These were said to mark the "second phase" according to Giddens. (Giddens, 1979, p. 237). Outhwaite regarded it as a "second variant". (Outhwaite, 1987b, p. 6). This second stage was said to be 'logical positivism' as developed in the philosophy of science by members of the 'Vienna Circle'. This was said to be a group which begin in 1907: "... around Frank, a physicist, Neurath, an economist, and Hahn, a mathematician", although he added the names of Carnap, Godel and Feigl. (Giddens, 1979, pp. 248-251). Hamlyn mentioned Schlick, Waismann and Carnap, adding that there were "originally fourteen in all". He also stated that Reichenbach and Hempel were supporters in Berlin of the same ideas. (Hamlyn, 1987, p. 306). Outhwaite also mentioned Nagel as a member, and Popper. (Outhwaite, 1987b, p. 6). The latter

was, however, not a supporter in many respects. The form of 'positivism' developed by the Vienna Circle originally was said to be 'scientific', 'naturalistic' and empiricist, and to have incorporated 'mathematical logic'. (Bryant, 1985, p. 54). On the roots of their ideas, Giddens stated that there was a clear link between these and Comte, and that the "principal mediator" between the two was Mach, the "physicist and physiologist". Giddens argued that certain elements of Comte's thought were found in Mach's writings, though not in the same form. He summed up these elements as first "... the reconstruction of history as the realisation of the positive spirit", that is, the development from religion through metaphysics to positivism. Thus, the second point was "... the final dissolution of metaphysics". The third point was a clear distinction between "... the factual, or the "observable" and the imaginative or "fictitious"", which Giddens saw as indicating that Comte " ... adopts the standpoint of empiricism". The fourth point was "The "relativism" of scientific knowledge".. This did not have, Giddens stated, the modern meaning of 'multiple realities', but referred to the argument:

"... that science confines itself to explaining the interdependence of phenomena, it does not claim to discover essences or final causes."

(Giddens, 1979, pp. 265-6)

Giddens argued that, as noted, these Comtean ideas were present in Mach's work. Chiefly, Mach was said to believe in the 'triumph' of the "scientific outlook" over metaphysics, and, like Comte, in an end to philosophy, except as "... the logical clarification of the bases of science". (p. 247). Giddens stated further that:

"For Mach ... the object of science is to discover relations between phenomena ... this carries the implication that theory has a purely heuristic role in scientific investigations. The precise identification of the mathematical functions that express the dependencies between phenomena in nature renders theory obsolete."

(p. 247)

As noted, 'logical positivists', at least in the early stages, stressed 'mathematical logic'.

Giddens also claimed that Mach stated directly what was "taken for granted" or not made explicit, in Comte's work: "... that no place is found for the reflexive subject". (p. 267). Mach was said to take a "quite unequivocal" stand on this, that:

"... the self or ego does not exist as a unity, it is merely an aggregate of sensations."

(p. 247)

hence there was no 'I' to be concerned with. Thus the "... moral welfare of mankind as a whole" could be emphasised, which:

"... linked back to Mach's conception of the relation between science and human progress."

(p. 248)

This was Comte's view of science also.

In Giddens' view it was the work of Mach which assisted in the growth of a "climate of opinion" which made 'logical positivism' possible. However, he noted that there were other sources for their ideas, and also that in some respects their work was at odds with Mach's. Giddens and also Hamlyn noted the influence on the Vienna Circle at its foundation of the ideas of analytic philosophy, based on the Kantian distinction between synthetic and analytic thought. Both noted the work of Moore and Russell, and Hamlyn cited Frege, in the development of this and thus in the Vienna Circle. Hamlyn also noted the mathematical logic of Frege. Both writers also stated that Wittgenstein, in his early work on language, was a major influence on the logical positivists. (Giddens, 1979, pp. 248-9; Hamlyn, 1987, pp. 289-308). The ideas of analytic philosophy are too complex to even summarise adequately. Their influence on the logical positivists can be very broadly stated to be formative in their view of what counted as a meaningful statement in explanation. On this last, they held in the beginning that any proposition that could not in principle be tested and 'verified' (the verification principle) was meaningless. (Giddens, 1979, p. 249). This idea, like Comte's own view of scientific thought, was equally an attack on theological and meta-

physical thinking. In their view 'hermeneutics', or understanding was metaphysical. (Outhwaite, 1987b, p. 6). However, they saw Comte's view of history as itself 'metaphysical', and for this reason, preferred to call themselves 'logical empiricists'. This view of science was said to be 'empiricist' (and positivist) in that "knowledge" came from "experience", that which is given, and statements would be verified by reference to that experience. Bryant commented on their view of 'meaning' that logical positivists had seemed more interested in "what sociologists say" than to what 'verification' might entail in research design. (Bryant, 1985, pp. 113-4). The logical positivists also had views on the unity of science which differed from those of Comte. *Their view was that scientific laws were reducible. That is:*

"... the laws, or more generally the language of the 'higher' sciences in Comte's hierarchy could be 'reduced' to that of the lower ones. The propositions in sociology could ultimately be analysed down into those of physics or material object language."

(Outhwaite, 1987b, pp. 6-7)

This again shows their concern with logical analysis and mathematical logic. From this viewpoint, the aim of sociology was to establish:

"... regularities between spatio-temporal variables [and the] ultimate aim of unified science to connect together all logically compatible laws."

(Bryant, 1985, p. 58)

Both Giddens and Outhwaite stated that the original "dogmatic" views of some members of the Vienna Circle were modified in the course of their division due to the German invasion of Austria. Giddens noted that there were already differences between them before this, however, with:

"... Hahn, Neurath and Carnap, the so called "left wing" ... the main figures in the shift away ..."

(p. 251)

from the original line, whereas:

"Schlick and Waismann were more inclined to hold fast to their established views."

(p. 251)

Giddens further stated that logical positivism:

"... lost the clear-cut identity that it previously had and devolved into a more general stream of positivist philosophy finding ready contacts with, and having a great deal of influence upon, the traditions of empiricism and pragmatism ... in Anglo-Saxon philosophy."

(Giddens, 1979, p. 251)

Outhwaite also stated that 'logical empiricism' changed over time, modifying on the one hand into what he called the "standard view" and Giddens called the "orthodox" view of science, and on the other 'mutating' into "conventionalism". (Outhwaite, 1987b, p. 10; Giddens, 1979, p. 252).

The 'orthodox' model, or what Outhwaite also termed the 'covering law' model, associated with hypothesis deduced from general laws. It is this model that Outhwaite termed the "third variant" of positivism. (p. 6). Cuff and Payne also saw the 'hypothetico-deductive' model as the "portrayal" of scientific method which was predominant, and Worsley also associated it with a "positivistic" approach. (Cuff and Payne, 1979, p. 157; Worsley, 1977, p. 74). However, this is perhaps problematic.

At any event, the original ideas of logical positivism engendered a considerable debate within the philosophy of social science. It was argued that they were based upon a false model of science. These 'scientific' criticisms had two aspects. One was the criticism of Karl Popper, who attacked the idea of 'verification' and also the principle of inductive logic as used in earlier positivism and by some logical empiricists. Popper's own views on 'falsification' were in turn questioned. The second criticism of logical positivism was on the concept of a 'value-free' science as an activity, and of one model of science. Part of this was 'conventionalism'. The two criticisms were not entirely separate, but were linked through the 'problem of falsification', therefore although the discussion below starts with Popper's arguments, the other issue leads from it and is partly merged with it. The ideas are discussed because the underlying arguments seemed relevant for 'non-scientific' theories,

since they are concerned generally with what counts as 'knowledge' and how it is acquired.

Popper himself is linked with 'positivism' because of his advocacy of the 'hypothetico-deductive' model. In some respects Popper can be called a 'positivist' and in others not. He was not a logical positivist in any event, although sometimes included in the Vienna Circle. Hamlyn stated that Popper "... has always hotly repudiated the categorisation". (Hamlyn, 1987, p. 304). Giddens also stated that the work of Popper " ... precedes - and is in some part of their sources" of later critics of the 'orthodox' model such as Kuhn, Feyerabend and others. (Giddens, 1979, p. 259).

However, not all the members of the Vienna Circle were inductivist. The example of Hempel has been noted previously, from Keat and Urry (1975, pp. 9-11). Bryant made a similar point. (Bryant, 1985, p. 115). Outhwaite also listed Carnap and Nagel as proponents of the 'standard' model. (Outhwaite, 1987b, p. 6).

Popper actually called himself a "realist", as when he stated " ... I propose to accept realism as the only sensible hypothesis". (Popper, 1979, p. 42). 'Realist philosophy' was noted by Keat and Urry as an alternative to 'positivist'. (Keat and Urry, 1975, pp. 27-45). Outhwaite made the same point. (Outhwaite, 1987b, pp. 19-44). However, Lukes, as noted previously, saw Durkheim as in 'some sense a realist'. (Lukes, in Durkheim, 1982, p. 11).

What Popper meant by 'realism', or as he also called it, "essentialism", (which is given another meaning elsewhere and contrasted with realism), (Keat and Urry, 1975, p. 42) because he said that 'realism' had also been called 'idealism', although elsewhere he contrasted 'essentialism' with 'naturalism', was that he rejected 'nominalism'. (Philosophy is like this!) This particular philosophical debate contains two ideas. 'Nominalism' is used in relation to the term 'universals', of which Popper used 'white' as an example, which can be applied to a range of things, but which, according

to 'nominalism', have no separate existence of their own. From this what Kolakowski had stated was the second rule of 'positivism' was derived, that nominalism means that general statements must refer to concrete objects as experienced. (Kolakowski, 1972, p. 13). 'Realism', on the other hand, as used by Popper, is the view that physical objects exist independently of being observed, hence "whiteness" really 'exists', that "... universal terms are held to denote universal objects". (Popper, 1961, pp. 27-8). Popper stated that he did not think that "language without universals" would ever work. So in this sense Popper was not a positivist. Also, 'realism' was given as an opponent of 'naturalism'. (Keat and Urry, 1975). The significance of this opposition was that the debate about methodological unity between the natural and social sciences, one of the themes of 'positivism', had been conducted around two dichotomies, 'naturalism' versus 'anti-naturalism', and 'positivism' versus 'idealism'.

'Naturalism' has two meanings. One is the idea, using the natural sciences as a model, that there is:

"... one logic of science to which any intellectual activity aspiring to the name of science must conform."

(Keat and Urry, 1975, pp. 2-5)

Durkheim, as noted, was seen by Giddens as 'naturalist' in this sense. (Giddens, 1979). The second meaning of 'naturalism' is found in the 'interpretive' approach, where it is seen as 'soft science', as living in a 'natural' setting. (Hammersley and Atkinson, 1983, p. 6). In this second sense 'naturalism' is seen as:

"... counterposed to the positivists' primary and prior commitment to a conception of scientific method reconstructed from the experience of natural scientists."

(p. 6)

Keat and Urry stated that 'naturalism' (in the first sense) has been seen as almost synonymous with 'positivism', a use they disagree with, so these two terms were linked together, as also were 'anti-naturalism' and 'idealism'. This last is, roughly, that mind creates the world observed,

which might seem in some sense related to subjectivist conceptions of reality, and hence to aspects of the 'interpretive' approach. The above dichotomy thus seems to have some crossed wires somewhere!

However, if 'realism' is seen as opposed to 'naturalism', and this, in its first meaning, was linked to 'positivism', then Popper, logically, was not a positivist, even though, as noted below, he did believe in the idea of a unity of method in the natural and social sciences.

Popper's two main objections to logical positivists were in their definition of a 'scientific statement', and the use of 'verification' to establish the truth of such statements, or 'theories' or 'laws'. He also disagreed with other views of knowledge. His general objection to 'positivism' was, as noted, the use of inductive logic to derive theories from observations. Bryant stated that Popper also rejected the equation of the non-scientific with the meaningless. (Bryant, 1985, p. 115).

Popper defined a scientific statement, as distinguished from a non-scientific one, as one which:

"... must be capable of conflicting with possible, or conceivable, observations."

(Popper, 1963, p. 39)

This is the principle of "falsification" instead of 'verification'. Popper argued that the problem with verification was that, for some theories, virtually anything could be taken as confirming it. (Popper, 1963, p. 39 of Popper, 1961, p. 134).

Popper attacked the principle of inductive logic in writing of the growth of "human knowledge". In his view, there was a problem with theoretical knowledge, because of "an apparent clash" of principles. He cited these as:

- "(a) ... it is impossible to justify a law by observation or experiment since it 'transcends experience';
- (b) the fact that science proposes and uses 'laws' everywhere and all the time ...

(c) the principle of empiricism which asserts that, in science, only observation and experiment may decide upon the acceptance or rejection of scientific statements, including laws and theories.

Popper claimed that in fact these did not clash, if it was accepted that acceptance of a theory or law was 'tentative', that is, that: "... all laws and theories are conjectures, or tentative hypotheses", which could be rejected by new evidence. The third principle, he argued, stood, since "observation and experiment" still decided whether a theory stood. But in his view, a theory was:

"... never inferred, in any sense, from the empirical evidence Only the fallibility of the theory can be inferred from empirical evidence, and this inference is a purely deductive one."

(Popper, 1963, p. 54)

Popper discussed other views of knowledge before stating his own. He stated that:

"... scientists have dared ... to create myths, or conjectures, or theories, which are in striking contrast to the everyday world of common experience yet able to explain some aspects of this ..."

citing the "Galilean tradition" and stating that in part he supported this, in particular the first of the three doctrines which he stated were part of this. This first doctrine of the 'tradition' was that:

"The scientist aims at finding a true theory and description of the world (and especially of its regularities or laws) which shall also be an explanation of the observable facts."

Other parts of this 'tradition' he did not accept; such as the idea that scientists could succeed in "finally establishing the truth" of any theory, nor the idea that "best scientific theories" described "essences, or the essential nature of things". (Popper, 1963, pp. 103-4). This was the doctrine of 'essentialism'. Popper disagreed with 'essentialism' in its concern for "ultimate explanations", meaning those theories or laws that required no further explanation. He did not reject the idea of 'essences', but denied its use, for:

"... whether essences exist or not, the belief in them does nothing to help us in any way and indeed is likely to hamper us."

(Popper, 1963, p. 117)

He argued that "science is capable of real discoveries" and also that it aimed at a true description. His view of knowledge was that scientific theories were:

"... genuine conjectures - highly informative guesses about the world which although not verifiable ... can be submitted to severe critical tests.

(p. 115)

Popper also claimed that the aim of science was "partly theoretical" and "partly practical", or concerned with prediction and technical application. (Popper, 1979, p. 349). He pointed out that in the history of science, any methods had been acceptable, but in his view they all had one thing in common, for:

"... they all consist of a logical deduction, a deduction whose conclusion is the explicandum - a statement of the thing to be explained, and whose premises consist of the explicans - a statement of the explaining laws and conditions."

(This is the 'covering law' model previously noted). Popper claimed that the principal change in method (of logical positivists) was:

"... the silent abandonment of certain implicit demands concerning the character of the explicans,"

for example, that it can be grasped by intuition, (or inductive logic).

Popper, 1972/9, p. 349). However, as noted, this was not the case for all logical positivists.

Methodologically, Popper rejected the view that "... science proceeds from observation to theory". (Popper, 1963, p. 45; see also Popper, 1972/4, p. 346). In discussing what he called the "bucket" versus the "searchlight" idea of knowledge, he claimed that observation was a process:

"... in which we play an intensely active part We do not have an observation ... but we make" [one]

(Popper, 1972/0, p. 342)

Popper further stated that observations were always preceded:

"... by a particular interest, a question, or a problem, in short, by something theoretical."

(Popper, 1973/9, p. 342)

He added that what he termed "the horizon of expectations" or the:

"... sum total of our expectations, whether sub-conscious or conscious, ... plays the part of a frame of reference, only their setting in this frame confers meaning on our experiences, actions and observations."

Hence, observations were preceded by hypotheses. For:

"... we learn only from our hypotheses what kind of observations we ought to make."

(p. 345)

This is what he meant by the "searchlight theory of the mind" as opposed to the "bucket theory", where observations led to hypotheses. As noted earlier when discussing Comte, the latter also believed that 'theory' guided observation, and so do some sociologists working with the 'interpretive' approach. Geer, for example, wrote of a "prefield hypothesis". (Geer, 1969, p. 153). Denzin also commented in relation to Becker's study of marijuana users that he: "... continually assessed his findings against his conceptual framework". (Denzin, 1978, p. 15). Becker himself spoke of using a theory in this study. (Becker, 1982, pp. 170-1). However, others in the 'interpretive' tradition seem to be using the 'bucket' idea, as in Glaser and Strauss's "grounded theory". Both types of approach are referred to again, particularly in the *section on analysis in this chapter*.

Returning to Popper, it was noted that, in his view of knowledge, he described himself as a 'realist', and 'rationalist', and as such was opposed to 'positivism'. However, he did believe in the basic unity of the natural and social sciences, although for different reasons than the original Comtean one. His view is discussed below. But first, it should be stated that Popper was not a positivist, in another sense than those noted so far. He rejected the idea of prediction as the aim of science, which had been at least the 19th century view, in favour of explanation. The 'interpretive' approach is also generally concerned with the explanation of findings, although some might reject this idea.

It is difficult to assess where Popper can be placed for, as noted,

Outhwaite described the hypothetico-deductive model as a variant of positivism. (Outhwaite, 1987b, p. 6). However, it is not clear what happened, or when, to induction, since this was, as noted, a feature of Durkheim's 'positivism'. Stanley and Wise, on this point, agreed that the "deductivist" model appeared in "research texts" as "positivism".

They went on to argue that, rather:

"... it is deductivist positivism which is presented to us as 'positivism' and inductivist positivism which is presented to us as 'naturalism'."

(Stanley and Wise, 1983, p. 151)

They were using 'naturalism' here in the sense that, as noted, Hammersley and Atkinson did, as opposed to 'positivism'. (Hammersley and Atkinson, 1983, p. 6). Certainly some forms of the 'interpretive' approach, seen as 'naturalistic' in this sense, do use induction to arrive at explanations from observations.

It has been noted that Popper, as a "realist", could have been opposed to 'naturalism' in its first sense, and so not have been a positivist.

However, Keat and Urry pointed out that though opposed, 'realism' and 'positivism' shared certain features, in their view. Both they considered, saw science as 'objective' and 'rational'. Objectivity had two aspects, as they saw it. One was that theories had to be: "... objectively assessed by reference to empirical evidence". Secondly, there was the idea that 'objects' had an independent existence apart from: "... our beliefs and theories about them". This gave the idea of a distinction between 'the world' and attempts to explain it. Science was thus held to describe not constructs, but 'reality'.

Rationality involved the idea of "general standards of scientificity" - what an adequate explanation was, what was the aim of scientific activity, and the way in which empirical evidence was assessed. Keat and Urry stated that although 'positivists' and 'realists' disagreed:

"... about what these standards are, both believe that they exist."

(Keat and Urry, 1975, p. 44)

This might also be true of at least some 'interpretive' sociologists, if not all, because, although their research takes account of the 'subjective' meanings of actors, as Durkheim did not, they are not wholly concerned with these. In discussing questions of bias and 'validity' there is some implication that there is a standard for adequate evaluation of accounts.

Returning again to Popper, it was noted earlier that he did believe in the basic unity of method in the natural and social sciences. He claimed that if the 'hypothetico-deductive' model were used, which tested by way of 'falsification', then this unity existed, because both relied on similar explanations. But although sounding 'positivist', his idea of 'unity of method' was not the 'naturalistic' (in the first sense) view that there was one logic of science to which social science might aspire. Both, in Popper's view, already shared it. He stated that while:

"... almost all the great students of this problem ... hold that the humanities differ radically from the natural sciences, and that the most outstanding difference lies in this, that the central task of the humanities is to understand, in a sense in which we can understand men, not nature."

(Popper, 1972/9, p. 183)

and he accepted that this 'understanding' was their aim, he himself doubted "... whether we should deny that is is the aim of the natural sciences also". (p. 183). His view was that if the "inductivist" and "verifying" model was not used, then his view of scientific explanation also aimed at 'understanding'. He gave four similarities of 'understanding' in both natural and social sciences. One, people could be understood through shared humanity, likewise nature, for people were also part of that. Second, people were understood through: "... some rationality in their thoughts and actions", and laws of nature could also be understood through their inherent rationality.

The point about understanding people through 'rationality' seems important, for even for 'ethnomethodology', a rather 'out on a limb' aspect of the 'interpretive' approach, seeks to demonstrate the underlying 'rules' of behaviour, which implies some degree of rationality in actions. But this sort of 'understanding' depends on some agreement as to what is 'rational'.

Popper's third point was that scientists attempt to understand the world as a work of art is understood, "as a creation". (This could also be said of the 'interpretive' approach, in a sense).

Fourthly, Popper held that in the natural sciences, there was:

"... a consciousness of an ultimate failure of all our attempts to understand."

He said that this failure had been discussed in the humanities in relation to the 'otherness' of people, and the:

"... impossibility of any real self understanding, and the inevitability of over simplification which is inherent in any attempt to understand anything unique and real."

(Popper, 1972/9, p. 184)

No one in the 'interpretive' tradition would really dispute this statement. In Popper's view, both 'sciences' shared the same methods of problem-solving, 'conjectures and refutations'. (Popper, p. 185).

Thus, Popper rejected any attempt to claim that 'understanding' was the prerogative of "the humanities" and was the means of demarcating the social and natural sciences. (Popper, p. 185).

Popper stated that when supporters of such an attempt criticised his view as:

"... 'positivistic' or 'scientistic', then I may perhaps answer that they themselves seem to accept, implicitly and uncritically that positivism or scientism is the only philosophy appropriate to the natural sciences."

(Popper, 1972-4, p. 185)

Popper certainly took a different view to Comte, and more particularly from Marx, on the idea of laws of development. He was critical of any attempt in

the social sciences to discover laws from which predictions could be made. Prediction of future events was impossible because human society was not a closed system, and consequently there were too many complex variables. (Popper, 1963, pp.339-42). There was also no way to measure anything like "trends". In his view, any such attempt was based on trying to apply the methods of physics to society, (and physics was the science for logical positivists). This arose from a mistaken view of reasoning in physics, especially inductive logic. If hypothetico-deduction, not induction, was used, the idea that theories could be falsified, and attempts at prediction were dropped, then, as noted, Popper believed in the basic unity between the natural and social sciences. (Popper, 1961, p. 34).

In saying that the 'historicist' position was based on: "... a misunderstanding of the methods of the natural sciences" and were "a misguided attempt to copy these", Popper used the term "scientistic" noted above. He attributed this to Hayek, who:

"...uses the term 'scientism' as a name for 'the slavish imitation of the method and language of 'science'. Here it is used, rather, as a name for the imitation of what certain people mistake for the method and language of science."

(Popper, 1961, p. 105)

Popper later added that:

"Hayek now agrees ... that the methods actually practised by natural scientists are different from what most of them told us ... and urged the representatives of other disciplines to imitate."

(Popper, 1972/9, p. 185)

Bechhofer made a similar point in stating that there were idealised accounts of what occurred in the natural sciences and could occur in some kinds of sociology. There was not much harm in such accounts, he considered, though he noted that as a description of most sociological research it was "unsatisfactory". He added further that: "... even actual science tends to be carried out rather differently". (p. 72). Bechhofer also stated that in sociology an "idealised 'scientific method'" might not be a useful

model. He stated that this did not mean, however, "mindless empiricism".

But he pointed out:

"... the research process ... is a messy interaction between the conceptual and empirical world",

not a "clear-cut sequence". Incidentally, he also considered that "deduction and induction" occurred at the same time. (Bechhofer, 1974, p. 73). This contrasted with Popper's view. Medawar, who agreed with Popper in rejecting induction, did note a much earlier statement than Bechhofer's of the dual connection, when he cited Jevons as saying that:

"... all inductive reasoning is but the inverse application of deductive reasoning?"

(Medawar, 1967, p. 134)

Entwistle, in a diagram of the 'hypothetico-deductive' model, also stated that induction was part of theory building in 'the scientific method'. (Entwistle, 1973, p. 16).

Worsley, who did regard the hypothetico-deductive model as the 'positivistic' method (p. 74) did also state that: "... in real life scientific work, the actual carrying out of research" which might include theorising, testing and possible reformulation of hypotheses to eventually arrive at some theoretical insight:

"... takes place at one and the same time and in a much more haphazard way."

(Mitchell, I. C., 1977, pp. 76-77)

He also stated that it could no longer be assumed that there was only one way of doing science, such as:

"... a standard set of procedures which we can use to test whether a proposition is valid or not"

because there was little agreement as to what constituted the "scientific method". (Mitchell, I. C., 1977, p. 58). In this respect his views were similar to Bechhofer's. Both these views could be classified as 'conventionalist' within the philosophy of science. The main theme of 'conventionalism' is that a 'convention' or general decision governs the choice of descriptions of the world they are not empirically given. As noted earlier, Outhwaite saw some 'logical positivists', at least, as shading into 'conventionalism'.

(Outhwaite, 1987b, p. 10).

Kolakowski stated the "fundamental idea" of conventionalism as being that:

"... certain scientific propositions, erroneously taken for descriptions of the world ... are in fact artificial creations, and we regard them as true ... because they are convenient, useful, or even because they have aesthetic appeal."

(Kolakowski, 1972, p. 150)

He added that:

"Rival hypotheses accounting for a given aggregate of facts may be equally sound from a logical point of view and hence our actual choices are accounted for by non-empirical considerations. In this sense our image of the world has a conventional character."

(Kolakowski, 1972, pp. 158-9)

These 'non-empirical' matters are in effect value-judgements. The role of these in scientific statements, and the idea of a 'value-free' science (or sociology) is returned to later. First, though, it is pointed out that the 'conventionalist' argument was not directly concerned with the issue of values, but was in part an attempt to solve the 'problem of falsification' in Popper's model of theory building. (Halfpenny, 1982, p. 102).

This problem arises for two reasons. As noted, Popper himself stated that laws and theories, as well as tests, were 'tentative conjectures' which could be in principle falsified. Bryant pointed out that:

"... according to Popper, a "basic statement" or "basic proposition" is a statement which can serve as a premise in an empirical falsification, in brief, a statement of a singular fact."

(Bryant, 1985, p. 115)

Since the premises referred to 'laws', or theories, in order to arrive at the "explicandum", and these were "conjectures", then these 'basic statements', as Bryant noted, were themselves "corrigible". But, as Bryant also pointed out, so were empirical tests. These last:

"... whether they yield corroborations or refutations, are themselves contestable."

(p. 116)

So all the statements in a hypothetico-deductive statement were 'contestable'.

Halfpenny also pointed out that, given this problem, that:

"... test implications are deducible from a hypothetical law only in conjunction with a set of auxiliary conditions and auxiliary hypotheses, perhaps summarised by a ceteris paribus clause. Consequently, it remains uncertain whether a falsifying statement results from the failure of the hypothesis under test, or the falsifying of one of the auxiliary hypotheses, or the failure to fulfil any one of the conditions."

(Halfpenny, 1982, pp. 102-8)

The 'conventionalist' solution to this problem of falsification is to allow some hypotheses to be accepted as 'conventions' or 'methodological rules', to allow progress so that other hypotheses could be tested. However, the problem with this solution was that given a lack of criteria for determining which hypotheses should be taken as conventions, or "unfalsifiable", in this way, then any hypothesis could:

"... be protected from disconfirmation by the whim of the individual scientist or by agreement among a community of scientists."

(Halfpenny, 1982, p. 103)

Popper himself stated in relation to 'falsification' that while it was possible:

"... to save a falsified theory by means of supplementary hypotheses ... this is not the way of progress in the sciences."

(Popper, 1972, p. 360)

A second problem with falsification was noted by Lakatos, who distinguished between what he termed 'naive' and 'sophisticated' methodological falsification in Popper's work. He said on this point that:

"... any scientific theory has to be appraised together with its auxiliary hypotheses, initial conditions, etc., and, especially, together with its predecessors so that we may see by what sort of change it was brought about ... what we appraise is a series of theories rather than isolated theories."

(Lakatos, 1970, p. 118)

If each 'theory' consisted of a hypothetico-deductive statement, each part of which was 'contestable', and a body of such 'theories' was appraised,

this would seem to magnify the problem of falsification already noted.

Lakatos himself also stated that there could be no falsification "... before the emergence of a better theory" (p. 119) and added that if this was so, then "... criticism becomes more difficult, and also positive, constructive". (p. 120). Lakatos noted the importance of the "proliferation of theories" for sophisticated falsification, because this "... stresses the urgency of replacing any hypotheses by a better one". (p. 122) and he argued that "the honesty" of this falsification:

"... demanded that one should try to look at things from different points of view, to put forward new theories which anticipate novel facts, and to reject theories which have been superseded by more powerful ones."

(Lakatos, p. 122)

Keat and Urry pointed out some objections to the idea that a theory should be abandoned only if an alternative theory existed which both explained everything that the first one did and:

"... explains and generates predictions not derivable from "(the first)" some of which have been confirmed by empirical testing."

(Keat. and Urry, 1975, p. 49)

These authors considered this view "too stringent", since the "... alternative theory might be preferable". Some theories might be totally at variance with observations, and should be abandoned. They stated also that there was no:

"... analysis of the stage at which it becomes rational to develop alternative theories to the one in trouble."

(Keat and Urry, 1975, p. 49)

But Lakatos had stated that:

"Falsification cannot 'compel the theorist to search for a better theory', simply because falsification cannot precede the better theory."

(Lakatos, 1970, p. 122)

Keat and Urry also noted the view of Feyerabend, which they sum up as being that:

"... a wide range of competing theories should always be formulated, and scientists should never be concerned solely with the development and testing of any one theory."

(p. 49)

But this was exactly the point made by Lakatos.

Keat and Urry themselves noted that there was an objection to abandoning theories in favour of others, that it might not be possible to compare them because of the lack of a "theory neutral" language for such comparison. They stated that many scientists noted this lack. They pointed, for example, to Hanson's view of perception, or seeing, that what is 'seen' depends on what is believed, and that it was these beliefs which were expressed in theories, so that the truth of claims about observation depended on the truth of the theories. So that, if the observations were true, then so were the theories. but if this was the case, then theories could not be falsified by means of true observation statements, since if these were true then the theories could not be false.

Thus, it was impossible to judge between two competing theories on the basis of what was observed, since the supporters of these theories could not agree on the truth or falsity of observations, which were related to their beliefs (or values).

However, Keat and Urry also stated the views of Hooker, that there was:

"... little evidence that the kind of theoretical beliefs normally involved in scientific disputes affected perceptual judgement."

(p. 53)

This seems a little too simple.

Keat and Urry themselves stated that:

"Much of the language used to describe what we see ... is theory laden ... it assumes the truth of various scientific theories and beliefs ... the ways in which we describe observations will be such that they appear to constitute fairly conclusive counter-evidence to theories that are opposed to these observational descriptions. It follows that in the formulation and defence of alternative theories, an important element will be the detection and challenging of the theoretical assumptions made in the existent observational language. Otherwise, many such alternatives will be prematurely dismissed."

(Keat and Urry, 1975, p. 56)

This seems as important for sociology as for science. It is sometimes possible not to be aware of one's own theoretical assumptions. This was one reason why both 'positivistic' and 'interpretive' models were explored in some detail.

When discussing 'conventionalism', Keat and Urry referred to both Kuhn and Feyerabend, although they did not state directly that these were 'conventionalist'. Both were, however, critical of one model of science, and also noted the effect of values.

Kuhn, for example, wrote of 'normal science' and 'revolutionary' science in terms of competing paradigms, or 'world views'. In 'normal' science, a set of habits and 'conventions', basic assumptions which are not questioned (as in 'conventionalism') form a bedrock of related theory within which 'puzzle-solving' takes place. 'Revolutionary' science occurs when 'anomalies' increase, to the extent that a new paradigm emerges. But this does not become accepted rationally, by reference to a standard, for Kuhn held that different theories were 'incommensurable', for each paradigm had its own internal standards. (Kuhn, 1970). Thus Kuhn seemed like Hanson, to be indicating that what was 'seen', or accepted as evidence, depended upon previous experience, which included beliefs - also, Kuhn, like Keat and Urry, was pointing out that a value free language for comparison did not exist.

These ideas seemed important when considering 'observation' in the 'interpretive' tradition in sociology, since if what we see depends upon what we believe, or our 'frame of reference' or 'hypothesis', or 'world view', then this does raise questions about the issue of validity and how this can be assessed, and by reference to which standard, especially when the concepts of validity and reliability have been associated with 'positivism' and the scientific model in the eyes of some writers.

Kuhn's views had some influence for the emergence of the 'New' sociology

of education, which adopted 'phenomenological' ideas. (Eggleston, 1972, p. 13). His book appeared to give some credence to the idea that knowledge was relative, and thus to a critique of the 'traditional' curriculum in schools, as well as of sociological method.

However, in the ensuing debate in the philosophy of science, Lakatos rejected Kuhn's view of alternating paradigms. He stated that:

"The history of science has been and should be a history of competing research programmes (or if you wish, paradigms), but it has not been and must not become a succession of periods of normal science: the sooner competition starts, the better for progress. 'Theoretical pluralism' is better than 'theoretical monism'. On this point Popper and Feyerabend are right and Kuhn is wrong."

(Lakatos, 1970, p. 155)

Feyerabend, in discussing Kuhn's view of scientific revolutions, was partly critical of both Lakatos and Popper. His view was that science:

"... both is, and should be, more irrational than Lakatos ... is prepared to admit."

(Feyerabend, 1970, p. 214)

He argued that this might be an essential feature, for:

"The numerous deviations from the straight path of rationality which we observe in actual science may well be necessary if we want to achieve progress with the brittle and unreliable material (instruments; brains; etc.) at our disposal."

(p. 219)

Feyerabend's view was that the sciences were a human "enterprise". So:

"... the choice between theories which are sufficiently general to provide us with a comprehensive world view and which are empirically disconnected may become a matter of taste."

(p. 228)

He added that "scientific method" was only:

"... an ornament which makes us forget that a position of 'anything goes' has in fact been adopted."

(p. 229)

So that scientific activity would seem to be a pragmatic affair, doing what works, with choice of actual practice a matter of 'taste' or value, or experience.

Worsley also stated that reason alone did not decide what scientists did, but that "hunches" and "serendipity" played a part in their activities. (Worsley, 1977, p. 60).

Medawar also pointed to the role of imagination in the development of theory. He was likewise critical of a unidimensional view of 'science', in terms of both scientists and their methods. He considered that there was no such thing as 'the scientific mind'. Scientists had dissimilar temperaments, for one thing, and did different things in different ways. Among scientists:

"... were collectors, classifiers and compulsive tidiers-up; many are detectives by temperament, and many are explorers; some ... artists, and others artisans. There are poet scientists, philosopher scientists and even a few misfits."

(Medawar, 1967, p. 132)

Medawar also rejected the idea of 'a' scientific method, with one set of rules. He cited one argument on this, which claimed that there were:

"... no rules for the pursuit of truth which shall be universally and peremptorily applicable."

(Master of Trinity, cited by
Medawar, 1967, p. 132)

Medawar himself stated that in science there were two views. One was of science as a critical analytic activity and one of it as: "... an imaginative exploratory activity". In the first, truth was seen as residing in nature, to be got at only through the "evidence of the senses". (This seems to mean empirical observation). From this view, the task of the scientist was:

"... discernment. This act of discovery can be carried out according to a method which, though the imagination can help, ought not to depend on the imagination; the Scientific Method will see him through."

(Medawar, 1967, p. 118)

Opposed to this was the second view, the "Romantic" idea of science.

According to this, said Medawar, the truth took shape in the observer's mind, as an imaginative grasp of what might be true. Thus, every outcome

of science was the result of a: "... speculative adventure, an excursion into the unknown". (p. 118) Medawar's view was that these two conceptions were in fact complementary, for there were elements of truth in both, even if they seemed to be contradictory. In Medawar's view, this fact was known by anyone who had actually: "... done or reflected deeply upon scientific research". (p. 118) That the two conceptions were complementary was because, in his view, every scientist must be: "... freely imaginative, yet sceptical, creative and yet a critic". (p. 118) There was, he considered, a sense in which the scientist must be both free in thought but also:

"... very precisely regimental; there is poetry in science but also a lot of book-keeping."

(p. 118)

It was in this way that the two conceptions were united, he considered.

Like Popper, Medawar was also critical of inductive logic, which he claimed had been a major model in England. He saw this form of reasoning as a:

"... logically mechanised process of thought which, starting from simple declaration of fact arising out of the evidence of the senses can lead us with certainty to the idea of general laws."

(pp. 118-9)

He called this a "myth". His view was that if this process was abandoned, and if a clear line was drawn between having an idea - the "imaginative aspect", and "trying it out", which was a: "... ruthlessly critical process to which many skills and hands must contribute" (p. 119) then the problem of induction was solved. In his view it was "as simple as that". He reiterated that imagination and criticism were equally necessary to the scientist. They seem equally necessary to the sociologist.

Medawar also argued that the 'hypothetico-deductive' conception joined the two sets of "contradictory opinions of science". His view was that a hypothesis meant, in professional vocabulary:

"... an imaginative pre-conception of what might be true, in the form of a declaration with verifiable deductive consequences."

Thus, scientists:

"... are building explanatory structures, telling stories which are scrupulously tested to see if they are stories about real life."

(Medawar, 1967, p. 152)

Popper, as noted, disagreed with the idea of 'verification', but his view of science as a matter of 'conjectures' and 'refutations', seems very much like Medawar's picture of the activity.

Also like Popper, Medawar contended that there were misconceptions about scientific thought. He stated that:

"The equation of science with facts and of the humane arts with ideas is one of the shabby genteelisms that bolster up the humanists' self esteem."

(Medawar, 1967, pp. 143-4)

Medawar's general comments on methods, and the 'conventionalist' argument, indicate that the term 'the scientific method' is an inexact one, to say the least. The epistemological debate around logical positivism, discussed above, made problematic what counted as a meaningful statement, the nature of theory and its link with observation, the grounds on which a theory might be rejected and at what point, and the whole idea of a 'value-free' science. Hence, the equation of 'positivism' (and of which version in any case) with 'the scientific method', seemed equally inexact. The discussion of these epistemological concerns has methodological aspects in the sense that it has been concerned with just this question of what 'the scientific method' might be. But it was also said earlier that there was another, more strictly methodological question associated with 'positivism', and that that was the view that it was linked to the use of 'quantitative' methods. It is this issue that is next considered, along with what these methods are said to entail.

Hammersley and Atkinson noted the connection of positivism with

quantitative methods, as did Cuff and Payne. (Cuff and Payne, 1979, p. 161; Hammersley and Atkinson, 1983, p. 4). Griffin also noted the assertion of this relationship, although she herself denied that there was a "hard and fast" relationship. (Griffin, 1985, p. 100).

Sometimes, it seems that the terms 'scientific method' and 'quantitative methods' are seen as referring to the same thing, but there is also the idea that 'quantitative methods' refer more directly to measurement in some form. Lofland, for example, regarded them as:

"... a highly complicated set of techniques and technology
... developed to meet five requisites of the serious quest
for causes"

(Lofland, 1971, p. 61)

Keat and Urry also referred to this idea of 'measurement', but also used terms connected with ideas of 'the scientific method'. They claimed that American sociology from "the 1930's to the 1960's" was "largely positivist", and as such was concerned to establish:

"... general laws of social life from which empirically testable consequences can be derived, operationalising concepts such that they refer to the observable and especially the measurable, and the statistical manipulation of naively collected and organised empirical data. Facts are sharply distinguished from both values and theories, facts, particularly statistical facts, are neutral between different theories."

(Keat and Urry, 1975, p. 90)

They also noted the views of Lundberg, who, they stated, was a 'positivist' philosopher of the natural and social sciences. (His idea of the need for a 'working hypothesis' was noted earlier). Keat and Urry claimed that in 1939 Lundberg, arguing that the methods of natural science could be applied to the study of human behaviour, held that:

"In particular, the supposed subtleties, complexities and dynamic character of social phenomena in fact necessitate and do not preclude the use of symbolic logic, mathematization and quantification. Without methods of measurement, it is impossible to characterise adequately the enormous variety and gradations of social life."

(p. 92)

Keat and Urry also stated that by 1964 Lundberg considered that the "... battle

for the positivist method had been more or less won". (Keat and Urry, 1975, p. 92). But this 'positivist method' of Lundberg seems to refer more to 'logical positivism'. As noted, in its early form, at least, the latter placed great emphasis on mathematical logic and 'verification', and some practitioners were also inductivists.

Lundberg certainly took the view that mathematical language was "the language of science". (Lundberg, 1942, p. 83). He also considered that the methods of science could be applied to the social sciences, and should be, because they provided greater "objectivity". This was thought necessary because of the "fallibility and inadequacy of the unaided senses". (p. 49) He held that the "... transition towards objective standards was definite" in "the social sciences". (p. 51). Lundberg also wrote of "verifiability" in "scientific" conclusions, and "verifiable" steps in "the scientific method" and "verification". (pp. 4, 8 and 9). He further stated with reference to induction, that:

"... the tendency in recent times has been toward the inductive approach and the quantitative statement of social observations."

(p. 51)

He also stated that:

"Fundamentally ... all "hunches", "intuitions" and hypotheses rest upon an informal, or even "subconscious" inductive basis."

(p. 119)

Lundberg also wrote in approving terms of Karl Pearson (p. 5, 11, 15 et al). Pearson was claimed by Medawar to be "a true Baconian", or inductivist. (Medawar, 1967, p. 138). However, Lundberg, in noting four levels of increasing rigour in the scientific method, stated that "the fourth and most advanced" was that:

"... found in experiments and other crucial compilations of data which are directed by systematic and integrated theory"

(p. 7)

His description of this, with reference to Newton and Einstein, reads very

much like the 'covering law' model of Hempel and perhaps Popper. Lundberg also wrote of the role of deduction in this. Deductions could be valid or invalid, thus, according to "the accepted laws of reasoning" theories could be checked. (Lundberg, 1942, p. 8).

Thus, Lundberg may have been a 'logical positivist' and have favoured induction, but, as with Durkheim, this did not necessarily rule out deduction. However, even if a 'positivist' of some kind or another, Lundberg did not absolutely reject 'qualitative' approaches. Of the suggestion that the subject matter of the social sciences and natural sciences entailed either approach, he wrote that:

"The assumed conflict between qualitative and quantitative methods must, then, be abandoned in favour of the view that these terms merely represent different stages of refinement and objectivity in our technique of description."

(p. 23)

He also stated that there was a place for "subjective impressions" provided these were not seen as "infallible". (p. 49). In his methods he also noted "fieldwork", which he saw as containing not just formal interviews and surveys but also "participant observation". (Lundberg, 1942, pp. 349-393). Lundberg, overall though, did stress 'objectivity' and the use of 'quantitative' methods. However, 'logical positivist' or whatever, it has to be said that, in terms of clarity of expression, he scores points over, for example, Glaser and Strauss in the 'interpretive' approach.

Referring to 'logical positivism', Halfpenny noted its connection with quantitative methods. He stated that as this concept of positivism:

"... began to dominate the philosophy of science in the middle of the twentieth century, it undoubtedly provided reinforcement, albeit indirectly, for the notion that positivist sociology, the natural science of society, consists of the application of multivariate and inductive statistics to descriptive social statistics collected by sample surveys."

(p. 59)

Halfpenny indicated that Paul Lazarsfeld had been important in developing this notion and said that:

"... although he had no direct contact with the Vienna Circle
... described himself as a European positivist."

(p. 59)

Lazarsfeld established a research institute, the Bureau of Applied Social Research, and there were others. Halfpenny saw these as:

"... strengthening the professional identity of American sociologists around the collection and analysis of data."

(Halfpenny, 1982, p. 59)

Marsh similarly noted the importance of Lazarsfeld, but also recorded that after his death the Bureau "virtually collapsed", and that this indicated that, according to Barton:

"... not even Lazarsfeld could institutionalise social research in a way that was not dependent on the person involved."

(p. 45)

Marsh stated that Lazarsfeld's "most significant" contribution "... was in developing the approach known as 'survey analysis'." Thus far Marsh agreed more or less with Halfpenny, but she stated that in Lazarsfeld's "autobiographical reflection" he said that he had wanted to bring together:

"... the qualitative and the quantitative, introspection and behaviourism, intensive and extensive methods, subjective and objective."

These were his "personal equations", but he was unable to "interest anyone else" in them. (Marsh, 1982, p. 44).

If so, and if Lazarsfeld was a 'positivist', it suggests that 'positivism' was more complex and less 'hard line', than its 'term of abuse' opponents credited it with being.

'Quantitative methods', from the statements so far, seem to be regarded as a set of techniques involving measurement of some kind, which are used either to gather information about variables, or to seek to establish causal relationships between these, or to test hypotheses about such relationships.

The 'measurement' can simply involve counting of some kind. For instance, Marsh stated in relation to the coding of variables placed in categories, that these categories "... need represent nothing more than a

nominal level of measurement". (p. 53). That is, the coding process, at a minimal level, being "... able to establish between a characteristic being present and being absent" (Marsh, 1982, p. 53). 'Qualitative' research also uses 'counting', as Atkinson and Delamont pointed out. They said that it:

"... does aggregate 'instances'. Data are regularly collected into typologies, and at least an ordinal level of measurement is regularly invoked by fieldworkers in the claim that, say, things happen more or less frequently, are of more or less importance, are more or less disruptive and so on."

(Atkinson and Delamont, 1986, p. 249)

Thus, 'quantitative' methods can mean either nominal or ordinal measurement or multivariate analysis, and they are not necessarily confined to 'positivist' sociology. Having noted this, the sub-section briefly considers such 'methods' in terms of what they are actually applied to, in terms of methods of data collection and analysis.

These methods include, as they did for Durkheim, various situations where variables are classified and compared. As such, they include "case studies". "comparative research", "historical research", experiments and surveys.

'Case studies' are those made of a single example, whether of individuals, a group or an institution such as a school. As such, they are also found in the 'interpretive' tradition.

Historical research has also been used by both classical 'positivists' like Durkheim and some in the 'interpretive' tradition. Weber, for example, who belongs partly in this tradition, compared different societies to discover the reason for the emergence of capitalism in Western Europe.

'Comparative' research, as noted, was seen by Durkheim as the method of sociology. Verma and Beard defined this as a method whereby:

"... some investigators try to make comparisons of situations which provide a similar kind of information to that experi-

mental studies might yield. Such studies may be described as causal comparative."

(Verma and Beard, 1981, p. 65)

They argued that such research formed a link between case studies and experiments and surveys.

Attempts to establish causal relationships can be seen as a form of research design which Bulmer stated sought to:

"emphasise the importance of attempting to approximate to the logic of experimental design"

whether it actually followed an experimental form or not. (Bulmer, 1977, p. 9). As such, it could be seen as similar to Durkheim's 'natural experiment'. He considered that the variation in human societies provided the opportunities for this.

Lofland, who basically disapproved of the use of quantitative techniques in 'interpretive' research, did identify certain "requisites" of a search for causes. One was that there had to be some variation, in terms of its presence or absence, of one variable, the 'dependent'. Then there had to be some "reliable and constant way" of measuring such variation among "units" displaying it. Then such measured variation in the dependent variable had to be considered together with some other measured variation in a variable "... that is provisionally thought to be a "cause" of variation in the dependent variable". This last was the independent variable. Lofland also stated that another requisite was a "representative random sample of a given population". (Lofland, 1971, pp. 60-61). The question of representativeness is considered later, as it affects validity, another concept also discussed later. For the moment, it is pointed out that the main problem with any attempt to establish causal relationships has already been referred to in relation to Durkheim and with reference also to Becker. That is, more than one cause may be involved in any observed effect, and it may be impossible to rule out all rival hypotheses. On a rather different level, this conclusion was indicated by the problem of Popper's 'falsification' and the conventionalist response to this.

In terms of causal relationships and their testing, Marsh stated that:

"surveys and experiments are the only two methods known to me to test a hypothesis about how the world works."

(Marsh, 1982, p. 6)

This seems a rather narrow view, because at least some users of 'participant observation' in the 'interpretive' approach have also sought to test hypotheses as well as generate them. (Becker and Geer, 1982). However, experiments and surveys are situations which are chiefly associated in many texts with the use of 'quantitative' methods and/or 'the scientific method'. They are both therefore briefly discussed with reference to their stated advantages and disadvantages and to certain problems, which have been gathered from various books. It is not the intention to give a detailed exposition, since neither an experiment or a survey was used in this research.

First, though, it must be pointed out that experiments and surveys, and indeed any form of research, are all to a greater or lesser degree affected by the need to meet the problems of validity and reliability. These terms are therefore briefly defined.

Validity relates to the question of whether a piece of work is a 'true' account. It is basically a concern with what reliance can be placed on the data and the interpretation placed upon it, but there are some differences of emphasis and so some different usages of the term. Moser and Kalton, referring to attitude scales in particular, stated that it was possible to test validity, where a recognisable criterion existed. They noted "predictive" or "concurrent" validity for this, meaning whether a measure used could describe future or present behaviour on the criterion. (Moser and Kalton, 1971, p. 356). The same authors, also with reference to attitudes, used the term "construct" validity. This refers to the use of a theory "to postulate" some association between an attitude scale "and other variables" and then tests "these associations" for confirmation of the theory. (p. 356).

Apart from these special definitions, there are two overall aspects of validity, external and internal validity. The former means whether and to what extent the results of a piece of research are generalisable. The latter covers the questions of whether it measures what it is supposed to, whether it presents sufficient and reliable evidence, for what it claims to be the case, and whether it does actually describe the behaviour or ideas that it is expected to, or claims to. It thus covers matters of sampling, which concerns the issue of how representative are the people studied in respect of some wider population, the methods used and their manner of use, and the analysis and interpretation of data, with reference to anything that could 'contaminate' the data and perhaps invalidate it.

Sampling, or choosing some proportion of a given population, is necessary in many forms of social enquiry. The total population of the country is not usually studied apart from the census, nor is the whole of any other defined grouping where this is large, for reasons both of time and cost. Although in a small-scale study, such as the present research, all the members can be studied, this still raises the question of how representative that group is of some wider grouping.

There are various forms of sampling, and many refinements and variations within each. (Gardner, 1978; Moser and Kalton, 1971; McNeill, 1985; Selltiz et al, 1965). It is not proposed to go into all of them here. The ones used are likely to depend on the purpose of the research. Where representativeness is an issue, which is not always the case, Selltiz et al stated that:

"the basic distinction in modern sampling theory is between probability and non-probability sampling:"
(p. 514)

and they added that:

"Probability sampling is the only approach that makes possible representative sampling plans."

(Selltiz et al, 1965, p. 515)

In choosing a representative sample, the principle of choice is likely to

involve first the principle of randomness. This means that each unit in the chosen population has an equal probability of being selected. This selection is thus a matter of chance, generally based on the use of a table of random numbers. Therefore a number of variations, such as age, sex, social class and anything else, exist in a given population, and these are considered as likely to be important variables for the research (which can be a 'hunch' or based on prior work). The second principle is likely to be stratification. This means dividing the population under study into categories beforehand. From this, a stratified group can be chosen, usually at random again. This 'random stratification' (although again there are some variations of it) is one of the major versions of sampling where many variables are included. The problem with including many of these is that to ensure representativeness a large number of people may have to be studied.

In assessing the validity of a study, the sampling procedures are an important point to be considered.

Reliability, the other concept noted above, is concerned with the question of whether a piece of research is repeatable, that is, whether another researcher, under identical conditions, would come up with similar data and conclusions. Reliability is often seen as less of a concern in the 'interpretive' approach, because in one respect studies in this field are unlikely to be repeatable because the differences between researchers may affect relationships, leading to differential access to data, which could affect results. There is also the problem of the individuality of the situation, with the effect of intervening changes in personnel, so that another researcher, unless concurrent with the first, would not be in the same position. However, the concern with reliability need not be entirely absent from consideration.

Validity and reliability are related concepts, because if a piece of

research was invalid for any reason, it would be unlikely to be repeatable, or so it would seem. However, the converse may not hold in all cases. For example, Moser and Kalton stated in relation to attitude scales, used in questionnaires and surveys, that a scale could be reliable but not necessarily valid, because it would be measuring something other than what it was supposed to. (Moser and Kalton, 1971, p. 235).

Having briefly defined these two concepts and sampling in general terms, experiments and surveys are considered, though not in detail as noted.

The term 'experiment' has a strong association with science and the laboratory, and so with 'positivism'. However, experiments in the social science do not necessarily take place in laboratory or other non-natural settings, although they can do. Some have more of an aura of 'science' about them than others, while some can be relatively uncomplicated, such as a teacher trying out the effect of two different teaching approaches in a classroom with no fuss. Experiments are also more likely to use 'quantitative' methods than other research forms.

An experiment is basically an attempt to test some form of hypothesis that certain things may occur, or that certain relationships may exist. It is because of this 'testing' that quantifying the results may be more important.

The first step can involve identifying: "all variables or factors ... relevant to the problem under study". (Verma and Beard, 1981, p. 67). The problem here is how to identify them, and by what criteria. Trying to do so could well involve some form of prior investigation, perhaps including participant observation involving 'qualitative' methods. It still has to be decided which variables seem most important and relevant. Such judgement may not necessarily be 'objective', (so far as this is possible) but can be a 'hunch' or subjective feeling, thus bias will creep in.

The second step is to divide the chosen population, such as children of a given age or stage of education, or a class, or some other grouping, into an 'experimental' and 'control' group. In choosing a sample from the possible population, where necessary, the idea is that these two groups should be 'matched' as far as possible so that variables other than the independent variable being tested can be held constant, while the independent variable is then systematically varied. 'Matching' can be a problem in terms of validity because with people it is never possible to be wholly certain that groups are identical even in the chosen respects. There can be other hidden differences.

The main advantage of using experiments in the social sciences is that they do allow a number of possible variables to be tested more or less systematically. As indicated, they also allow for 'hunches' or imaginative guesses about which possible variables to test, to be given some play. In this sense they are 'scientific' in the sense that Medawar used this term.

However, there are disadvantages in using experiments where people are concerned. Verma and Beard noted that they could be difficult first because they were "costly", and secondly because human behaviour was: "... most difficult, and at times impossible, to control and manipulate". (Verma and Beard, 1981, p. 67). However, they considered that difficulties were not insuperable. There are other problems, however. For example, McNeill indicated that there are ethical considerations where people are concerned. He cited the 'authority' experiment of Milgram (1965), and the 'prison' experiment of Haney, Banks and Zimbardo (1973). In both of these psychological damage to participants was a real possibility. (McNeill, 1985, pp. 45-6). These experiments were possibly closer to a popular image of 'science' as a search for 'facts' at any cost. Not all social science experiments are of this type, however.

McNeill also pointed out another problem. This is that knowing that they are involved in an experiment can affect the responses of people. He cited the well-known 'Hawthorne Effect' as an example. (p. 47). This is another instance of some other variable than the one actually being tested causing the observed effect.

However, where people do not know that they are being used in an experiment, this is also an ethical issue. In cases where deception is involved, this may ultimately be 'self-defeating', as Kelman observed. (Kelman, 1979, p. 198).

Where experiments do not take place in natural settings, this can affect the responses of individuals, as some tests on children's use of language have indicated. Where they do take place in such settings, there can be some difficulty in controlling variables.

Given such problems, experiments have not been as widely used in the social sciences as in the physical sciences. Where they are, they are not necessarily all 'hard', but can vary in type and the degree of quantification.

Surveys, widely used in the social sciences, have, as much as experiments, been consistently associated with 'quantitative' methods and so 'positivism'. Marsh noted sardonically that:

"The criticism that surveys are positivist seems to have grown in popularity as the funds for surveys have declined."

(Marsh, 1982, p. 48)

Atkinson and Delamont also pointed out the connection in their criticism of a paper by Hamilton on the case study as a method for its "... crude stereotypes of research traditions". They stated that Hamilton, in contrasting case study and survey, had proposed the latter as "essentially positivist". Amongst other things, these authors were also critical of Hamilton's view that 'ethnographic' research was not concerned with

generalisation. (Atkinson and Delamont, 1986, pp. 248-9).

Burgess also observed that "sociological research" was at one time closely identified with survey methods. This "research" was contrasted with "anthropological" studies which used "field methods", including participant observation, so it was clearly the 'other' tradition which was seen as using survey methods. (Burgess, 1984, Preface). In Burgess's view such a sharp dichotomy no longer existed, and Marsh rejected in any case the idea that surveys were inherently 'positivist'.

Marsh defined a survey as:

"... an investigation where a systematic measurements are made over a series of cases yielding a rectangle of data.

b The variables in a matrix are analysed to see if they show any patterns.

c The subject matter is social.

In other words, the surveys ... have a particular method of data collection, a particular method of data analysis and a particular substance."

(Marsh, 1982, pp. 6-7)

There might be some dispute as to the degree to which methods of data collection and analysis were so particular to surveys.

Lundberg gave a rather broader definition of a social survey.

He stated that it was:

"nothing but a co-ordinated series of investigations or researches into related problems."

(Lundberg, 1942, p. 389)

As such, he considered, it utilised various methods such as the historical, the case and the statistical, rather than just one. (p. 389).

Moser and Kalton were also less specific than Marsh. They stated that it was in fact very difficult to define a social survey because:

"the term and the methods associated with it are applied to an extraordinary wide variety of investigations."

(Moser and Kalton, 1971, p. 1)

They stated, however, that surveys could be divided into two types, either

"descriptive" or "explanatory". The former was a "fact-finding" stage.

The function of the latter:

"may be theoretical, to test some hypothesis suggested by sociological theory - or severely practical - to assess the influence of various factors ... upon some phenomenon, but, whichever be the case, the purpose is to explain the relationship between a number of variables"

(Moser and Kalton, 1971, pp. 2-3)

Bulmer similarly noted a distinction between "descriptive" and "analytic" surveys. These are akin to Moser and Kalton's two, but the definitions are a little different. Bulmer stated that "descriptive" surveys were:

"designed to portray accurately the characteristics of particular individuals, situations or groups (in terms of behaviour, attitudes and dispositions to act) and to determine the frequency with which such behaviour or attitudes occur in the population being sampled."

(p. 54)

This raises problems as to how as well as if, such accuracy and determining may be achieved.

"Analytic" surveys, Bulmer stated, were those which attempted to

"test hypotheses about the relationships between variables in order to understand and explain social phenomena."

(Bulmer, 1984, p. 54)

Bulmer considered that in Britain the 'descriptive' survey had been more common. It has been noted earlier that Rex also pointed out the 'book-keeping' tradition of British sociology. (Rex, 1978). It was noted in the Review that many educational surveys, such as that of Bassey, were descriptive, although not all were.

Bulmer also considered that many of these surveys had used inductive reasoning. He argued that in British surveys research:

"Popperian ideas" [unspecified] seem to have had a relatively slight impact on the practice A large number of studies still espouse an unexamined inductivism or what Stinchcombe has called 'Marxified Fabianism'. Britain lags behind seriously in the exploitation of the analytic potential of social surveys."

(Bulmer, 1984, p. 55)

This comment was also made in the 1977 first edition of this work, so

presumably Bulmer had not changed his mind in the interval.

The comment seems to imply that it is the "analytic" surveys which might use "Popperian ideas", which in turn seems to imply that these involve deductivism rather than inductivism.

Marsh, in a chapter on the history of surveys, also noted the existence of the 'fact-finding' tradition. She offered a partial agreement with Bulmer's view of the predominance of this tradition in Britain. She stated that the 'Social Survey' (which merged with the Office of Population Censuses and Surveys in 1970) had not been "so innovative" in analysis as in methodology.

She stated that:

"It is still firmly within the tradition of fact-finding surveys, and this has not encouraged the development of analytic procedures to explain these facts."

(Marsh, 1982, pp. 34-5)

However, she also noted the development of statistical procedures as survey research grew. In her view, the:

"gradual elaboration of a more complex model allows a fruitful interaction between one's existing ideas and the data."

(p. 96)

She also argued that both deductive and inductive reasoning were used in analytic interpretations, because in her view:

"To turn up one's nose at exploring fully anything more in the expensive survey data one has collected would be folly indeed,"

(p. 96)

The concept of 'explanation' found in Moser and Kalton's and Bulmer's distinctions, and that of 'understanding', raise the philosophical problem of what exactly they mean, and by what and whose criteria they are judged to have been achieved. Also, 'explanation', where it involves the view that a causal link can be established, can never be regarded as doing so with certainty, whatever the form of research. In 'Popperian'

terms, all explanations are tentative. Marsh, who was concerned to:

"examine the extent to which surveys can provide explanations of social phenomena which meet Weber's requirement that they should be both causally adequate and adequate at the level of meaning"

(p. 69)

recognised this difficulty. She stated that, in relation to the concept of cause, (meaning is discussed later) that: "It is important to accept ... that causes do really exist out there in the world". (p. 69). She had earlier observed that in survey research, testing causal hypotheses was a matter of drawing inferences from existing variations in a population by a "rigorous process of comparison". However, she added that while a researcher would try to control other variables that might be thought to produce an effect, it was never possible to get over "the purists' objection" that a causal relationship could not be "definitely established". Also, even if a panel, or series of data, were available over time, which helped with the problem of deciding which variables were "prior to others", this did not solve the further problem that any causal relationship might be explained by some other "unmeasured factor". (p. 7).

Marsh also said that:

"Properly designed, anticipating rival causal explanations as far as possible and building in ways to test them, surveys can provide evidence for and against different causal models. This evidence is not proof, however, it is only as good as the model is."

(p. 96)

Hence the importance of survey design and the choice of methods.

In relation to the distinction noted above, Marsh argued that "... the line between description and explanation is hard to draw for several reasons". (p. 37). She made the further point that:

"it is especially dangerous to start equating explanation with science and description with non-science."

(Marsh, 1982, p. 37)

This recalls the rejection by Popper and Medawar of the equation of science with 'facts' and the humanities with 'understanding'.

In terms of both scope and methods, it was once thought by this researcher that surveys were something which were large-scale and used questionnaires, which is perhaps a general perception. However, they can be large-scale or small in scope, depending on various factors, such as the purpose, whether a survey is commissioned or not, and by whom, or undertaken by an individual or a small number of people, the time required and the likely cost in the light of the available resources of both. In terms of methods, the use of questionnaires is only one of those available as methods texts point out.

In discussing briefly surveys and the methods used in theory, the intention is again not to produce a detailed critique, since there are many books, but only to pick out some of the principal features of surveys, and disadvantages ascribed to them, as against the 'interpretive' approach.

The term survey covers not one but a range of activities, as Lundberg suggested. In terms of actual methods of data collection this range can include questionnaires, interviews and observations, or the use of documents, or historical comparisons, to either describe a group in terms of certain characteristics or compare across a range of groups. They can be exploratory, or can be used to test theories or build on previous work. They can be both 'quantitative' and 'qualitative'. Most of the methods used in surveys can also be found in 'interpretive' research, although questionnaires are less usual. The actual form of observations and interviews may be rather different in a survey, though this is not necessarily the case. Like other forms of research, whether experimental or 'interpretive', a survey has a purpose, and therefore an important consideration is how to best design it to achieve that purpose. Overall design includes, given the overall research interest, decisions about the kind of questions to ask which might cover this. Then there is the question of to whom they are to be put, or sampling, and the means of choosing the sample. As noted, this is a problem in itself. Design also involves consideration of the most appropriate methods, in

the particular circumstances, of seeking answers to the questions.

One of the principal ways of formulating and asking questions in a survey is some form of questionnaire. This method is the one with which people are most familiar, since many will have answered at some time, even if only of the market research type. Thus it is easy to see how the misapprehension that this is 'the survey method' arose. It is discussed first because it is a major means of collecting data in a survey.

Within the overall research design, questionnaires also have to be designed. This is a whole problem area in itself, as Moser and Kalton indicated. (Moser and Kalton, 1971, pp. 308-10). Much of the literature on research design seems to concern itself most with questionnaire design in one form or another.

This researcher has experienced some of the difficulties in design, having had to develop and use a questionnaire at fairly short notice at the insistence of one head teacher, something discussed in Section Six of this chapter.

The main difficulties with questionnaire design deal with the actual formation of the questions. This is known to be full of problems. As Gardner stated:

"Many chapters and even books have been written given essential guidance on asking questions on the known pitfalls."

(Gardner, 1978, p. 35)

Thus, as he argued, "common sense" is not enough. One problem is that of the language used. Designers can try to make the actual wording clear and unambiguous, but even a limited practical experience showed that this is difficult. However careful a researcher thinks he or she has been in this respect, there still remains the possibility that respondents may either not understand the wording at all and so fail to answer the question or alternatively may answer it, but with an interpretation that

is different from the one intended or foreseen by the designer. (In passing this is an issue for all forms of written communication including a thesis).

Then there is the issue of the range of questions, and whether there is what Moser and Kalton termed "content validity". They defined this in relation to attitude scales as meaning that:

"The ideas should contain the common thread of the attitude under study, but between them they should cover the full range of the attitude and cover it in a balanced way."

(Moser and Kalton, 1971, p. 356)

This would seem necessary in general and not just in relation to attitudes, nor to survey research, but it is not easy in practice to ensure this. Also, there is the problem that if too many questions are given, the respondent may become bored, or frightened off.

Then there is the problem of the type of question, whether factual, or asking for opinions or attitudes, or reasons for actions. The factual kind are generally easier to formulate than any of the others, but even so there can be problems if the respondent cannot clearly understand exactly what is required. Even factual questions can have assumptions built into them, so that not enough alternatives are considered, again a matter of range.

Moser and Kalton pointed out that with respect to opinions, that there can be a number of aspects to any one issue, and a respondent may feel one thing in relation to one aspect and something else over another. This would seem to apply also to attitudes in general. Moser and Kalton, stated that because of this feature, opinion questions were "... more sensitive to changes in wording, emphasis, sequence", than factual questions. (Moser and Kalton, 1971, p. 317).

In respect of questions asking for reasons, Marsh, as noted, wrote of the idea that surveys have to be "adequate" at the level of both cause and meaning. It is one of the criticisms of surveys that they are not very good

at getting at respondents' meanings. Therefore this point is discussed. Marsh rejected this particular criticism, saying that survey designers had made considerable efforts in this area. She noted that there were many problems, and referred to Parson's model of action among others. She stated that:

"... there are severe difficulties both in getting actors to account for individual actions, and in discovering any broad goals that people are pursuing which could account for their actions. They are not always insurmountable, but they require very careful survey design."

(p. 109)

She noted two problems in particular. One was that actors might not necessarily be aware of their own motivations. The other was that answers to 'why' questions in particular could depend on what a person thought was wanted. She noted a view (Henry) that because of this, 'why' questions could not be asked.

In relation to the first point, Marsh observed that actors could know many of their reasons for action. When they attributed these to external constraints, this was a means of: "... preserving their experience of personal autonomy and ability to act rationally". (p. 107). She held that the existence of "unperceived constraints" did not "undermine" this point. In the present research, teachers did in fact refer to external constraints as reasons why they did or did not do certain things.

Marsh stated that on the whole actors were aware of their own motivations, though not necessarily of all the determinants of their actions. Thus, their reasons might be "adequate" in terms of meaning but not of cause. She stated that:

"Their reasons are often much better informed and are interesting in their own right, but have no logical status over any other explanation."

(p. 107)

Thus, neither in survey research nor 'interpretive', should actors' reasons be accepted at face value.

On the question of bias, Marsh argued that it could exist, but that it could be taken into account, and other methods used where there were difficulties in getting information from the actors themselves. This also holds for 'interpretive' research, as indicated in this chapter when discussing data collection and the issue of validity.

Marsh summarised her views about asking questions about meaning by saying that:

"... we cannot agree with Henry about the use of actors' meanings. Actors do know a great deal about why they behave as they do, and we must not eschew the insights they have. But we should treat them with circumspection. As with so many of the sweeping criticisms of survey research, this is better seen as a caution against asking 'why' questions inappropriately or badly, over-interpreting the results, rather than as a reason for abandoning the attempt."

(Marsh, 1982, pp. 107-8)

The comment about the need for care in asking for reasons applies to 'interpretive' research as much as surveys.

The general point that Marsh made about the question of meanings, that survey research was just as concerned with this as its critics, is an important one.

Apart from issues such as wording, range or type, another problem is the order in which questions are asked. This is important because if a series of questions invite a particular manner of response, this can introduce 'response set' bias. Order can also be a means of introducing checks against deliberate efforts on the part of respondents.

There is also the problem of whether questions should be 'closed' or 'open'. In the former, responses have to be made in a specified number of ways. This is done in order to be able to pre-code the questions, which makes for greater ease and a shorter time in analysing the results. The problem is that 'closed' questions can mean that some kinds of information may be missed, if the respondent feels that the categories given do not

really fit his situation or views. (This response has been felt by the researcher in answering questionnaires and was also a point made in the answers given by some respondents in the questionnaire she used). Hence, some questions are usually 'open', which means no fixed response and thus allows for the introduction of some 'qualitative' data. The problem here is that responses may include a mix of categories, or introduce totally fresh ones. This makes for greater difficulty in sorting, and thus takes more time. However, on the other hand, 'open' questions may indicate that a previously unconsidered variable might be important.

Thus, questionnaire design is full of problem areas, and in any or all of these there is the possibility of bias, with a consequent effect on validity. This is just as true, however, of 'interpretive' research.

Because of these difficulties, a careful survey usually contains a trial run or pilot of a questionnaire. This is an attempt to discover any problems in design before going to further expense with the whole sample, so that attempts can be made to iron them out. Marsh stated that many criticisms of surveys had arisen because of poor piloting, causing findings to be disputed and the 'survey method' to be discredited by opponents. (Marsh, 1982).

After questionnaire design, the next problem is how to administer it in the main study. There are two methods available, and both have pitfalls.

A questionnaire may be posted or delivered to the sample, to be filled in by the respondents themselves, or it can be administered directly by an interviewer who asks the questions and writes down the responses.

In the first case, one problem is that there is no control over administration of the questionnaire. Even though a covering letter explaining the survey may have been sent, and even if willingness to take part has been ascertained beforehand, (neither of which things may have happened) misunderstandings can arise. If there are any ambiguities still remaining,

or in spite of efforts the language, while clear, is unfamiliar to the respondent, or it is in any way not clear what is required, the respondent may fail to complete some or all parts of the questionnaire, or may give answers which are difficult to fit into the coding scheme, or which are unintelligible and/or illegible. All of these 'errors' can introduce bias into the final report. Thus, with a questionnaire not directly administered, there are many difficulties which can affect the reliance which can be placed on them, a point the researcher was well aware of when she was not permitted to administer her own in the main school of the research.

Any or all of these problems can affect the response rate, and as Gardner and Moser and Kalton, among others, have stated, the chief difficulty with a self-administered questionnaire is that the response rate is generally lower than with one administered directly. (Gardner, 1978; Moser and Kalton, 1971). This can affect validity, because the problem then is to decide how representative are those who actually reply.

For this reason, a more directly administered questionnaire is often preferred, since the response rate is usually much better, although this form is more expensive than a postal questionnaire.

This process shades from a questionnaire into the interview as a method, although in this case it is a 'formal' or 'structured' interview (terms noted when discussing the 'interviews' used in this research).

An interview of this kind uses the questionnaire as an interview guide, which is also termed an 'interview schedule'. This type of 'interview' is at the extreme formal end of the interview type continuum. It is the same for practical purposes as the questionnaire which is posted, except that the interviewer asks the questions and records the answers. Each interviewer is supposed to ask the same questions in the same order, and give the same explanations where needed. The intention is to make this form of interview highly standardised, so that the data gathered can be compared

more easily and not be prejudiced by interviewer bias. Therefore, interviewers are usually trained for the particular survey, and supervised.

One advantage of this type of 'interview' is that, with someone there to explain what is required, there is less likelihood of questions not being answered due to misunderstanding on the part of the respondent.

Also, like the postal questionnaire itself, it is a means of getting information from a number of people relatively quickly.

However, because even a formal interview involves face-to-face interaction, even of a minimal kind, there is always the possibility of bias. Marsh pointed out that this could exist on the part of either the interviewer or the respondent, or both. (Marsh, 1982, p. 107). McNeill pointed out that:

"All researchers have to think carefully about whether the interviewer's sex, social class, race or accent will affect the answers that are given. No amount of training can overcome problems like this."

(McNeill, 1985, p. 36)

In other words, the perceptions of both parties to the interview of each other can affect the data, and it is difficult to foresee everything of this kind of interactional problem.

Another factor which would affect the data is that an interviewer could misunderstand the response to a question without realising it, and so code it wrongly, or alternatively the answer could be written down illegibly when in a hurry and so be marked as unrecorded.

Where little or no training is given, the problems are likely to be worse.

Apart from the highly formal interview, a survey can use more informal types, such as the 'guided', where there may be a list of questions to be asked, but the order is left in the hands of the interviewer, or there may be a number of topics to be covered, but the actual questions

may be more general. Or some interviews of this kind may focus on a particular topic.

One advantage of the slightly less formal approach is that more information may be produced, where a respondent is able to expand on his views. However, standardisation is more difficult, and sorting and analysing the results is accordingly more time-consuming and expensive. Also, there is even more possibility of bias, with the interaction becoming more personal. Further, the more an interview departs from the formal, the greater the skill and experience required by the interviewer.

A survey may also use direct observation either as a supplementary check on interviewee statements, or as a means of deciding what questions might be included in a questionnaire.

Gardner stated that:

"unless one is measuring opinions and attitudes on topics and in populations with which one is already familiar, observation of some kind is essential in the exploratory phase of the survey."

(Gardner, 1978, p. 31)

However, even direct observation can introduce bias, because people not only see something but also interpret what they see in the light of their own experience. Thus two people can observe the same features on events but perceive them differently. Also, there is the problem that observations have to be recorded. This can be difficult in some circumstances, if the observer is trying to appear 'invisible'. If a 'schedule' is used, the categories have to be wide enough so that details which might be important are not unduly restricted or excluded.

'Participant' observation can also be used in a survey, something the researcher had not known originally. This, like observation, is used either as a supplement to interviews or questionnaires, or as part of an exploratory study.

Since 'participant observation', including less formal interviews, and the use of documents, with some use of historical methods, was used in the present research, they are fully discussed in the section dealing with the methods of data collection used in this research. Hence, they are not discussed further here. The point is, though, that 'qualitative' methods are not confined to 'interpretive' research, as 'quantitative' are not confined to 'positivistic'.

Where a survey uses less formal methods, with greater freedom being left to interviewers, the problems with validity and reliability are likely to be greater. However, when such methods are used, the survey is likely to gain in depth. Moser and Kalton noted this, in observing that:

"Sometimes good judgement requires the deliberate sacrifice of quantitative precision for the greater depth attainable by more intensive methods of attack."

(Moser and Kalton, 1971, p. 3)

Greater depth is often claimed for 'interpretive' methods as against the survey's greater spread of surface detail. However, where a survey uses a range of methods, including more intensive ones, it might seem to have the best of both worlds.

However, generally speaking a survey does not extend for as long a period as 'interpretive' research. An important exception to this is the long-term survey such as that undertaken by Douglas (1964, 1968). These produce panel data over time, and thus allow for follow-up and extended comparison and checking of the results of the original survey.

But where a survey is not of this kind, there can be difficulties where time is short. One of the problems is how far to seek to re-interview respondents to check on statements, or how many attempts should be made to check on people who did not respond or were not at home. Where money is a major factor, the time and expense involved in chasing up people may have to be limited. This again raises the problem for validity of how representative are the non-respondents against the respondents, in

terms of the wider population, or how valid are some of the data, where statements by respondents seem questionable.

'Interpretive' research, being generally more long-term, is seen as giving more opportunities for this kind of follow up.

Thus, surveys as a method, or rather a range of methods, have both advantages and disadvantages, like any research.

This part of the sub-section has discussed the idea that 'quantitative' methods belong to 'positivism', while 'qualitative' are assigned to the 'interpretive' approach. It has indicated that such a division is not accurate, since surveys at least can involve the use of 'intensive' qualitative methods, and there is nothing in the 'interpretive' approach to prevent the use of quantitative procedures, even at the minimal level of counting.

The concepts of validity and reliability have also been defined and discussed, since the former is of concern to all forms of research and the latter also, since it may have some relevance even in interpretive research.

The methods claimed to be associated with 'quantitative' research and so to 'positivism' have also been considered. It has been indicated that both research 'traditions' may use a similar range of overall activities, such as comparison, a case study, or the historical method. In terms of practical activities, it has been stated that experiments are in some ways more like the image of 'science' and hence of 'positivism', although pointing out that they are not necessarily either held in a laboratory nor quantitative, though they are more likely to use the latter techniques than surveys. However, the existence of ethical problems has been noted.

Surveys have been shown to cover a range of methods, ranging from

formal questionnaires to participant observation, and some of the problems involved in questionnaire design and administration have been discussed. As noted, there are objections to the association of surveys with 'positivism', and also to the idea that they are not concerned with the 'meanings' of actors.

This discussion of the methods associated with 'positivism' brings to an end the issues with which this sub-section has been concerned.

Overall, the sub-section has explored the ideas of 'positivism' and the association with 'the scientific method'.

In the course of doing so, it has been discovered that 'positivism' is an "ambiguous" term. It has been used to refer to different things at different times, and to different levels of discourse, the ontological, epistemological and methodological.

Because of the differences the development of 'positivism' was traced with reference both to periods and levels of meaning. In doing this, it was discovered, particularly with reference to Durkheim, but also in relation to 'structural-functionalism' and 'logical positivism' why 'positivistic sociology' came to be associated with 'holism', objectivism and the advocacy of 'the scientific method'. However, it was also found that there were possible links or "convergences" between Durkheim's idea of 'representations' and the idea of consciousness in phenomenology, however unacknowledged. It was also observed that, even if 'structural functionalism' became the dominant research tradition in Britain and America until the sixties, it did not follow that all working within it had exactly the same concerns as Comte and Durkheim.

In discussing the development of 'positivism', it was also observed that some of the overall 'methods' advocated by Comte and Durkheim, such as 'classification', 'the comparative method' with the use of 'concomitant

variation', were also to be found in the 'interpretive' tradition as well as in science itself, as was the basic concept of causal analysis. In discussing Durkheim's view that explanation was monocausal, the basic problem of causal analysis was raised, and referred to again.

In discussing the epistemological aspects associated with the development of 'logical positivism, reference had to be made, in the course of following up some references by Hammersley and Atkinson amongst others, to debates within the philosophy of science about the nature of scientific theories, with particular reference to the views of Karl Popper and the problem of 'falsification'. The issue of whether a 'value-free' science was even possible was raised, given the lack of a 'neutral' language to discuss competing theories. It was found that some scientists rejected this possibility, because values, or subjective impressions, might enter into the choice of theory or the evidence used. It was also found that some scientists also rejected the idea of a scientific method, while others went further and considered that 'anything goes'. Altogether it was found that if 'positivism' was ambiguous, then so was the term 'scientific method', so that the equation of the former with the latter was at best inexact and at worst misleading.

In discussing Popper, the issue of whether he was 'positivist' was raised, because of his view that there was a unity of method between the natural and social sciences if the 'hypothetico-deductive model' was used. This model was equated with 'positivism' in many texts. It was noted that Comte and Durkheim used induction, as did some but not all logical positivists. Thus, the equation of 'naturalism' (which was shown to have two diverse meanings) with induction and the 'interpretive' approach, and that of 'positivism' with deduction was queried.

Similarly, so was the relationship between 'quantitative' and 'positivist' and that between 'qualitative' and 'interpretive'. This

dichotomy was found to be not absolute, since both 'traditions' could use both 'methods' in some form.

As indicated, it was found that at the general level both research traditions might use a similar range of activities. At the practical level, some differences were found, but less than this researcher had originally thought, particularly in respect of surveys, and the concern for actors' meanings.

Overall, as stated at the beginning, this sub-section was part of a learning process, reflective in nature. As such, writing it was in the nature of a 'voyage of discovery' for the researcher. In the course of this many misconceptions had to be revised. In particular, any notion that there were rigid distinctions or clear-cut dichotomies between the two traditions had to be abandoned. As observed at the beginning, these ideas may be 'old hat' now, but were not so for the researcher.

The following sub-section considers the 'interpretive' approach.

2. Interpretive Sociology

The intention of this part of the section is to set out the basic ideas about the relationship of individuals to 'society' and the task of social researchers which can be subsumed under the general heading of 'interpretive', since it was within this general approach that the research was considered and carried out.

The various 'perspectives' considered here could also be called 'hermeneutical', since they are concerned generally with 'understanding' the intentions of actors.

With some, though not all, of these approaches, the theoretical concepts within them were developed in reaction to 'naturalism' in the sense of the concepts and methods associated with the 'natural sciences'. This is particularly true of phenomenology. However, methodologically, most have

been linked in texts with opposition to 'positivism', in that they have been 'naturalistic' in the sense of living in 'the natural world' with the objects of social research. However, the main aim of this part of the section is not to discuss opposition to 'positivism' as such, but to set out the theoretical ideas within these 'approaches', and to indicate those which were of importance for the research in some way at some point, either at the beginning in terms of the general approach, or during it in terms of ideas which seemed to be illuminating for the accumulating data, particularly during the later analysis and writing up stages.

As noted, in the introduction, there are difficulties in discussing an 'interpretive' approach. This is because there are many strands which could be placed under this heading, each of which has its own origins and problems. This is in contrast to 'positivism' which, although 'ambiguous' and with its own internal differences and developments is still, basically at least, one strand, not several. As noted, there were many issues involved in discussing 'positivism', and doing so entailed some length because these issues were complex. The various different 'approaches' which could be classed as 'interpretive' have similar complexities. Therefore, with several strands, it is impossible to discuss them all, either equally or at such length as with 'positivism'.

Also, the concern with what was seen as most relevant to the research led to a decision to omit certain theoretical ideas. One of these was existentialism as a philosophy which was seen as merging into, or being very similar to, phenomenology, which is discussed.

The other omission is the 'critical theory' of the Frankfurt School and its 'descendant', Habermas, although this is a strong critique of 'positivism', and members of the early school, such as Adorno, debated with Popper. The concern with re-interpreting the 'structural' or 'positivistic' aspects of Marxism was seen as falling outside the main concerns of the

research and this section. Also, some of their criticisms of 'positivism' are similar to those of other critics. For example, if science is recognised by scientists as not being value-free, then by definition sociology based on 'scientific principles' cannot be either, and the issue of values was discussed in the first part of this section. However, the idea of Habermas that different 'cognitive interests' or forms of knowledge, two of which he termed the 'technical', which belonged to the 'empirical/analytic sciences' and the 'practical' which he considered to be the sphere of the 'historical/hermeneutic' forms of enquiry, are inter-related, seems both correct and important. So does his view that in modern life 'technical' knowledge may have assumed undue prominence. Also important is Habermas' acknowledgement that changes in material conditions are necessary for the development of co-operative understanding.

Habermas' concern with 'self-reflection', symbolic interaction and 'communicative action' could be seen as placing him within the 'interpretive' approach in some respects. Also, in reinterpreting other thinkers, he discussed Weber's concept of rationality, and Weber partly falls within this 'tradition'.

However, the idea that such self-reflection and communication could develop into a critical understanding of political reality, even with any breakdown in consensus which might seem to be happening in present time and hence into a possible emancipatory transformation of society, seems problematic. While the aim of developing a critique of social and political theory is both interesting and important, it again was seen as lying outside the immediate concerns of this research. (Bernstein, 1979, pp. 174-225; Frisby, 1974, pp. 205-224; Lally, 1976, pp. 62-67; Pusey, 1987). The decision to omit ideas is, of course, based upon a value-judgement which may or may not be valid. Nevertheless, one is sometimes seen as inevitable.

Given these omissions, this part of the section notes the different

origins of the various strands within the 'interpretive' tradition, and picks out the general ideas associated with these, before making some comments on the methods usually attributed to this 'approach'. When discussing these, again only the principal ideas are mentioned. This is because, since the research utilised such methods, they are discussed more fully in relation to topics such as gaining access, data collection and analysis, and so on, which are the subject of later sections of this chapter.

Before stating these strands and discussing them, the general line of the 'interpretive' critique of 'positivism' is set out.

This was that the subject matter of the social, or 'human' sciences was qualitatively different from that of the natural. Winch, for example, recorded the view that the concepts involved in studying human society were 'logically incompatible' with explanations in other branches of science. (Winch, 1958). Social scientists, like those in the natural sciences, had to examine the phenomena under investigation, but these were seen as part of a 'social' activity, and were thus subject to human purposes and meanings. It was this, it was argued, that rendered a different approach necessary. The view was that the social scientist had to enter imaginatively into some kind of 'understanding' about the meaning of the phenomena to be found within the social activity being studied. (Winch, 1958).

It is from this viewpoint, as summarised above, that the social sciences are seen as having to take account of the 'subjective' meanings of individuals in social settings, including the observer as part of this setting. A social science researcher is not seen as 'out there' from the people being studied, because as an ordinary member of 'society', he or she will have values and biases just as much as any other member. Thus the idea developed that a researcher's account was only one possible explanation of the phenomena under observation, and not necessarily a superior one. Thus, from the 'interpretive' standpoint, the researcher's own efforts to make

sense of the situation, the meanings he or she attaches to actions, his own or others, become part of the matter under investigation. As noted in the previous section, however, the ideas and interests and value of natural scientists are also recognised among some practitioners as also possibly influencing investigations in their fields. From the 'interpretive' standpoint, a 'value-free' social science is seen as an impossibility. The 'values' have to be recognised and taken into account when assessing validity. Conventionalist natural scientists, however, make the same point. Also, as noted, some practitioners of what have been classed as 'positivist' methods are also concerned with the meanings of actors, and acknowledge that bias can exist in both the researcher and the researched.

The idea of studying the 'subjective' meanings of social actors, including researchers, is that social situations, or 'structures', are largely shaped by the interactions of those partaking in social relations, which over time build up such structures. In the course of such relations, shared interpretations, a form of 'collective consciousness' of 'commonsense' taken for granted assumptions are built up, and it is these which make social relations possible in the future. This can be thought of in terms of Kuhn's 'paradigm'.

Because people are seen as actively 'making sense' of situations and relations with others, and acting in the light of their interpretations of these, the task of the social researcher according to the 'interpretive' view is seen as 'understanding' how they do this, as well as him/herself interpreting events. It is considered that such 'understanding' can only be achieved by ascertaining the meanings that actors themselves attach to events. This is seen as requiring some first-hand direct participation in the social situations of the people being studied. As noted in the first half of this section, such participation can also take place in other forms of research. It has also been indicated that Popper, at least, rejected the view that such 'understanding' was the prerogative of social scientists,

and also that 'explanation' as well as 'understanding', was not seen as totally irrelevant by all 'interpretive' researchers.

These ideas, and the overall view that social individuals create 'society' rather than the other way round, have, as noted, many sources, and these have some differences even if they have some ideas in common.

The various 'perspectives', with the omissions as stated above, which were seen as part of the 'interpretive' approach and relevant to this research, are discussed next. They include the 'social action' perspective or the 'verstende', the symbolic interactionist, the anthropological/ethnographic, the phenomenological and the ethnomethodological. Each has its own origins, although there are cross links.

This part of the section therefore examines these perspectives in turn, which together form part of the 'broad church' of interpretive sociology. This, in contrast to models of society which might be considered 'holistic' or 'macro', concentrates generally on individualistic 'micro' situations, although not necessarily being either ignorant of structural issues, nor unable to deal with them.

The 'social action' perspective is considered first, as part of the work of Weber. Weberian concepts were found useful for certain aspects of the research.

The idea of 'social action' came from Weber. His view was that social institutions should not be reified, for only persons, not things, acted. This view was partly developed in opposition to both Durkheim and Marx. Action, for Weber, was:

"social in so far as, by virtue of the subjective meaning attached to it by the acting individual (or individuals) it takes account of the behaviour of others, and is thereby oriented in its course."

(Weber, M., in Coser and Rosenberg, 1982, pp. 191-2)

To study the 'subjective meanings', Weber developed the concept of 'ideal

type'. Outhwaite noted that this was not Weber's invention, but it was distinguished from its sources by the "peculiar freedom" Weber allowed in its construction. Outhwaite stated that Weber's 'ideal type' was:

"grounded in a radicalised (because subjectivised) version of Rickert's notion of value reference; this links together the influence on the social scientist of general cultural values and his or her more personal and pragmatic concerns in a given piece of research."

(Outhwaite, 1987b, p. 101)

So Weber's 'ideal type' was an invention, created in order to discover causal relationships by a form of analogy, not a reference to real people. Weber thus sits uneasily on the divide between 'holistic' and 'individualistic' interpretations of 'society'. His work could be taken, and was taken by Parsons, as noted in the first part of this section, to support 'structural-functional' approach.

Weber, on the basis of ideal types, typified '*orientations to action*', for example, in terms of its degree of rationality, as; rational in relation to a goal, to a value, or whether affective or traditional. ('Critical Theory', as proposed in Habermas, was concerned with the dominance of the first concept of rationality, the instrumental or 'zweck-rational', concerned with control of resources, work and survival).

Weber argued that as social actions involved social relationships, they tended to require predictability and regularity. This view led Weber to discuss the problem of order and to develop the concept of 'legitimate order'. Weber then typified this in terms of the social actors' motivations to obey it. These motivations were, affectual, rational in relation to values, based on religious values, and based on self interest, through expectations of consequences. (Weber, 1964). 'Legitimate' use of power was also typified as, charismatic, traditional or legal (rational).

Hence, social action, rationality and legitimate order and the use of power are linked concepts in Weber's work.

However, it is not clear why social action should be linked with rationality necessarily. Pusey considered the relationship to be "confused and unsystematic", when referring to Habermas' reconstruction of Weber. (Pusey, 1987, p. 29).

The idea of legitimate use of power led Weber to develop the concept of bureaucracy, linked to rational action and legitimate authority. (Weber, in Coser and Rosenberg, 1982, pp. 327-335). This last term provided a useful reference when considering the role of the head teacher in infant schools, because it was something actually referred to. Heads had a clear notion of their position in this respect, something referred to in Chapter Four.

Weber's view of 'society' contrasted with that of Marx and also of Durkheim. He considered that the basic units were 'status groups', which had a common culture and which formed the main source of identity for their members. These 'status groups' had differential 'social honour' or prestige. His idea was that these status groups could be related to an economic position - or social class - or to differential access to power (political parties), but also to religion or region. Thus a 'status group' was not solely a matter of social class, based on economic or material factors, and in this he was in opposition to Marx, who, as noted, saw 'society' as composed of two groups with different relationships to the means of production.

Weber considered that the various 'institutions' of society were the site of competition between status groups, for the scarce resources.

Education was one such institution. Weber considered that status groups asserted educational ideologies which represented group interests. A status group could gain power and try to institutionalise its ideology, and could achieve domination, or authority, through monopoly and the use of constraints, and a status group, having become dominant, could then resist

other groups by the use of 'defensive' ideologies. In Chapter Three, where 'ideology' is defined, the idea of its use as serving to promote, which includes to defend, group interests, is given in a definition by Watson. (Watson, 1979). In the course of the research, teachers were heard to claim 'professional' knowledge, particularly in relation to parents. It seemed to the researcher that this could be seen as a form of defence mechanism, particularly, though not only, at Moorland.

Also, the idea of 'status groups', together with the idea of these having different 'cultures', came to mind after having observed the clash in values between teachers and their views of pupils and their home background at Moorland.

Although certain Weberian concepts were found relevant during the research, his version of subjective social action was seen as less so. It was easy to understand why his idea of a 'social actor' was taken over by Parsons, who first used the term 'action frame of reference'. He followed Weber's concept of orientations, and used it to build the theory of 'role', as behaviour attached to a particular position or 'status', which involved the 'role expectations' of other social actors. Social interaction confirmed these, and so built into a system. Parsons saw action as 'explained' if shown to be 'system maintaining', hence his 'functionalism'. It was noticeable that the term 'role' was used by most head teachers when discussing their position, and sometimes by other teachers, and was used by heads in relation to their 'status'.

Although Weber's use of an 'ideal type' to 'understand' the meanings of social actors seemed unsatisfactory, his advocacy of 'the comprehensive method' seemed relevant, and his idea that 'explanation', in causal terms, should be complemented by 'understanding' of meanings, also did. (Freund, 1979, p. 164). As noted, Popper rejected the idea that 'understanding' was not part of science.

In terms of causality, Freund commented that Weber, along with Pareto, introduced the concept of multiple causality. Freund stated that:

"the essence of his critique is aimed at the difficulty, often the impossibility, of finding a single antecedent cause for a social phenomenon, as causal monism would have it. In general, an event is explained by a plurality of causes, and it is up to the researcher to appreciate the weight of each one."

(Freund, 1978, pp. 169-170)

As noted when discussing causal analysis in the first part of this section, this is an idea which is taken for granted by most sociologists today, something Freund also remarked on. In this, Weber stands in direct contrast to Durkheim who, as noted, firmly rejected the concept of multiple causality.

Weber has been considered in the 'interpretive' section because of his stress on the need for 'understanding' the subjective meaning of social actors, as well as because other concepts, stemming from his view of 'social action', were found to be relevant during the research. However, because the means of such understanding was the construct of an 'ideal type', Weber does not wholly fit into the 'tradition'. (Bendix, 1966; Gerth and Mills, 1967). However, his views influenced Schutz's phenomenology, as will be indicated later.

Other 'verstehende' theories concentrate more directly on individuals in interaction with others, and the individual meanings such 'social actions' attach to their actions. One of these is the next 'perspective' within the 'interpretive tradition' to be discussed, which is 'symbolic interaction'.

Symbolic interaction is a general term covering a number of ideas which have various sources. Cuff and Payne said about it that:

"It can be argued that Symbolic Interactionism is not a unified perspective in that it does not represent a common set of assumptions and concepts accepted by all who practice the approach.

(p. 89)

Nevertheless they argued that several "characteristics" could be identified,

which made the "approach" "distinctive". (Cuff and Payne, 1979, p. 89).

It is associated with the 'Chicago School', and with ideas shaped by Mead. However, some of the basic ideas were stated first by Charles Cooley, with whom Mead worked at the University of Michigan. Mead was an instructor at Michigan when Cooley was there.

Coser stated that Cooley was influenced by Darwin and biological inter-relationships, and by William James. From the latter he developed a view:

"... of the mind as forever changing and expanding in terms of novel experience but also, the notion of selves being constructed through a variety of transactions with the outside world."

(Coser, 1979, p. 307)

Coser noted that another influence on Cooley was that of "a social psychologist", Mark Baldwin, who, having studied children's development, considered that:

"the personality of the child can only be studied in social terms."

(p. 307)

From these ideas and others, Cooley developed his theory that an individual "self" could not be known apart from the selves of others in "society". The way in which a person came to see himself was said to be his 'self-concept', and this was regarded by Cooley as the outcome of social 'interaction' with others. The 'self' thus could be:

"defined as a "looking-glass self", because it is reflexive and arises in the person's consciousness through an incorporation of the views of others in a process of communicative exchange."

(p. 308)

Coser also stated that Cooley's concern with "the organic relations" of the individual 'self' and 'society' led him to a consideration of the importance of "primary groups" for the emergence of 'community' feeling, because in these groups individuals were seen as linked together "by sympathy and affection" and because of this could therefore gain a sense of something other than themselves, 'society' on a small scale, in effect. Thus, in Coser's words:

"There is no looking-glass self without primary groups and without a community."

(Coser, 1979, p. 308)

Cooley's ideas were taken further by George Herbert Mead. However, Mead had other sources for his theory of social interaction. At Harvard Mead studied psychology and philosophy. Ryan stated that two of his teachers in the latter were Josiah Royce and William James, and that of the two it was Royce whose ideas most impressed Mead at the time. (Ryan, 1977, p. 569). Royce himself was influenced by Hegelian ideas on the nature of the mind and its relation to reality, and his concept of 'absolute idealism'. This last was the view that everything existing was a form of one 'Mind'. Hegel's 'absolute mind' was the highest form of thinking, found in philosophy. Royce developed his own form of 'absolute idealism'. One of the ideas put forward was that to be able to think of "an orderly continuous world" required the assumption that there existed an:

"Absolute experience to which all facts are known and for which all facts are subject to universal law."

(Royce in Flew, A., (ed) 1984, p. 308)

Another concept was that of an idea having both an "internal" and "external" meaning. The former meant the purpose the idea fulfilled and the latter, or 'cognitive' meaning, was derived from this purpose. (p. 308).

William James' philosophical theory was 'pragmatism', founded by C. S. Peirce. The principal concern of this was with a method of finding the meaning of concepts, and establishing the truth of ideas. It contained the views that:

"one's idea of anything is the idea of its sensible effects distinguished according to their practical significance"

and that an idea was 'true' if "responsible investigations" agreed on it after examining it closely. ('Philosophy', p. 264).

James developed Peirce's theory. He is said to have claimed:

"that all metaphysical disputes could be either resolved or trivialised by examining the practical consequences of

alternative answers ... scientific theories are instruments to guide future action, not final answers to questions about nature."

and in questions of "religion and ethics" it was sometimes possible to choose between alternative hypotheses by following "inclination" if 'rational' choice was impossible. Also, the truth of an idea depended on whether it had "fruitful consequences". ('Philosophy', p. 184).

From both of these philosophies Mead developed later his related concerns with both an ethical view of the way individuals were attached to society, and of the relationship between 'mind' and 'nature'. But ideas on the latter subject were influenced by Mead's studies in Berlin, where he also studied psychology. Here he encountered the work of Wilhelm Wundt, as well as discovering "the impact of Darwin on social thought". (Ryan, 1977, p. 570). Wundt's work was concerned with "the role of *gesture* in communication, both among animals and men". (p. 570). It was this work Ryan stated, which was the starting point for Mead's conception of the development of "language and intelligence" in society, from an interest in this social significance of gestures. (Ryan, p. 570).

After Berlin, Mead went to the University of Michigan, where he met Cooley and his theory of the relationship of self and society, and Dewey, who was also pragmatic in philosophy, as well as a psychologist and educationist - one of the sources of 'progressive' ideas in education. As a philosopher, Dewey built on the ideas of both Pierce and James, to produce an "instrumentalist" view of the nature of "thought, logic and the acquisition of knowledge". ('Philosophy', p. 93). This is said to involve the idea that:

"Ideas, concepts and judgements are instruments functioning in experienced situations and determining future consequences."

(p. 174)

It was experience that tested the adequacy and truth of propositions and judgements. Hence Dewey's concern in education with practical experience in the development of conceptual thinking in children.

When Dewey left to head the departments of philosophy, psychology and pedagogy at the University of Chicago, Mead went with him, and it was there that his theories were synthesised into 'interactionism', which was basically a concern with the relationship of 'mind and nature', developed from philosophy, linked to a view of the importance of communication for the development of social action.

Mead's argument on the 'mind/nature' relationship was that there was no "sharp separation". The development of the mind requires the acquisition of language, and this itself depended on the "physiological development of the human organism". (Cuff and Payne, 1979, pp. 91-2).

It was the way the human mind developed that made the response of humans to 'communicative gestures', and hence action, differ from that of animals. The action of animals was basically a matter of stimulus-response, or reflex actions, an "automatic association between the circumstances of behaviour and the behaviour itself". (Cuff and Payne, 1979, p. 90).

The ability to acquire language, a 'symbolic universe', made human behaviour more than this, through social interaction. A social act, as Mead developed Cooley's concepts, involved the interpretations of the meanings attributed by individuals to the gestures/behaviour of others. By reason of their ability, through thought and language, to exist in this 'symbolic universe', social actors could make 'objects' of themselves and their actions, and interpret these, and could reflect upon others' reactions to their actions. In this way a concept of 'the generalised other' could be built up. Mead noted that childhood was an important stage in this process. Distinguishing between 'play' and 'games', he considered that in the former, behaviour might be initiated by a child on his own. 'Games' involved interaction with others, and to participate successfully meant understanding the viewpoints of others, and anticipating their

behaviour. Thus 'games' were a form of social integration. (Ryan, 1977; Cuff and Payne, 1979).

The meanings constructed in this and other forms of social interaction helped to shape the 'self-perceptions' of an individual actor and influence his future actions. That is, an individual came to interpret how he or she thought others saw him or her, moving from the subjective to the objective, from the "I" to the "Me", in a reflexive process which also involved his or her perceptions of others, so that human social behaviour was not automatic, as in the 'stimulus-response' system attributed to animals. Individuals, as social beings, possessed, through the 'symbolic universe' a 'disposition' to make what they saw as an 'appropriate response', as they interpreted the 'gestures' of others, and there was nothing 'fixed' as to what this might be.

Coser observed that in seeing him/herself as an object, the 'me', and in reacting in terms of the attitudes of others there was something very similar to Durkheim's concept of internalisation going on. (Coser, 1979, p. 310). Also Ryan, commenting on Mead's ethical theory, derived from pragmatism, said that this "straddled the gap between utilitarianism and Hegelianism". (Ryan, 1977, p. 571). Mead did not accept that "impulses were satisfied for pleasure" as in utilitarian theory. According to Ryan:

"For him, the sequence-impulse perception, manipulation, consummation, - was self-explanatory."

(p. 571)

But actions could be rational or irrational according to whether an impulse could be satisfied on "the largest scale", or whether satisfying one impulse prevented doing so with another. But Mead held, following Kant and Hegel,

"that a genuinely ethical standard, as opposed to the standard of individual prudence, had to be universal."

(p. 571)

From his views on the symbolic understanding of meanings, and the 'generalised other', and his views on ethics, Mead developed the concept of the attachment of individuals to the "moral ambitions of the society", through taking on, "the desires of the generalised other". According to Ryan, from Mead's viewpoint,

"Religion expresses this attachment of individuals to the demands of society in an ideal form."

(p. 571)

This is also very close to Durkheim's understanding of religion.

As stated, Mead worked in the departments of philosophy and psychology. In the latter field he described himself as 'behaviourist', but he did not mean by this what Watson later came to mean, purely stimulus-response, as noted. Rather, his 'behaviourism' was 'social', taking account of inter-subjective understandings. However, as Coser stated, this did not mean that his theories were totally 'subjective'. Coser commented that:

"Mead remained steadfast in his social objectivism. The world of organised social relationships was to him as solidly given in inter-subjective evidence as the physical world. To him, society is not a mental phenomenon but belongs to an "objective phase of experience"."

(Coser, 1979, p. 311)

'Social behaviourism' meant that: "having an individual self was the result of social processes". (Ryan, 1977, p. 570). These 'social processes' could be observed. Ryan stated that, for Mead,

"the psychological unit of inquiry was the act. That is the basic unit is a completed action, not the components of such an action."

(Ryan, 1977, p. 570)

These acts could be studied. Cuff and Payne commented that the term 'behaviourist' for Mead meant not "subject matter" but "rather his manner of proceeding". (Cuff and Payne, 1979, p. 92). This meant that Mead's account was:

"built upon the observation of the ordinary activities of social life, upon publicly available and commonly observable facts"

(p. 92)

Cuff and Payne observed themselves that if "the hallmark of science" was not what was studied but rather a matter of "public availability of its observations", then Mead's 'behaviourism' could be seen as 'scientific'. (p. 92).

They stated that their account of Mead appeared to contradict the view that 'symbolic interactionists' consider that 'scientific methods' cannot be applied to sociology. As they stated, and as was argued in the previous section, such a view depends on what is meant by 'scientific method'.

Mead developed his concept of a 'symbolic universe' and his view of social interaction in the course of his work in the philosophy and psychology departments of Chicago, as noted. However, there were those in the department of sociology there at more or less the same time who had similar philosophical interests and a concern with group interaction. Robert Park and William Thomas (until 1918) were two of these, and their influence was such that Fisher and Strauss stated that there appeared to be two interactionist traditions, one stemming from Mead and one from Park and Thomas, although there were links between the two. Some 'interactionist' sociologists drew on both 'Chicago' traditions, while others inclined more towards one than the other. (Fisher and Strauss, 1979, p. 458).

This is one reason why Cuff and Payne regarded 'symbolic interaction' as not being a single perspective, as they were noted as stating. However, another reason why it is not was indicated by Williams, who wrote of the division into two sub-schools, that of Chicago and Iowa. (Williams, 1976, p. 120). The differences between these two are discussed later.

Park and Thomas' influence can be gauged from the fact that they "singly or jointly helped train most of the second generation" [of Chicago sociologists]. (Coser, 1979, p. 313). These included among many

others Herbert Blumer and Everett Hughes, and these in turn influenced others, such as Howard Becker. Fisher and Strauss included Erving Goffman as an interactionist in the Park Thomas tradition, and Hargreaves also considered him to be an interactionist, though not "typical or representative". (Fisher and Strauss, 1979, pp 459-60; Hargreaves, 1978, pp. 8-9).

Strauss, together with Glaser, may also be seen as following in this tradition, Glaser by association. (Fisher and Strauss, p. 487).

Both Park and Thomas were concerned with both the understanding of social change, seen as evolutionary, and with how this might be directed so that such change was 'progressive'. These concerns seemed pressing in the circumstances of turn of the century America, with the increasing spread of urbanisation and large scale movements of people, both from rural areas to the cities and from Europe to America, and the consequences for such movement on group values and attitudes.

Both saw the relations within groups and between groups as crucial in the process of change, and the problem for study that of how relations between different groups could move from conflict to co-operation. Both saw individuals as potentially 'creative' and thus able to change, but also both recognised that there were real social constraints which could include, apart from economic factors, entrenched and inherited group attitudes. Park, taking the idea of society as communication from Dewey, and the idea from William James of people being 'blinded' by 'masks', and believing in a democratic community, considered that by finding the 'news' and telling it to people, sociology could give them the knowledge to escape from traditional attitudes and group conflict. Thomas also believed that understanding the constraints which affected people's views was important, with the need to study the social psychology of the relations between individuals and particular contexts. Hence, for

Thomas, 'interaction' could not be simply subjective meanings, since these were always related to social contexts.

Park also saw sociology as the study of "the big picture" (Fisher and Strauss, p. 470). This meant knowing what was happening in the world, which required:

"a wide scope of knowledge of the world, an acquaintance with various sociological and other relevant theories."

(p. 470)

Such knowledge then had to be communicated.

Both Park and Thomas considered that the spread of rational knowledge was essential for social change. However, Park saw this knowledge as needing to be communicated to people in general - an emerging "democratic community", while Thomas considered that social reform required the knowledge to be given to an educated elite.

Park on the whole was less optimistic than Thomas about the likelihood of social reform, particularly in the ability or inclination of 'leaders' to direct this. (Fisher and Strauss, 1979, p. 457-475).

Both Park and Thomas considered that the task of the sociologist was to explore the lives of individuals, and discover what factors lay behind the decisions that they took in the course of these, and what therefore caused attitudes to harden or change. Park's interest in race relations and life in urban, often ghetto, communities, led him to see the city as a laboratory. Also, Thomas was interested in the way people adapted to changes in circumstances, particularly the subjective definitions they made of their objective situations. Hence his statement that: "If men define situations as real, they are real in their consequences". (In Coser, 1979, p. 315).

Given this concern with 'real lives' in the context of social change, and Chicago's actual position at the centre of a developing area, Park

wanted:

"above all, to train scholars who would be able to explore the social world, and especially the urban scene"

(Coser, 1979, p. 316)

Hence, whether by "design" or "adaptation", what happened was that:

"fieldwork oriented studies, mainly in Chicago, became the hallmark of the department's contribution."

(p. 312)

The concept of 'fieldwork' was borrowed from anthropology, which is discussed later in this section. Burgess recorded Park's comment that:

"The same patient methods of observation which anthropologists like Boas and Lowie have expended on the life and study of the life and manners of the North American Indian might be even more fruitfully employed in the investigations of the customs, beliefs, social practices and general conceptions of life prevalent in Little Italy on the Lower North side in Chicago, or in recording the more sophisticated folkways of the inhabitants of Greenwich Village and the neighbourhood of Washington Square, New York."

(Park, 1982, p. 6)

Thus, Chicago sociology became associated with studies of life-styles of individuals or particular institutions as well as particular social groups. Burgess, like Coser, noted the effect of Park's journalistic training, which led him to propose the use of unstructured interviewing and observation. However, Burgess also noted that Chicago sociologists used various other methods, which included "surveys, documentary evidence and statistical data". (Burgess, 1982, p. 6). Burgess also stated that in fact one of the characteristics of the Chicago School was this use of a variety of methods, "which combined observational materials with different types of documentary evidence." (p. 7). He added that the result of the various methods was:

"urban ethnography based on highly detailed description studies of natural areas, institutions and individuals in the city."

(Burgess, 1982, p. 7)

'Ethnography' is another term borrowed from social anthropology.

The idea of studying life histories as part of the research was in accordance with Thomas' view that to understand an individual's attitudes to

situations, or "institutional conditions" in the present, their "inter-nalised attitudes" or results of past interactions, had to be studied.

This:

"implied investigations into people's past as well as their presents, but past and presents viewed contextually."

(Fisher, and Strauss, 1979, p. 469)

This seemed to the researcher a useful concept in relation to ideas about infant education which might affect present practice, but also have a past history. Ideas, which affect attitudes, also need to be 'viewed contextually'.

The researcher would also have liked more information on the 'life histories' of the teachers seen, particularly at Moorland, but for various reasons this was difficult to come by. The 'descendants' of Park and Thomas, as Fisher and Strauss observed, used various parts of the 'dual inheritance'. Some later practitioners may have tended to focus more on the subjective meanings of individuals and to have not considered, or played down, the notion of the interplay between freedom and constraint. Some criticisms along these lines have been made.

Hargreaves, for example, though not exactly a critic, did comment that as 'ethnographic' studies proceeded, they increasingly needed to publish 'news' about unknown groups to attract attention, so there was a tendency to focus on the bizarre. If they considered "mainstream groups", their findings were seen as "obvious". Hargreaves also argued that there was a lack of theoretical development for he stated that:

"Ethnography frequently became so entranced by relatively atheoretical reporting of the hidden crevices of society that it failed to produce not only 'formal theories' but also the lower level 'substantive' theories which Glaser and Strauss so rightly perceived as crucial to the future of S1."

(Hargreaves, D., 1978, p. 9)

Hargreaves argued also that interactionists' early attacks against 'positivism' were "empty" because they did not provide "any adequate social scientific substitute". (p. 9).

Burgess noted another similar criticism of Chicago sociologists by Douglas, who was said to have criticised "the low level of analysis" of their studies. (Burgess, 1982, p. 7).

The criticism of being 'atheoretical' has been made of interactionism in general. However, in the first part of this section it was noted that Atkinson and Delamont rejected the view that "ethnographic" or "qualitative" work, including "symbolic interactionism", was unconcerned with "generalisation". (Atkinson and Delamont, 1986, p. 249).

Burgess also reported another criticism by Douglas of Chicago sociologists, that their:

"approach to field research does not reveal the processes among groups in urban society as it is based upon assumptions of a 'little community' where conflict and complexity are missing."

(Burgess, 1982, p. 7)

This criticism is rather like that of Sharp and Green, who considered that interactionists were unable to consider power and structured issues, because they were said to:

"ignore it and assume that interaction occurs on the basis of democratic negotiation between interested parties who are political equals."

(Sharp and Green, 1975, p. 12)

Hargreaves dismissed this view as "arrant nonsense". (Hargreaves, P., 1978, p. 11).

The researcher also disputed this idea of Sharp and Green in respect of 'phenomenology' as has been stated. From an interactionist viewpoint as Hargreaves also observed in relation to his work, relations between teachers and pupils (or between teachers and parents for that matter), are not exactly those of 'political equals'. In this research, teachers were very much the main 'definers of the situation'.

Interactionists and phenomenologists can consider questions of power

and ideas of constraint if enough questions are asked. Sharp and Green appeared either to have had an inadequate understanding of interactionism/phenomenology, or not to have applied it correctly if they did understand it, to have made this assertion.

In respect of Chicago sociologists, Blumer, for example, certainly was critical of 'positivism' and the idea that certain 'scientific' procedures should be used in sociology. Blumer, who was the first to actually use the term 'symbolic interactionism' (he was said to have coined it in 1937) was against the use of such 'quantitative' procedures as 'variable analysis' and "mathematical modelling, systems analysis and survey research". (Williams, 1976, pp. 118-9).

Blumer considered that the findings produced by studying the relationship between variables would not give an adequate understanding of human behaviour. (Cuff and Payne, 1979, pp. 92-93). As noted in the first part of this section, Lofland was similarly doubtful of the use of 'quantitative' procedures in 'qualitative' research. (Lofland, 1971, pp. 61-2).

However, Burgess' comment that Chicago sociologists used a range of methods has already been noted, so not all Chicago practitioners agreed with Blumer in this respect. Also, there were those in the department, such as Ogburn, who advocated the use of statistics. (Coser, 1979, p. 312).

Like Mead, Blumer was against any concept of human behaviour as a matter of automatic 'stimulus-response'. Behaviour, in his view, had to be understood in terms of human purpose in particular circumstances. Therefore Blumer held that social interaction in natural settings had to be studied. In 1969 he set out the basic ideas of 'symbolic interactionism' as being:

"that human beings act towards things on the basis of the meanings that the things have for them" ... "that the

meaning of such things is derived from, or arises out of, the social interaction that one has with one's fellows ..."
"that these meanings are handled in, and modified through an interpretive process used by the person in dealing with the things he encounters."

(Blumer, in Williams, 1976, p. 118)

This was very much in line with Mead's social psychology. Thus, for Blumer, the aim of the sociologist should be "a sympathetic and sensitive understanding of their [individuals] general outlook of the world". (Cuff and Payne, 1979, p. 97). The aim was also to see how various social processes

"can be seen as organised patterns of conduct and social interaction across the span of daily experience."

(p. 97)

Blumer also saw interaction between groups as a creative process likely to lead to democratic progress in which he, like Park and Thomas, was interested. Blumer was also in favour of the study of the development of public opinion. Fisher and Strauss argued that Blumer did not wish to accept that emerging opinion might be "reactionary". They added that this was:

"especially true in the realm of race relations, where "subjective" reality - the idea that people are biologically distinct - contributes powerfully to the discriminatory patterns that prevail."

(p. 478)

They added that:

"Like Park, Blumer counts on migration, urbanisation and occupational specialisation to break down the social barriers and to make people aware of their common interests."

(Fisher and Strauss, 1979, p. 478)

Thus, Blumer was not ignoring 'social constraints' and their effects on attitudes, so much as being optimistic about the power of knowledge to change these 'social barriers'.

To the researcher, it seems that increased knowledge of other social groups does not necessarily lead to greater awareness of 'common interests'. Apart from race relations, where Blumer's idea is at least problematic, teachers may come to know a great deal about their pupils and the families

and homes they come from. Such increased knowledge may well be "reactionary". At Moorland it seemed to reinforce negative stereotypes of 'working-class' homes, which were not seen as having much in common with 'middle-class' homes such as those of teachers themselves. This is shown in Chapters Four, Five and Six.

Everett Hughes was more sceptical than Blumer in respect of the power of group interaction and "creative group response" to bring about "progressive, democratic relationships". (Fisher and Strauss, 1979, p. 478).

Thus he also was well aware of constraints. Hughes undertook with others a series of studies of occupational groups. One reason for this was his view that work was an area of central importance for modern society, and consequently it was often within an individual's occupation: "that an individual's sense of self is often formed, defined and affirmed." (Cuff and Payne, 1979, p. 97). Thus, Hughes, and his associates were concerned to show how membership of an occupational group affected individuals. They also considered how group cohesion was built up and maintained as well as relations between one occupational group and others with whom its members dealt, such as clients or patients. Hughes considered as a result of his investigations that for any occupational grouping, the main concern of members was with the survival of the group. It was to this end that there was a stress on internal cohesion and the exclusion of others, within the limits which might be set by a need for members of an occupation to have the tacit support and consent of those others in order to carry on the occupation.

This idea came to mind when hearing teachers in the two main research schools simultaneously stress and defend their 'professional' knowledge against either attempts by parents to become involved in 'basic curricular' affairs such as the teaching of reading or the suggestion by others that parents should do this, and also say that they wanted parental help and co-operation.

Hughes, like Park and Thomas, considered that public awareness of occupations, of what members of an occupation do to others, was essential for a "democratic public". Openness could mean that occupations could exist generally within "a competitive system" supported by the public, where occupations could "check each other" and "adjust their standards", in consequence, "to the actual conditions of group life". The public, if well informed, would understand "the limits of good service in general". It was the task of sociologists to spread the "news" of occupational life in order to make the public 'knowledgeable'. (Fisher and Strauss, 1979, p. 479).

Hughes developed the concept of 'dirty work' in relation to some occupations. This was the idea that there are certain tasks which 'society' or individuals in groups want doing, but want someone else to do for them. Caring for 'insane' people or criminals could be examples of such tasks. 'Dirty work' thus involved the tacit consent by individuals to what was being done in their name by and to 'others'. Hughes argued that in the absence of public knowledge and openness about what was being done in actual group interaction by an occupational group in 'dirty work', there could be no free discussion. In these circumstances such a group could become over-powerful in relation to external 'others', and thus be able to deal with these without suffering "the check or sanction of a democratic public". (p. 479).

This lack of knowledge was seen as dangerous for democratic progress. Thus, Everett Hughes was well aware of issues like power and constraint, and the limits these could place on individual freedom.

Hughes also developed the concept of 'career' as part of his work on occupations, but this meant more than its use in 'professional' terms. It meant progress in terms of passing through stages, during a life, in various situations. In theoretical terms, Hughes was interested in George

Simmel's concept of 'formal generalisation'. This meant distinguishing between "the 'form' and 'content' of social phenomena". (Cuff and Payne, 1979, p. 99). Thus, social phenomena might have a different content but have similar 'formal' structures of social relationships. The concept of 'career' was used to show this similarity across different situations. Thus, a mental patient or inhabitant of any other institution could have a 'career', in the sense of moving into and through different stages of 'being a patient' or being a 'marijuana user', one of Howard Becker's studies which used this concept.

Becker was influenced by Hughes, and was associated with him in a study of medical students, and like him, was well aware of the issue of power and the ability by some groups to try and control others, as Hargreaves also observed. (Hargreaves, P., 1978, p. 11). It is difficult to see how his work could be seen as lacking a concern with such issues.

In much of his research, Becker was concerned with the ability of more powerful groups to define members of others, who were in competition with them for "social space" as being in some way 'deviants'. He developed the concept of 'labelling', or typifying people, on the basis of some perceived attribute or behaviour, as belonging to such a category as 'deviant'. Becker was concerned to discover the processes involved, with why, and how and by whom people were 'labelled', and with the consequences of such categorisation. In this he shared concerns with Thomas and Hughes.

A basic idea of 'labelling' is that once it has occurred, then the behaviour of social actors can be interpreted as confirming the label. One consequence of this may be that groups and individuals so labelled may come to accept the label, and to accept the definition as 'appropriate'. Thus 'deviants', (or an 'Iron Lady') may come to see themselves in these terms and act accordingly, thus 'fulfilling expectations' for those applying the label. This is the idea of the 'self-fulfilling prophecy'.

However, 'responses' to such 'stimuli' are not automatic. There remains the possibility that the 'label' may be changed if those 'labelled' do not accept the definition and do not conform to it in their behaviour. It is not a static process. Also, the group initially labelled as 'deviant' or anything else, may in turn 'label' those who 'label' them, and form their own 'society' in reaction. Blumenstiel observed that "social problems" were the consequence of this process of labelling of 'outsiders' by 'insiders' and vice-versa, and the "mutual misunderstanding of the validity of each others' life-styles and in-group legitimacy". (Blumenstiel, 1973, p. 194).

Some studies of schools, taking an interactionist viewpoint, have shown that teachers (generally the more powerful group) do typify pupils, and that pupils, in their interactions with teachers, respond to their perceptions of the latter's perceptions of them. The consequences of such action and reaction can be 'social problems' of many kinds, such as the polarisation of pupils into different sub-groups, such as 'pro' and 'anti' school, with consequences for their educational 'career' in both senses. (Hargreaves, D., 1967; Rist, 1970; Lacey, C., 1970; Nash, 1973; Hargreaves, Hester and Mellor, 1975; Woods, 1979 and Beynon, 1985).

At Moorland, the main research school, 'labelling' and typification of pupils by teachers was evident. It was, however, difficult to say what the long term consequences of this would be, or even if the observed pupil behaviour was caused by other factors. Certainly, though, some children even this young, seemed 'anti-school'. Also, in so far as Moorland pupils seemed in general, in comparison with those in other schools, to be less advanced 'academically', it was possible to see many of them as ending up in lower academic groups in junior or secondary schools, especially if being 'behind' was combined with behavioural problems for teachers. Following up this idea was outside the scope of this research, but it is at least feasible that processes described by researchers in secondary schools

originate in infant schools.

In political terms, Becker was very much concerned with wider social issues and democracy. The interactional competition between groups tended to "systematically punish and promote forms of deviance" while at a "lower" level "integration" between such groups was "low". (Fisher and Strauss, 1979, p. 481).

Becker was concerned that the power difference between groups could both not only lead to injustice, in that some people were labelled as 'outsiders', but could also make a 'democratic society' impossible by excluding them in this way. Blumenstiel argued that in this respect "labelling theory is remarkably reminiscent of the anomie theory which it supposedly dismisses". (Blumenstiel, 1973, p. 194). 'Outsiders' were "detached" from the "insider" group, and according to Blumenstiel such detachment was "the essence of anomie". (p. 194). Thus, the concern for social integration was similar to Durkheim, who found in 'anomie' or 'normlessness' danger for 'social order' and 'society'. This indicates that both 'traditions' share not only methods but concerns.

Becker, unlike Thomas, did not see powerful groups as open to reason or a 'just exchange' of ideas.

In his concern for democratic progress, in how a democratic society might be achieved, Becker was in the Park-Thomas line. Like Park, he was interested in those 'at the margins' of society, as not having allegiance to 'traditional' (or Establishment in Blumenstiel's comment) values and thus as "presenting Options". For Becker, a democratic society would be open to incorporating the very deviant values:

"which moral entrepreneurs and other powerful groups preclude and stigmatise."

(Fisher and Strauss, 1979, p. 481)

This openness to reception of alternative values, and so 'democracy' was what seemed impossible.

Becker's focus on 'outsiders' or 'the underdog', was criticised by Alvin Gouldner, as both Fisher and Strauss and Blumenstiel comment. He was alleged to have helped, even if inadvertently, those in power, such as "federal bureaucracy", by giving them 'secret knowledge' which could be used to the detriment of the 'outsiders', and also for apparently presenting a "dispassionate sympathy" which was comforting to "guilt ridden liberal academics", according to Fisher and Strauss, and of "exploiting the miserable and lonely" according to Blumenstiel.

Becker, in reply to these criticisms, indicated that for him, like Park and Thomas, the sociological task was to 'bring the news', to tell how groups or individuals are actually treated by others, as Hughes also held, and to show how people reason and are, in order to get behind "false self-concepts" imposed by powerful groups on others. Hence group interaction had to be studied, and the actual processes of imposing definitions, and the social consequences of these.

In this part of his work Becker can be seen as very much concerned with the structural consequences of group interaction and questions of differential power. Thus, as indicated previously, Hargreaves was correct to state that Sharp and Green's general criticism of 'interactionism' could not be sustained.

However, Becker was concerned with other issues, though the idea of situational constraints, which can be seen as 'structural', was present. He was interested in the process of 'socialisation', of an individual learning how to 'become' something, such as a medical student or marijuana user. This view of 'socialisation' was very different from the idea of socialisation as patterns of behaviour laid down in childhood and carried into adult life. This is seen as 'stimulus-response' behaviour, which is conceptually rejected by interactionists. Socialisation is seen by them as a learning process, in response to particular situations, which does

not end in childhood but continues into adult life. (Cuff and Payne, 1979, p. 113).

Thus, behaviour, and conceptions of the 'self', can change as new situations require new 'learning'.

Becker showed this process at work in his study of medical students, with his associates. In the course of their training, students had to cope with impossible demands as regarded work required of them, and in consequence modified their 'long term perspective' of being good doctors in favour of a 'short term perspective' of 'getting through' and passing examinations. Thus, they learned how to become students who could cope with the system and find out what was necessary in order to succeed in those circumstances. This was not an 'internalised' and permanent 'socialisation', but a change to meet a particular situation.

The concept of socialisation as 'learning how to become' was raised by some of the teachers in the research, who pointed out that children had to learn to cope with school demands, and change from being a child in a family to being a pupil in school. Thus behaviour appropriate to school rather than home had to be acquired.

It can be argued that children in school, learning how to be pupils, do in the process also discover how to 'get through', and 'give teachers what they want', and that this is just as much a part of what it is to be a pupil as it is of 'being a student'. So teachers can have their idea of 'appropriate' behaviour, both social and academic, of the school child, and seek to impose these on children, but the latter may not necessarily 'internalise' these 'official' values, but rather learn to 'cope' with the situation in various ways, as indicated in Woods, for example. (Woods, 1980). Sometimes such 'coping' can take the form of outward 'compliance' and sometimes that of outright rejection and the formation of anti-school groups, as shown in various studies apart from that of Hargreaves, as well

as other adaptive strategies. (Willis, 1977; Beynon, 1985; Pollard, 1985).

Thus, teachers' attempts to 'socialise' and control children are not necessarily successful. In the research, particularly at Moorland, some children were compliant, while others certainly were not.

Becker's consideration of 'socialisation' as the adaptation of people to particular situations has some similarities with part of the work of Erving Goffman. As noted, Fisher and Strauss observed that Goffman was not always considered as an interactionist, but was so regarded by many. They also noted that he was 'trained' in Chicago, and was influenced by Hughes. (Fisher and Strauss, 1979, p. 479).

In part of his work Goffman developed the concept of 'total institutions'. These were places, such as mental hospitals or prisons, in which the barriers between different spheres of life, such as work, play or sleeping, are broken down, and where therefore those in charge are able to regulate inmates in almost all aspects of their life, because also, in these institutions, they are cut off from external influences. Goffman showed that those in control attempted to reshape or 'socialise' inmates into behaviour which made life easier for the staff, and that this involved almost a 'stripping' of an inmate's previous identity through various "mortification" rituals. Goffman showed that even in such situations inmates attempted to retain some element of themselves, as well as adapting to the system outwardly in order to keep out of trouble. Goffman showed that in these circumstances behaviour which might elsewhere appear irrational, such as 'hoarding', was perfectly rational in view of the lack of privacy to which inmates were subjected. Even here, inmates attempted to find 'space' for themselves outside the orbit of staff. Goffman indicated clearly that although in the case of 'total institutions', which are extreme situations in one sense, individuals are formed into particular forms of behaviour by the coercive constraints under which they live, the attempts to control

their self-perceptions cannot totally succeed. (Goffman, 1968).

Thus, Goffman too was aware of issues like power and constraint, and was concerned, like Mead and Thomas and Park, with the relationship between these and individual freedom. His general position was that individuals were not totally free to act as they chose, but neither could behaviour be completely determined by structural factors. This is shown further in the aspect of Goffman's work known as dramaturgy. Here, Goffman presented the concept of social actors as being engaged in a form of 'drama', of putting on a 'performance' or 'show', with the aid of 'fronts' or stage props such as particular clothes or equipment or possessions to make this convincing for the various 'audiences' to which they play. The 'stage' is the various contexts or social situations in which the drama is played, and the 'lines' or 'directions' are the social rules. These can in some cases be clear as to the limits of 'acceptable' behaviour, so limiting the 'performance'. Social life was thus seen by Goffman as a form of 'impression management', which could be 'sincere' or 'cynical' - in the latter case there was a discrepancy between the public 'performance' and the actors' private beliefs. Goffman also used the concept of 'regions', the 'front' and 'back'. The 'front region' was where the performance took place, while the 'back' region was the area where actors would relax their 'performance' and perhaps express private beliefs.

The various performances were seen as related to individual actors self-perceptions of themselves - trying out roles - and also to the perceived expectations of others, in the various social interactions. Thus, the 'self' was not static but dynamic. It was 'creative' in the Meadian sense, because although the 'social rules' were known, these did not completely control behaviour in situations. (Goffman, 1969).

Fisher and Strauss summarised Goffman's views on the relationship between the self and social situations, expressed in a later paper, as being that:

"situations are potentially problematic, and that others responses to one's actions are always potentially problematic, but that one's own self is also problematic, In short, the individual is sufficiently complex to allow for appropriate flexibility, yet the freedom is far from total, since the self is social, acting in reference to social rules."

(Fisher and Strauss, 1979, p. 480)

The researcher had the idea of 'performance' very much in mind in this research, in particular with two aspects of 'becoming a researcher'. Seeking to gain access to schools in the first place, and continuing in them, was a matter of 'self-presentation' and 'impression management', including the wearing of what were seen by the researcher as the 'right clothes', points mentioned in a later section.

There was also the aspect of 'becoming a PhD student' in relation to the research supervisor and other academics and fellow students.

In both cases there were ideas about the role, but a lack of knowledge, so it was a matter of 'trying the part' and modifying it over time in the course of interaction.

Both these aspects are 'scenes' where the 'lines' are to a considerable extent set within limits by others, and there is a built-in power differential as well as the possibility of different interpretations of the research role from that of the 'actor' playing it being held by 'significant others' such as the head teacher/gatekeepers and the research supervisor.

There was also a question in mind as to whether the teachers were also putting on a 'performance'.

A different aspect that also came to mind was that of 'front' and 'back' regions, particularly while in schools. Teachers as 'actors' have their 'stage' in the classroom, with the staffroom as the 'back region'. The researcher found it not only extremely difficult to find a 'back region' of her own, which meant a continual 'performance', but was also conscious

of intruding on the 'back regions' of teachers needing to relax.

To sum up thus far, the origins of 'symbolic interactionism' have been discussed, and also the work of some sociologists connected with the 'Chicago School'. Many more of the latter could have been mentioned, but the point was more to show the roots of this approach, as well as indicating those concepts from studies within it which were seen as particularly useful to the researcher, both in relation to work in schools and to the activity of being a research student in and out of the research sites. It was noted in the course of the discussion that there were several criticisms of 'Chicago style' interactionists.

One of these criticisms centred around the idea that they placed too much stress on 'subjectivity' and 'freedom', the voluntary nature of social actions, ignoring 'structural issues', usually meaning 'political' or 'economic' or other major 'constraints'. From the discussion of Mead and other originators and their 'descendants', it seems clear that this criticism is unjustified. Even if some researchers using this approach may have focused too much on individuals it is not true of the 'School' as a whole.

Another criticism has been that 'symbolic interactionism' was theoretically weak. This depends on what is meant by theory. If this is defined as a tentative explanation of observed events, at whatever level, then symbolic interactionism seems to generate these from the data, as Glaser and Strauss stated could be done. For example, Becker's explanation of why medical students changed their 'ideas' from the long to short time perspectives, or Strauss et al's for why there was a difference between the treatment of 'chronic' and 'acute' psychiatric patients, in spite of an ideology of egalitarian treatment, are theories, even at a lower level.

To say that they are not seems to mean that a fairly rigid model of a 'theory' is being adhered to.

If by 'theory' is meant something like 'Marxism' or even 'functionalism' or some equivalent 'grand theory', to borrow from Merton, then perhaps critics are right. However, it seems just as much a 'theory' of this kind to suggest that what appear to be solid 'structures' are in fact the result of past group interactions and conflict and accommodation, created by the different purposes of groups over time, and perhaps maintained in the present by differences in power between groups. After all, in one sense this is what Marxism was also saying.

Another criticism of 'Chicago-style' interactionism, only noted briefly in passing, was that its method of participation, on its own, was not very effective. Some of these criticisms came from within the Chicago School itself. Fisher and Strauss, for example, stated that it was the kind of attack which:

"Samuel Stouffer and William Ogburn levelled at Blumer and other interactionists; namely that sociology required statistical studies in order to test anything, otherwise it was just a variety of social philosophy."

(Fisher and Strauss, 1979, p. 486)

Other criticisms were developed elsewhere. As noted, Williams wrote of there being two distinct sub-schools of interactionism, those of Chicago and Iowa. He reported views that the differences between the two lay in the former preferring:

"phenomenological approaches, participant observation and "sensitizing concepts" all linked with a "logic of discovery" whereas Iowa interactionists have preferred operational approaches, the Twenty Statements Test and "definitive concepts", all linked with a "logic of verification"."

(Williams, 1976, pp. 121-2)

Williams himself stated that the concerns of the 'Iowa interactionists' were to try and fit:

"symbolic interactionist research and theory under the legislative control of the deductive-nomological method of enquiry, continually emphasising verifiable indicators of the subjective."

(p. 121)

Williams was critical of attempts to mix two methodological approaches because

he considered that these were based upon different theoretical assumptions about the nature of 'society'. This view has been noted before. It is the general criticism of many 'interpretive' researchers. However, as observed in the first part of this section, it is one based on a particular view of 'the scientific method' and ideas of 'positivistic' sociology, which may be mistaken, given that neither of these has a single clear meaning. Since there may be no such thing as 'the scientific method', and if some practitioners in the 'other' tradition have used some of the methods also found in the 'interpretive', and if some advocates of 'quantitative methods' such as Lazarsfeld (mentioned by Fisher and Strauss as influencing Glaser) or Lundberg, have not dismissed qualitative data collection, then the idea of ideological 'purity' in relation to choice of methods seems misguided. As already noted, Burgess commented that Chicago sociologists had used a range of methods in any case. Coser also stated that Albion Small, the first director of the Chicago sociology department:

"seemed from the very beginning to have been committed to a deliberately eclectic stance."

(Coser, 1979, p. 312)

So 'eclecticism' was not always seen as wrong. Fisher and Strauss also, in writing about developments in interactionism as "Combiners, Crossovers and Borrowers" said of one 'radical sociologist' that his:

"criticism reminds us that nobody who is trained in a tradition needs to carry it like the Holy Grail, either through life or through graduate school, pure and uncontaminated by other traditions."

(Fisher and Strauss, 1979, p. 486)

Fisher and Strauss also commented that 'outsiders' have always used "bits and pieces" from the interactionist body of work. They added that: "traditions do get combined, whether consciously or not" and commented that this was "the way of the intellectual world". (p. 488). It could be argued that even 'pure' symbolic interaction itself exemplified this, since there were not only several sources for the ideas of the 'founding fathers', but Park also borrowed the method of participant observation from anthropology, which, as

stated in the first part of this section, was a tradition with other views of 'society'.

As also noted in that part, Marsh said in relation to the use of inductive and deductive reasoning in surveys, that it was "folly" to "turn up one's nose" at anything which would help in the exploration of the data. It seems equally wrong to reject a method just because of its alleged theoretical implications, if it will help in the collection of data, that is, if it fits the researcher's 'purpose in hand'. Many sociologists will take a 'pragmatic' or 'conventionalist' line in practice in any case. Of course, it is important to know what the possible 'theoretical' implications of methods are, as well as the practical problems of these, although even this can be difficult for the beginning researcher. It is also important to be aware of the stance of those writing methodological textbooks, something that depends partly on prior knowledge in order to recognise it, which is not always possessed, and partly on clarity by the authors of such textbooks on what their position is. There were two examples of a lack of clarity causing some methodological confusion for the researcher, for slightly different reasons.

Having been recommended to read Glaser and Strauss, the researcher did so, but found it very difficult to know what they meant by 'theory', and how to place them exactly. On reading Williams much later, the researcher was relieved, in a sense, to find that he stated about these authors that:

"an aspect of their enterprise that remains unclear is what model of theory they are holding to."

(Williams, 1976, p. 135)

He regarded them as taking an "eclectic view". (p. 136). Since there is a tendency, in the 'loneliness of research', to think that it is the researcher's fault if something is not clear, it is very reassuring to find that others share this difficulty.

Denzin was another 'interactionist' textbook found confusing at the time of initial reading. It seemed to the researcher somewhat muddled in its discussion of interactionism, so that the researcher did not understand the principles on which the book was based. Williams, however, stated that among other things Denzin was trying to "synthesise elements from both Iowa and Chicago interactionism". At the time of first reading Denzin, the researcher was not even aware that there were such 'sub-schools'.

Williams also regarded Denzin as being "eclectic", particularly in: "the introduction of the concept of triangulation into symbolic interactionist methodology". (p. 122).

This researcher does not, unlike Williams and other critics, object to 'eclecticism', seeing it as almost inevitable, if only unconsciously, but is concerned to point out that beginning researchers are not necessarily aware of divisions, or attempts at integration, within a particular approach, and writers of 'textbooks' on methods do not always clarify their own position. As noted, Lofland, at least, indicated that for him 'qualitative' and 'quantitative' methods did not mix.

These comments conclude the discussion of 'symbolic interactionism'. It has been dealt with at some length, since it was one of the most important influences upon the research as it developed, in terms of reflexive ideas, and also because the method of conducting the research, participant observation, is associated with this approach, though not solely.

Symbolic interactionism has been shown to be not 'a single perspective', but one with a number of sub-divisions. It has been shown to have various sources, and not to be necessarily confined to a single method. Within the perspective there have been those advocating statistical procedures and also a 'scientific' approach.

It has been shown that symbolic interactionism has been consistently concerned with the tension between the creative individual and social

constraints, and that the question of inequalities in power has not been neglected. It has also been indicated that interactionism, however, is against any idea of determinism.

The main ideas taken from the study, apart from the more general ones above, were the concern with real lives, based upon close observation, and with this the idea that the past as well as the present should be studied, and placed contextually, since present attitudes may be the result of past interactions. This was another reason for looking at the history of infant education. Then there was the concept of labelling and typification, together with the need to study group interaction and the process of typification. The idea of 'self-preservation' and 'performance' in various situations was also useful in assessing some aspects of the research.

Various criticisms of symbolic interactionism were noted, and were seen as mainly unjustified.

Finally, the 'purist' view was noted that methods should not be mixed, because different methods entail different views of 'society'. Against this was the concept of 'eclectism', which was observed to have been practised by interactionists in any case. However, a plea was made that 'textbooks' should be clearer in stating the theoretical position of the writer, especially when an amalgamation is being presented.

The next perspective to be discussed is anthropology. As observed above, this was the source of methodological 'borrowing' by Park. However, such borrowing from anthropology was not unusual, for, as noted in the first part of this section Durkheim and Weber, as well as Marx, looked at accounts of 'primitive' societies, comparing these to modern industrial ones. Participant observation in some form continues to be the main means of studying 'societies' in the interpretive tradition. One of the reasons for discussing anthropology is that it was looked at with reference to participant observation or 'fieldwork'.

'Anthropology' is, of course, a field of study in its own right, rather than a perspective within sociology. This field is considered as somewhat broad in scope. One definition is that it is:

"the study of the origin, development and nature of the human species Through the systematic analysis and comparison of all that can be discovered about humanity, anthropology seeks to develop increasingly profound and useful knowledge about the human condition."

(Beals, Honer and Beals, 1977, p. 1)

These authors thus seem anthropology's general aims as being "the explanation of humanity" and also:

"to understand ... the manner in which humans form groups and develop distinctive ways of life, and the various limitations and potentials inherent in humanity"

(p. 2)

Given this, width and depth, only some general points can be made.

Anthropology has traditionally been associated with the study of 'exotic' or 'primitive' societies, different from those of its practitioners. (Frake, 1983; La Fontaine, 1985; Spindler, 1970; Srinivas, Shah and Ramaswamy, 1979). However, apart from 'Chicago style' interactionists in America, anthropological studies of 'communities' in urban societies have been done. (Gulick, 1973, pp. 981-3). In Britain, for example, Welsh villages were studied by Frankenburg. (Frankenburg, 1957, 1966). Some of this work was developed within the University of Manchester, and the works of Hargreaves and Lacey in schools was part of this initiative. Because of their attention to social interaction, they were also seen as 'interactionist'.

It was observed in the course of reading that, as a field of study, anthropology has its own divisions and a range of sub-fields. (Beals, Honer and Beals, 1977; Voget, 1973). It is not the intention here to discuss all of these, which would in any case be a Herculean task, nor to cover the divisions that are mentioned in detail, for the same reason. Those that are mentioned are social and cultural anthropology and structuralism. Ideas

which seemed relevant to the research are noted in the course of the discussion.

Although a separate field, anthropology is seen as having close links with a range of other disciplines. As well as history and sociology, these are variously reported as biology, logic, linguistics, psychiatry, social psychology and philosophy. (Hudson, 1973; Leach, 1974a; Levi-Strauss, 1968; Lewis, 1976; Voget, 1973).

In terms of its own development, perhaps its closest links are with sociology. Some theorists have traced the roots of anthropology in the work of classical thinkers through to those of the Renaissance. However, its real beginnings are generally ascribed to 18th century ideas of 'progress' and the possibility of a 'Science of Man', and 19th century concepts of evolution. (Voget, 1973). Thus, it has the same basic roots as sociology. Lewis noted the 'confused frontiers' between the two (and between anthropology and other disciplines) in saying that:

"Some authorities hold that social anthropology is really a sub-division of sociology Others assert that anthropology is the tree and sociology the branch."

(Lewis, 1976, p. 24)

In any event, anthropology has had an almost symbiotic relationship with sociology, with 'borrowings' of theories and methods going both ways. Anthropology has also had similar disputes to those in sociology, such as whether it should be 'positivist' or 'humanist', or use 'quantitative' or 'qualitative' methods. (Hudson, 1973, pp. 122-3; Pelto and Pelto, 1973, p. 265).

There have been those who stressed the need for objectivity, while others stressed the subjective nature of the enterprise, and those who have taken an intermediate position, as in sociology generally and 'interactionism' in particular.

This has some implications for the view that theoretical 'purity'

should govern the choice of methods. This will be referred to later.

Before discussing the divisions of anthropology noted above, two terms which are associated with anthropology are defined, because they seem important. These are ethnography and ethnology. In strict terms, the former means writing about peoples, and the latter the study of peoples. However, they are somewhat differently defined by others. Ethnography, for example, is stated by Leach to be "the straight description of custom, either traditional or contemporary". (Leach, 1974a, p. 371). Voget listed ethnography as "description and analysis of specific cultures" and ethnology as "descriptive and comparative" with both as part of "cultural anthropology". Lutz described ethnography as:

"a holistic, thick description of the interactive processes involving the description of important and recurring variables in the society as they relate to one another, under specified conditions, and as they affect or produce certain results and outcomes in the society."

(Lutz, 1986, p. 108)

He added that in his view ethnography was not a case study, focussing on "a single issue", and not:

"a field survey which seeks previously specified data, or a brief encounter (for a few hours each day for a year, or 12 hours a day for a few months) with some group."

(p. 108)

This research was not a case study, as defined above, but on Lutz's definition it was not ethnography, for the second reason. This point will be discussed later.

Wolcott defined ethnography as:

"literally, an anthropologists' "picture" of the way of life of some interacting human group; or, viewed as process, ethnography is the science of cultural description."

(Wolcott, 1975, p. 112)

He added that 'ethnography' and 'ethnology' were sometimes "used interchangeably", but he considered that "ethnography" should be taken to refer to basic descriptive work; while "ethnology" should be taken:

"to indicate more theoretically oriented statements about relationships and meanings either within one group or among a number of societies."

(p. 112)

Wolcott observed that the term "case study" was also used. He stated, that, in 'discrete' terms, this provided "a handy and unassuming label".

'Ethnography' implied:

"both a more comprehensive and detailed report and the perhaps unattainable ideal of a complete and perfect account."

(p. 112)

Thus his definition of 'case study' differed from that of Lutz. In this research, 'case study' was seen as either focussing on a single issue or on a single school, hence it was rejected as a description of the research.

However, 'ethnography' is associated with 'fieldwork', the living in a 'community' for an extended period of time as participant observers. (Pelto and Pelto, 1973).

It depends upon how 'community' is defined whether research in schools can be classed as 'ethnography', a point which will be considered later.

Frake defined "ethnography" as "the science - and art - of cultural description". (Frake, 1983, p. 60).

Boas defined "ethnology" as "the science dealing with the mental phenomena of the peoples of the world". (Boas, 1966, in Black, 1973, p. 515). This was a distinctive interpretation, but the ideas of theoretical construction and comparison are at least implicit.

Levi-Strauss defined 'ethnography' as:

"the observation and analysis of human groups considered as individual entities ..." [it] "thus aims at recording as accurately as possible the respective modes of life of various groups."

(Levi-Strauss, 1968, p. 2)

He regarded 'ethnology' as that study which "utilises for comparative purposes ... the data provided by the ethnographer". (p. 2). This is as wide as

to be virtually meaningless. Also 'ethnography' has some different meanings or interpretations.

However, in general ethnography can be seen as both the study of a 'community' by living in it for a considerable period and also the production of a written account, while ethnology can be seen as the comparison of different peoples or groups, through the use of ethnographic accounts, in all or some respects, with the aim of building up theoretical explanations. This present research was in part comparative, and did look at other 'accounts' as the research proceeded and in the analysis and writing-up stages.

These terms having been defined, the distinction between social and cultural anthropology, which was mentioned as one which was seen as relevant, is discussed.

Voget observed that the concept of 'culture' was important for anthropology, and one which was: "central to the distinction between anthropology and other disciplines, notably sociology". (Voget, 1973, p. 2).

Many 'interpretive' sociologists might disagree with the implication that 'culture' was not a concern of sociology, providing it was not a 'taken for granted' concept and was not seen as 'determinist'.

Voget stated that 'culture' could "broadly" be defined as: "any product of the social life of man, either in the past or in the present". (p. 2). He also questioned a definition by Linton that:

"A culture is the configuration of learned behaviour and results of behaviour whose component elements are shared and transmitted by the members of a particular society."

(Linton, 1945, in Voget, 1973, p. 2)

Given this definition, it might seem legitimate to write of 'the culture of a school' or 'the culture of teachers', as has been done in the sociology of education at times. However, to speak of the culture assumes that there is only one in a school or among teachers. Studies show that this is not

necessarily so. There may be different 'cultures' in a school both between teachers and pupils, and within both groups. In Chapter Three, the existence of different belief systems in a school, and conflict, is noted. (Lee, 1984; Hargreaves, 1967; Richards, 1979; Willis, 1977).

In Chapter Five, the idea of a possible 'culture clash' in a school is raised.

Voget stated that 'cultural anthropology' had diverged into two separate approaches as between "British and French social anthropologists" and "American and German ethnologists". Both groups had distinguished between the "social" and the "cultural". (Voget, p. 2).

According to Levi-Strauss, social anthropology was concerned with "the study of institutions considered as systems of representations" while cultural anthropology was concerned with:

"the study of techniques which implement social life (and sometimes also, to the study of institutions considered as such techniques)."

(Levi-Strauss, 1968, p. 3)

According to Voget, the principal feature for social anthropologists was:

"the social structure, conceived to be the orderly arrangement of persons in the society according to their status rights and obligations"

with 'culture' being confined "after Radcliffe Brown":

"to the 'learnt ways of thinking, feeling and acting' that form a part of the 'social process'."

(Voget, 1973, p. 2)

Thus British 'social anthropologists' stressed social structure, together with French 'colleagues'.

American 'cultural anthropologists' on the other hand, stressed 'culture' and:

"viewed the social order through the integrative values and purposes implicit in or explicitly voiced in the culture patterns."

(Voget, 1973, p. 2)

Lewis, commenting on the difference between British and American anthropologists also made the point that the former placed their:

"primary emphasis on social relations, treating culture as a vehicle or medium for social interaction rather than an end in itself."

(Lewis, 1976, p. 19)

He added that American anthropologists:

"who give priority to culture and to cultural patterns, underestimating (as it seems to us) the social dimension."

It might be thought that both dimensions require equal study, rather than priority being given to one rather than another. In infant schools, for example, as in others, there are 'structural' features, not a matter of interaction, such as the position of the head versus teachers, and teachers as against that of pupils and parents.

The reported anthropological difference is important when considering the relationship between 'interpretive' sociology and anthropology, because it becomes a matter of which anthropology is being borrowed from. Also, if there are theoretical differences behind the use of a particular research method such as participant observation, the 'purist' objection to using a method with theoretical implications which are disagreed with seems unsustainable. 'Participant observation' as used in anthropology cannot necessarily be equated with a view of society as composed of interacting individuals, nor with a lack of interest in 'scientific methods'.

For example, apart from 'culture', La Fontaine noted that two main concepts for 'social anthropology' are 'function' and 'structure'. These two were important in the work of Malinowski and Radcliffe-Brown, both of whom were seen by Lewis as "the authentic founders of modern social anthropology". (Lewis, 1976, p. 52). Voget, however, considered that "British social anthropology developed under the intellectual aegis of A. R. Radcliffe-Brown." (Voget, 1973, p. 28).

As noted in the first part of this section, British social anthropology was seen as structural functionalist, but it developed into this, to some extent as a matter of rivalry between Malinowski and Radcliffe-Brown. Lewis regarded them as "rival prima donnas". (Lewis, 1976, p. 52). They did have different conceptions of 'function'. For Malinowski, 'functionalism' was based on "biological and psychological imperatives", and the various social institutions were connected to these and 'functioned' to satisfy them. (Voget, 1973, p. 30). Thus, 'function' was "the equivalent of need". For Malinowski:

"all sociologically relevant impact and interaction are organised, that is, they occur between institutions."

(p. 30)

While Voget observed that Malinowski was:

"inclined to universalise the basic structure of human nature as well as the institutions to which it gives rise."

(p. 31)

He added that in one respect Malinowski distinguished between "primitive" and "civilised" societies, because he saw economic exchange in the former as not being based on "utilitarian" motives, (which seemed to be ascribed to the latter) but rather on the principle of reciprocity. Leach did not mention the primitive/civilised distinction. According to him, Malinowski saw this principle as being found in all social behaviour, and that he recognised it as a form of "communication", because "it not only does something, it says something". (Leach, 1976, p. 6). What is expressed in the principle of reciprocity is social relationships, whether of equality, superiority or inferiority in status. It thus had a social function.

Mauss expressed a similar view in his work on gift exchange. (Mauss, 1954). Scott Cook observed that Malinowski and Mauss were the:

"first in demonstrating how gift exchanges create, symbolise and maintain status relations."

(Scott Cook, 1973, p. 825)

However, he added that Raymond Firth, one of Malinowski's students, was the

first to deal "analytically with the theoretical economic problems of gift exchange". (p. 825). The concept of 'reciprocity' in social relations was also ascribed by Levi-Strauss to Radcliffe-Brown as well as Mauss and Malinowski. (Levi-Strauss, 1968, p. 161).

That this principle does operate in society was shown by Blumenstiel in his amusing discussion of courtship. (Blumenstiel, 1973, pp. 208-9). The idea of it, and status relationships, were in mind in this research in relation to the process of trying to establish and maintain 'friendly relations' with the teachers and the 'choice of role', together with the possible effect of these relationships on validity. These are matters discussed in later sections of this chapter.

For Malinowski generally, humans were seen as a form of animal with 'culture' as "man's distinctive form of biological adaptation". (Lewis, 1976, p. 53). For this reason, Voget saw Malinowski as thus producing a "theory of culture rather than a theory of society". (Voget, 1973, p. 26). However, he was functionalist if not structuralist. Voget said of this distinction that:

"The view of culture as an instrument developed by man in his own service leads Malinowski to insist that he is not a structuralist. However, he seems to accept the determinism of custom and insists that native peoples are unaware of the causes of their actions."

(Voget, 1973, p. 31)

This seems rather like the contradiction noted by interactionists between man as a creative individual yet constrained by the influence of past attitudes, as noted by Thomas.

Radcliffe-Brown, unlike Malinowski, saw 'function' as a social fact which related to the maintenance of a 'social structure'. He followed Durkheim's view of society, and was influential, during a six year stay in Chicago, in the introduction of Durkheim's work to the United States, and thus in the development of 'structural-functionalism' as the dominant

sociological theory from the late 30's to the 60's, a position noted in the first part of this section.

After 1937, Radcliffe-Brown's ideas influenced many students who had trained under Malinowski. (Voget, 1973, p. 29). Thus 'structural functionalism' became the major 'paradigm' in British social anthropology. Radcliffe-Brown is credited with the conception of 'structural functionalism' through his linking the idea of 'function' with that of 'structure'. (Lewis, 1976, p. 56). Like Durkheim, he was also concerned to develop a 'scientific' study of society, He:

"made constant reference to uncovering the social laws by which various social structures are organised and function."

(Voget, 1973, p. 28)

Both Malinowski and Radcliffe-Brown, though 'functionalist' in different ways, were concerned either with 'institutions' or 'systems' and less with individuals and were thus 'holistic'. Nevertheless, as anthropologists, they were concerned to discover how the people they studied explained their own society and were thus foremost in developing 'fieldwork' of the participant-observation kind, where the social anthropologist lives for a long time with the people being studied. Pelto and Pelto observed that in fact Malinowski was credited with:

"Being the originator, or at least the major developer, of the style of fieldwork that involves intensive and long-term immersion in the daily lives of native peoples."

(Pelto and Pelto, 1973, p. 243)

Lewis observed that in studying 'social life', social anthropologists took a "holistic, comprehensive, catholic approach". (Lewis, 1976, p. 19). What this meant in practice in terms of fieldwork was summed up by La Fontaine, who stated that:

"Fieldworkers will usually have some technical problems in which they are interested, but they will always be studied as part of a total system. The second objective is to compare the system, or parts of it, with others, relating the analysis to a general body of knowledge about society, testing previous conclusions and thus hoping to advance our understanding further. Each piece of research is thus part of social anthropology as a whole and is not merely a way of recording quaint customs and communal ways of life"

(La Fontaine, 1985, p. 19)

This has a distinctly 'objective' and 'scientific' ring to it. However, La Fontaine also stated that "ethnocentricity" was the "central dilemma" of social anthropology. This term meant that:

"We are all ... members of a particular society and this must influence the way we think of other societies."

(La Fontaine, p. 20)

Thus, social anthropology, if structural functionalist, was aware of the problem of bias.

The idea of ethnocentricity can also be applied to members of one social group researching others within a 'civilised' society. These, whatever their theoretical orientations, have similarly to be aware of the possibility of bias for this reason, and the possible consequences for validity. The concept also seems relevant when considering how actors being studied view other social actors. That is, in infant schools such as Moorland, teachers, as members of one 'social class', may view pupils who are 'working class' in terms of their own affiliations. Thus, 'ethnocentricity' has a number of applications.

One aspect of anthropology which seemed important was its relationship to history. Radcliffe-Brown considered that in social anthropology both:

"historical and theoretical explanations were valid. But for some reason he argued that anthropologists cannot do historical work, because primitive people lack writing."

(Hudson, 1973, p. 119)

Hudson argued that Radcliffe-Brown was influenced as much by Spencer as by Durkheim, and that:

"Armed with functionalism, he rejected the "conjunctural history" of earlier anthropologists, ruling out history as a proper object of study for anthropologists."

(p. 119)

Hudson noted that because of this, the 'fallacy of the ethnographic present' had been attributed to Radcliffe-Brown. He observed also that the latter and Malinowski were seen as regarding "traditional notions of history in a primitive society" as being only: "ideological by-products ... a reflex of social structure". (p. 119)

Evans-Pritchard, a student of Radcliffe-Brown, developed a different conception of history, as well as attacking 'positivist' research. Evans-Pritchard argued that:

"conceiving of societies as natural systems and of sociology as a science has undesirable theoretical implications."

(Hudson, p. 121)

He saw social anthropology as a humanitarian subject, like "art or philosophy". Anthropological research was a form of writing about history, in all except the first stage of living amongst those being studied, where the historical 'document' was actually produced. On leaving the 'field', the experience is reflected upon, and 'translated' with reference to the researcher's own culture. Finally, the account is compared with those of other societies by other anthropologists. According to Evans-Pritchard, having produced the 'document' the remaining work is similar to that of an historian. (Hudson, pp. 121-2).

Evans-Pritchard also observed that as a consequence of social anthropology's separation from history under 'positivistic' structural-functional attitudes, social anthropologists did not receive training which would give them "the critical skills necessary to exploit documentary sources effectively" (Hudson, p. 122), and this was necessary because such sources were often the only means of studying some peoples. Also, not considering history in relation to 'primitive' peoples made any account of their societies present them as static, which was a false impression. A third consequence was that social anthropologists did not examine "an interesting series of problems" relating to "folk history" or "the factors that govern a society's choice of the events that are to be committed to tradition". (Hudson, p. 122).

Hudson also noted that Evans-Pritchard's criticism that Malinowski and Radcliffe-Brown were 'ahistorical' was attacked by Ian Schapera, but stated that this attack missed the point of Evans-Pritchard's view, which was that "preliterate people" had a history in the same sense as literate

societies, they were not a special class.

Hudson also noted that some British social anthropologists "have refused to take part with this positivist-humanist controversy" with Ian Lewis seen as one such. (Hudson, 1973, p. 123).

The argument about the place of history in anthropological research may not seem relevant to sociological research in a literate society. However, training in the 'critical skills' required for studying historical documents is not necessarily part of a sociologist's training either. Where, as in the present research, some historical study is deemed to be necessary, mistakes in interpretation of documents and other sources can easily be made. Moreover, the context in which documents were originally produced has to be considered. It was the view of this researcher that some of the ideas expressed about infant education in the research could only be 'understood' by a reference to history, particularly when some of the people studied referred to the 'history' of their school, a kind of 'folk tradition', in different ways. Without some attention to history, the 'fallacy of the ethnographic present' may be a fault of sociological accounts. It was a contention of this research that 'interpretive' methods were able to consider history, which in one sense involves an 'interpretive' account of the past, studying ideas held by particular people at a particular time.

Thus far, 'social' anthropology has been discussed. As stated earlier, American anthropologists were mainly 'cultural'. This tradition was largely begun by Franz Boas. He is seen as being the 'father' of American anthropology. Voget stated that he "gave immediate direction to American cultural historicism", (Voget, 1973, p. 33), while Colby and Peacock stated that:

"American anthropology began in earnest with Franz Boas" [he]
"set standards for ethnographic work which have been matched
only by isolated individuals in the past."

(Colby and Peacock, 1973, p. 615)

Voget also noted the early lead taken by Boas in fieldwork. Boas was not, however, a 'fieldworker' of the Malinowski type. He was stated to have kept at a distance from the daily lives of the people he was studying, and to have scarcely practised "his own advice" to learn the native language. He relied more on key informants and usually did not actually live in the community but stayed in a hotel. (Emerson, 1983, p. 3).

Boas's original background was in the natural sciences, for he was trained as a "physicist" and "geographer". (Levi-Strauss, 1968, p. 8). He was concerned to apply the rigorous methodological standards of science to the study of anthropology, speaking of 'the science of anthropology' being applied to "the history of human society". (Voget, 1973, p. 33).

Scholte also noted that Levi-Strauss praised Boas as "the founder of anthropology's practical and theoretical bases". (Scholte, 1973, p. 656). Levi-Strauss himself noted that Boas ascribed "a scientific aim and universal scope to anthropological research". (Levi-Strauss, 1968, p. 8).

Thus, both in social and cultural anthropology there was a stress on a "scientific approach" and methodological rigour, rather than simply a description, or a focus only on subjective meanings in spite of living amongst the people studied and taking note of their accounts and explanations. This again has implications for the idea of theoretical 'purity' in relation to methods.

The main thrust of the work of Boas and of his students was against any idea of evolution or inherent psychological traits as determining 'culture'. Bourguignon noted his opposition to early ideas around the turn of the century that:

"differences in the behaviour of human groups were seen as based on an evolution of human institutions, reflecting in turn the evolution of human biological capacities."

(Bourguignon, 1973, p. 1081)

She also stated that he was opposed to Bastian's idea of the 'psychic unity of mankind', the idea that human beings had to produce "similar fundamental ideas". (p. 1085).

Boas rejected any idea that "the same phenomena" were "always due to the same causes". (p. 1085).

Thus, he not only held that custom was not "determinist", as Voget noted, but also as Alland and McCay observed, that no factor, whether "racial" or "environmental" could be seen as "the direct cause of a particular behavioural system or cultural trait". (Alland and McCay, 1973, p. 162; Voget, 1973, p. 39). Thus Boas and his students, reacting against any idea of determinism, developed "a radical cultural relativism". (Bourguignon, 1973, p. 1081).

This aspect of 'cultural anthropology' can be seen as fitting in well with the ideas of Mead and other interactionists, who similarly held a non-deterministic view of the relationship between individuals and society.

As against an evolutionary concept of culture, Boas took a 'diffusionist' view. The basic idea of diffusionism is that:

"where you find the same or similar techniques you propose the existence of some form of cultural connection or 'borrowing'"

(Lewis, 1976, p. 60)

Thus, Boas looked at various tribes in neighbouring geographical areas, and studied the distribution of cultural features. But any idea that any such distribution had determinist overtones he rejected, as noted. His view was that cultural traits were "independently diffused", and that instead of "cultural complexes" being adopted wholesale, it was more a matter of "single traits" being taken up, as seemed necessary. Voget summarised this as meaning that:

"Any particular people, according to the structure of their culture, are inclined to take up cultural features and to rework them according to taste and requirements of compatibility."

(Voget, 1973, p. 33)

For Boas, rather than an "identical sequence of events" determining the spread of cultural features, it could be that "detailed historical reconstruction might point towards convergence". (Bourguignon, 1973, p. 1085). Boas was therefore concerned with the study of the processes of "acculturation and dissemination" which were "historical methods". (Voget, 1973, p. 34).

This study involved, as summed up by Levi-Strauss:

"The detailed study of customs and of their place within the total culture of the tribe which practises them, together with research bearing on the geographical distribution of those customs among the neighbouring tribes."

(p. 6)

This had a dual purpose, for through such study the "historical factors" behind their development could be "determined", and also "the psychological processes which made them possible". (Levi-Strauss, 1968, p. 6).

As noted, Boas' view of cultures was that they were "mental phenomena", as Black observed. (Black, 1973, p. 509 and p. 515). Thus, there was choice over which elements of 'diffused' culture would be 'taken up' according to the needs or wishes of the society involved in 'borrowing'. There were thus both 'subjective' and 'objective' elements in such 'borrowing'. Boas was concerned to discover the relationship:

"between the objective world and man's subjective world as it had taken form in different cultures."

(Levi-Strauss, 1968, p. 8)

Boas attempted to 'reconstruct' the cultural past of those without written histories through the use of 'key informants' relating memories, and stories, including myths and folk tales. Pelto and Pelto related that for Boas the actual present of the society could get in the way of this 'reconstruction', giving his account of his disappointment that "another potlach" was being held and he hardly got anything done as a result. They pointed out that this account showed the difference between "informant oriented and observation oriented fieldwork". (Pelto and Pelto, 1973, p. 243). That there can be such a difference implies that care must be taken when 'borrowing' a method.

Both Hudson and Levi-Strauss commented on the rigorous standards that

Boas applied to his work, and taught his students to respect. Hudson was critical of Boas' 'reconstruction' attempts, stating that although he used historical methods he did not actually write history. He stated that:

"Actually, neither Boas nor his students did much historical research in the sense that the BAE did ... and their history was largely inferential."

(Hudson, 1973, p. 117)

The BAE was the Bureau of American Ethnology formed in 1879, and Hudson noted that it published a number of accounts of Indian life which were "basically historical". First hand ethnographic information was produced as well as using documentary sources. (Hudson, p. 117).

Pelto and Pelto observed that:

"strictly historical anthropology, when it is concerned with a people's cultural past, does not necessitate very much living among, or identifying with, the "natives"."

(Pelto and Pelto, 1973, p. 243)

Since immediately afterwards they related Boas' comment about the potlatch, there was an implied criticism of his lack of concern for the 'present life' of the people he was studying.

Levi-Strauss, on the other hand, was more favourable to Boas' attempts at 'reconstruction', although stating that "In the entire work of Boas the result appears to be rather negative". (Levi-Strauss, 1968, p. 7). Levi-Strauss stated that Boas was well aware of "the infinite variety of historical processes" which shaped the "subjective world". He himself stated that "knowledge of social facts" had to be founded on:

"induction from individualised and concrete knowledge of social groups localised in time and place."

(p. 9)

This knowledge could be gained only "from the history of each group. However, in most cases, given "the nature of the subject matter of ethnographic studies such history was out of reach. Levi-Strauss commented on the rigorous standards "of the physicist" that Boas had introduced:

"in tracing the history of societies for which we possess only documents that would discourage the historian."

(p. 9)

His view was that when Boas was successful, the "reconstructions" were in effect "true history". However, Levi-Strauss argued that even so, this was a "micro-history", that of "the fleeting moment", which could not really be related to the past. Nevertheless, Levi-Strauss' conclusion was that Boas' work would continue to dominate "all subsequent development". (Levi-Strauss, 1968, p. 9).

One area where this seems true is in the field of linguistics, the study of the development of language, and through this again on the 'structuralist anthropology' of Levi-Strauss himself.

As noted, Boas had defined ethnology as the study of mental phenomena. Black stated that Boas regarded linguistics as falling within its range. She quoted Boas as saying that language was "one of the most important manifestations of social life". (in Black, 1975, p. 515). He is said to have noted that the "laws of language" were used unconsciously by speakers, (p. 515). Levi-Strauss, also writing of Boas' ideas on language, stated that Boas:

"showed that the structure of a language remains unknown to the speaker until the introduction of a scientific grammar."

(Levi-Strauss, 1968, p. 19)

However, Boas found in respect of language use, that native speakers perceived objects and events in terms of conceptual categories. Boas considered that, according to Black, this showed:

"the necessity of classification, that the infinite variability in actual experience must be grouped into discrete units."

(Black, 1973, p. 515)

Even when grammar was introduced:

"the language continues to mold discourse beyond the consciousness of the individual, imposing on his thought conceptual schemes which are taken as objective categories."

(Levi-Strauss, 1968, p. 19)

Boas applied these ideas about the underlying structure of language as being unconscious, and the necessity in the human mind to classify, to other social phenomena. The study of language could throw light on their similar underlying structures. However, Levi-Strauss quoted Boas as stating that:

"the essential difference between linguistic phenomena and other ethnological phenomena is, that the linguistic classifications never rise to consciousness, and thus give rise to secondary reasoning and to reinterpretations."

(In Levi-Strauss, 1968, p. 19)

However, the two forms of phenomena were essentially the same. But the advantage of the 'linguistic method' was that:

"on the whole, the categories which are formed remain unconscious, and that for this reason the processes which lead to their formation can be followed without misleading and disturbing factors of secondary explanations, which are so common in ethnology, so much that they generally obscure the real history of the development of ideas entirely."

(in Levi-Strauss, 1968, pp. 19-20)

From this, it might seem as if linguistics was seen as a form of 'ideal type' which could be applied to the study of other cultural phenomena.

The influence of Boas on linguistics was stated by several writers. For example, he was said by Levi-Strauss to have made use of his propositions: "in laying down the foundations of American linguistics". (Levi-Strauss, 1968, p. 20). Durbin stated that many early linguists could also have been classed as anthropologists, since they had trained under Boas. His 'psychological approach' was also said to have been important in the later development of this area of study. (Durbin, 1973, p. 454).

Black similarly pointed out his influence, observing that Sapir, amongst others, was a student of his. Whorf, of the 'Sapir-Whorf hypothesis', was a student of Sapir and also was associated with Boas. (Black, 1973, pp. 516-9).

In anthropology, as noted, Levi-Strauss recorded the influence of Boas

in the development of his own formulation of 'structural anthropology'. However, there were many other sources for his ideas apart from Boas. These have been said to include, for example, Rousseau, Freud, Durkheim, Mauss, Marx and Saussure amongst others. (Badcock, 1975; Scholte, 1973). Levi-Strauss did claim to be a Marxist, though not 'of the usual kind', a claim of which Glucksman was sceptical. (Glucksman, 1974). Badcock regarded Levi-Strauss as standing firmly within the French school of sociology, in the Durkheimian tradition, rather than simply an anthropologist. However, he also stated that Levi-Strauss would be seen as having "two sociological methods, not one", one from Durkheim in certain aspects of 'structuralism' and one from Marx in another. (Badcock, 1975, p. 75).

Although Levi-Strauss' version of 'structuralism' has aroused considerable admiration, it has equally provoked considerable criticism and scepticism. (Glucksman, 1974; Leach, 1974b; Mennell, 1974; Scholte, 1973).

It is not the intention here to discuss 'structuralism' in detail, for it is a complex theoretical 'structure' with equally complex terminology. Several books have been written by Levi-Strauss, and several by critics. This researcher has neither the time nor the inclination to produce another critique. However, in reading both Levi-Strauss and some of his critics, as part of looking at anthropology, some of the basic ideas seemed relevant for this research. As with social and cultural anthropology, the main concerns of 'structuralism' are summarised. As with other aspects of necessity such a summary misses out much that is important, but this cannot be helped, and is especially inevitable with such a difficult area. The intention is thus to pick out the main themes, commenting as elsewhere in terms of the research, and also to indicate that 'structuralism', as anthropology, is concerned with some basic dichotomies associated with sociology, such as the subjectivist/objectivist, the individualistic/holistic, the relationship between nature/culture, and the static/dynamic distinction.

Structural anthropology is based upon a number of ideas. However, its overall aim seems to be the examination and analysis not of what is observed or related or used, that is, of various cultural forms, but of the underlying 'structure' of these.

By 'structure' Levi-Strauss meant a model by means of which such analysis could be undertaken. To count as 'structure', a model had to have certain characteristics. First, it should be a "system". That is, it should be made up of "several elements", none of which could be changed independently. Secondly, it should be possible to order "a series of transformations resulting in a group of models of the same type". (Levi-Strauss, 1968, p. 179). The concept of 'transformation' in relation to analytic comparison basically means that the elements of the structure of one aspect of culture may be converted or changed or 'transformed' into those of another in some way, perhaps as inversions or oppositions.

The third characteristic of a 'structure' was that, given the first two, it should be possible "to predict how the model will react if one or more of its elements are submitted to certain modifications". (Levi-Strauss, 1968, p. 179). Lastly, the model should be such that it would "make immediately intelligible all the observed facts". (p. 180). Since it is less concerned with what is directly observable and more with underlying 'structures', structuralisation is a theoretical or 'rationalist' rather than an empirical approach to the study of cultures (and societies) and relates more to super-structure rather than the existing 'social structure' itself.

One idea in structural anthropology is that which was suggested by Boas, that the 'conscious' explanations used by members of a society for cultural phenomena may be 'rationalisations', rather than 'real' explanations, or, in Levi-Strauss' terms, 'conscious models' may act "as a screen" to hide "the structure of a certain type of phenomena". (Levi-Strauss, 1968, p. 281). However, such models might be accurate, but whether they were biased or not,

such "cultural norms", although not 'structures' in themselves, did:

"furnish an important contribution to an understanding of the structures, either as factual documents or as theoretical contributions similar to those of the anthropologist himself."

(p. 282)

This seemed an important idea, though in a somewhat different sense to that which Levi-Strauss was concerned with. In relation to actors' 'subjective meanings' and explanations, actors can sometimes say why they do things, as Marsh stated, a view mentioned in the previous part of this section. However, she also pointed out that such explanations need not be accorded a higher status than other explanations. (Marsh, 1985).

If actors give explanations, it does seem as if these are something to be studied, whether they seem accurate or not. Levi-Strauss stated that where 'conscious models' were not, where there were differences between what was said and what was done, that: "the very bias and type of error are a part of the facts under study". (p. 282). So 'subjective' explanations should not be taken at face value as a description of what actually is happening, as perhaps some 'interpretive' sociologists, in rejecting 'objective' explanations, seem to be suggesting. However, Levi-Strauss seems to go further and to be arguing for an objective rather than a subjective approach, with the subjective something to be analysed in terms of a model. According to Glucksmann, Levi-Strauss considered that:

"structure is not observable and cannot be perceived or discovered by the senses. It has a real existence, but cannot be reached by fact collection."

(Glucksmann, 1974, p. 234)

They can only be reached by the analyst, it seems.

The idea that there are underlying 'structures', which can be discovered and compared, is based upon the view that there are universal properties of the human mind which 'structure' all cultural phenomena. So structuralism seeks to analyse such phenomena in terms of the way that these show such 'deep structures' of the mind. According to Levi-Strauss, the human mind is

neurologically programmed to perceive everything, such as the distinction between the self and others, between humans and animals, and between different forms of these, in categories based on a series of binary oppositions, which can be arranged in terms of pluses or minuses, or positive/negative distinctions, and which can be combined and recombined in various patterns. As Scholte, amongst others, pointed out, this conception of the mind has links with cybernetics and mathematics. (Scholte, 1073).

As a conception, it seems closer to Kant's 'a priori' view than Durkheim's 'collective consciousness'. Bottomore and Nisbet described it as a "Neo Kantian perspective" although they also stated that other parts of "Structural Anthropology plainly belie this". (Bottomore and Nisbet, 1979, p. 582).

If Levi-Strauss' conception is true, it could account for the propensity of individuals to 'typify' others according to the presence or absence of some characteristic.

'Structuralism' is based upon the view that various cultural phenomena can also be considered in terms of such binary oppositions, or the main categorising processes of the mind. Thus, for Levi-Strauss, the relationship between nature and culture is that the two are not opposed but interwoven necessarily. Real differences, such as those between animals, are perceived in the mind, which is material and so itself part of nature, and are then conceptualised and categorised in terms of such oppositions, and then transformed into cultural phenomena such as 'totemism', through combining the basic elements in new forms, into 'signs' or symbols. Structuralism uses the concept of signs found in linguistics. A sign is a representation of:

"a concept, or an idea, but is not in itself a concept or idea. Similarly, it is derived from perceptions of the world - it is something - but, in the new use to which it is put it is not that thing."

(Badcock, 1975, p. 46)

Signs have symbolic significance in terms of the related conventions or patterns or systems of related concepts, such as red in traffic lights standing for danger and thus meaning 'Stop'.

Another structuralist argument is that, in analysing cultural phenomena, attention should be focused not on the constituent elements but on the sets of relationships between these, because these indicate the real structure. Thus, attention is paid less to real social interactions of individuals than to what they reveal, to the analyst, of the underlying 'deep' structure. Thus, the structuralist interest is holistic rather than individualistic.

One of the most important ideas of structuralism is that other cultural phenomena besides language are communication systems. Levi-Strauss considered art, religion, myth and marriage and kinship systems as examples. Also, he saw the latter as based upon reciprocal exchange, like the Malinowski/Mauss principle in economic exchange. Badcock commented that the principle of reciprocity linked individuals to society, thus solving the Durkheimian problem of "static functionalism". (Badcock, 1975, p. 31).

As previously noted, Leach also ascribed to Malinowski the idea that this principle was found in all social behaviour, and that he also recognised it as a "mode of communication". (Leach, 1976, p. 6).

As 'communication systems', such cultural phenomena can therefore all be analysed in the same way that language can:

"into constituent elements ... which can be organised according to certain structures of opposition and correlation We should then be able to construct a chart with + and - signs corresponding to the pertinent or non-pertinent character of each opposition in the system under consideration."

(Levi-Strauss, 1968, p. 86)

The actual cultural phenomena he was analysing in this way in this instance were French and English cuisines.

Language has as its basic elements phonemes, or signs for sounds, which are combined into words in various ways. Words stand for things. Although the assignment of words to things has an arbitrary nature in the beginning, conventions of meaning develop. However, it is not words but the relationships between them, and their combination into sentences, which is the significant syntactical dimension, and indicates the 'deep' structure.

Language can also, in linguistics, be analysed in terms of the Saussurian distinction between 'la langue' and 'la parole', between the 'rules' of language and its use, or, as Badcock put it, between the 'code' and 'the message'. (Badcock, 1975). Similarly, other aspects of cultural life are also seen as having 'rules', based on different combinations of binary oppositions, which are stable, and usages, which may reflect change. This is the 'synchronic/diachronic' distinction. As applied to language, 'synchronic' relates to 'la langue', or the 'code'. It refers to: "the instantaneous, atemporal and unchanging, the systematic, holistic and functional". (Badcock, 1975, p. 68). 'Diachronic' refers to, in contrast:

"the temporal dimension ... the succession of moments, of the passage of time, of change, evolution, decay and regrowth."

(p. 69)

Levi-Strauss used the distinction in music between melody, seen as the synchronic element, and harmony, seen as the diachronic, to express this relationship between the stable and static and the changing, dynamic nature of cultural phenomena, in his analysis of myths.

However, although language was a system of communication and his theory was that in society cultural phenomena such as art, myth, ritual and religion were other forms of language, Levi-Strauss rejected the charge that he thus reduced everything to language. He stated that:

"To derive from language a logical model which being more accurate and better known, may aid us in the structure of

other forms of communication, is in no sense equivalent to stating that the former is the origin of the latter."

(Levi-Strauss, 1968, p. 83)

Language was simply an example of what he meant by 'structure' as a model.

'Structuralism' also asserts that the concept of structure, the model, may be used to expose and analyse both the underlying regularities and similarities, not only between different cultural phenomena within one cultural group, but also between that and others, using the concept of 'transformation'. This is possible, according to structuralist theory, because the same 'elements', combined into sets of relationships in various ways, are found across 'cultures' because all reflect the same underlying structure of the mind.

These, then, seem to be the main ideas of structuralism, in so far as a brief outline can summarise something as complex and wide-ranging. In trying to summarise, many aspects were omitted, such as the links with Freudian psychology and Sartrean existentialism/Marxism. (Badcock, 1975; Leach, 1974b).

Also left out were an indication of the structuralist analysis of myths, which would be applied to our own folk tales, and also the concept of women as an exchange commodity between men. (Levi-Strauss, 1968, p. 61).

Structuralism is full of interesting ideas, and Levi-Strauss' method of analysis of various phenomena can cause the rethinking of 'taken for granted' aspects of our own 'culture', as indeed anthropology in general can. It has had a considerable impact on anthropology and also sociology, hence its selection as one division to be discussed.

However, it is 'theoretical', concerned less with the visible actual relations of interaction than with the invisible, hidden aspects which postulates. It has previously been remarked that it seemed as if these 'underlying patterns' could only be reached by the analyst. Mennell observed

in relation to this, that Levi-Strauss' theories:

"often involve the wildest generalisations" [and his] "explanations, whether of myths or kinship systems, would fail by a mile to satisfy Schutz's 'postulate of adequacy', being entirely incomprehensible to the native and far from clear to many of his social scientific readers."

(Mennell, 1974, p. 61)

However, Mennell also stated that it was an "essential part" of the work of "social scientists to generalise ... and produce theories" for testing by others. (p. 67). Many 'interpretive' sociologists would accept this idea.

One reason for the production of 'models' and analyses which 'natives' cannot comprehend could lie in Levi-Strauss' 'fieldwork'. Leach observed that Levi-Strauss did not employ the "Malinowski-style intensive fieldwork", which used "the vernacular" and involved long stay with those studied. According to the biographical notes he supplied, Leach seemed to be implying that Levi-Strauss' fieldwork was of relatively short duration. (Leach, 1974, pp. 10-11). He also stated that whereas an "experienced anthropologist" may well develop a model after a few days, longer stay, and learning to use the language, will whittle away at the model, so that the lack of understanding will seem "even more formidable" than in the first few days. Although this researcher did not have a 'model', she felt the discouraging effect of 'understanding' seeming to recede in the longer stay school. Leach remarked that:

"Levi-Strauss himself has never had the opportunity to suffer this demoralising experience and he never comes to grips with the issues involved."

(Leach, 1974b, p. 19)

Leach also stated that Levi-Strauss based his enquiry and most of his subsequent "ethnographic observations" and theories "on the use of special informants and interpreters". (p. 19) Thus, like Boas, he either missed or ignored what was actually happening, or else preferred to accept the verbal explanations of informants as expressing the 'real' relationships and meanings.

Although contrasting Levi-Strauss' fieldwork with that of "nearly all Anglo-American social anthropologists" in his 1974 book, Leach elsewhere compared 'structuralism' at the theoretical level, which he called 'rationalist', with 'functionalism' which he labelled 'empirical'. He argued that the "rival theories" of anthropologists were "themselves part of a single interacting whole". (Leach, 1976, p. 5). He contended that both accepted:

"the central dogma of functionalism, that cultural details must always be viewed in context, that everything is meshed in with everything else."

(p. 5)

Because of this, he considered that the two approaches were "complementary" rather than "contradictory" and that one was "a transformation of the other". (p. 5). As he went on immediately to discuss Malinowski's idea that 'reciprocity' underlies all social relations, he seemed to be implying that the approaches had a reciprocal relationship. This is a neat idea, whether accurate or not. It is not quite clear, though, why stressing the need for culture to be contextualised is necessarily 'functionalist'. That apart, it can certainly be argued, as noted before in the discussion of symbolic interactionism, that there is a good deal of eclecticism in actual sociological 'work'.

With this summary of structuralism, the actual discussion of anthropology in terms of certain divisions comes to an end. Like symbolic interactionism, anthropology has been discussed at some length, though for rather different reasons in part.

One of the reasons was, as with symbolic interactionism, to show the range of views, in a limited way, within anthropology and to outline the principal features of the divisions discussed, and in the course of this to indicate and comment on those ideas which had seemed of relevance for this research. But a major aim was to underline a methodological point already raised in the discussion of symbolic interactionism, and mentioned

earlier in this particular part of this half of the section, that is, the difficulty of finding a 'pure' method. This point was not appreciated when anthropological accounts were first read but became clear on wider reading.

Thus, in this account, the links between anthropology and other subject areas was noted, particularly that with sociology. It was observed that anthropology has experienced the same arguments as sociology, and that, there has been much 'borrowing' between the two. The concepts of ethnography and ethnology, seen as important related terms for anthropology, were defined. The division of social and cultural anthropology and structuralism were outlined. In respect of ideas, it was observed that in relation to the first two both 'structural' and 'cultural' elements required equal study.

The issue of ethocentricity was also raised in respect of study within one's own society, and the related possibility of bias which might affect validity, and also in respect of judgements used by the people being researched.

The relationship of history to anthropology was mentioned, and the lack of training in historical skills noted by Evans-Pritchard was also observed to apply also to sociologists, and that historical research was sometimes necessary.

The concept of the principle of reciprocity as underlying social relations was also seen as relevant for field relationships.

From structuralism, the ideas taken were that if there are universal characteristics of the mind, this might account for a tendency towards typification, something found in the research at times. Also, the view that the explanations of actors are as much a matter for examination as any other feature in the research and cannot therefore be accepted at face value.

As noted at the beginning of this discussion of anthropology, it was first

looked at for the accounts of participant observation it gave, that is, the original interest was methodological. Many of the accounts were interesting in themselves, and the style was pleasing. They seemed in many cases more fresh and immediate than some sociological texts. These accounts included, for example, Spindler (1970), Wax (1971) and Srinivas, Shah and Ramaswami (1979) among others. Most are mentioned elsewhere in this chapter.

Not being an anthropologist, the existence of various theoretical divisions (and various sub-fields) was not fully appreciated, although the existence of 'structural-functionalism' and 'structuralism' was recognised. The latter in particular was not originally considered. Theoretical issues were not the first concern, as far as anthropology was concerned. However, interest led to wider reading, and it was in the course of this that the existence of a number of theoretical differences between anthropologists became evident, despite the fact that they appeared to be using the same method. Later it was recognised that there were differences even here. The first of such differences brought into question the idea of 'borrowing' a method. This was then a primary reason for writing of anthropology at some length, apart from it being an area of general interest and one having some ideas relevant from this research.

The aim was to show the differences in views of anthropologists, to indicate that the same method can be used despite these, just as surveys are not necessarily carried out by 'positivists', something Marsh noted. (Marsh, 1985).

It thus seems illogical to say that a certain method must not be used because it is 'tainted' with a particular view of 'society'. Anthropology's 'method' could also be considered as 'tainted' in the same way, either because of its 'structural-functionalism', or because of its regard (on both sides of the Atlantic) for 'scientific study' as well as for the explanations

of members of 'societies', for "theory" as well as "data gathering and description". (Frake, 1983, p. 62).

Even if methods were 'tainted' through alliance to particular views, which is open to doubt, it would not follow that they would not be used. Lewis' comments on Levi-Strauss' "notions" seems relevant for this. He stated that:

"it is legitimate to utilise Levi-Straussian notions where these seem appropriate as it is to derive inspiration from Freud - without necessarily being a dogmatic, doctrinaire Freudian."

(Lewis, 1976, p. 66)

This seems an important counter to the argument that methods should be rejected on ideological grounds. If ideas can be 'borrowed', where they seem useful, as they have been in this research, then so can methods, even if these were tied to particular views. After all, buying an article in a shop does not imply an interest in taking over the establishment.

Thus, methodologically, and theoretically, a pragmatic eclectism would seem to be the most sensible approach, providing it is based upon knowledge and not 'taken for granted' assumptions that one is superior to another.

However, there are separate methodological issues connected with 'borrowing' quite apart from any theoretical attachments. These are some problems raised by Lutz in relation to 'ethnography'. As noted, he objected that a "field survey" or a "brief encounter", which might be for some hours a day over a year or for longer hours over a shorter period was not 'ethnography'. He defined activities like these as 'ethnographic'. Ethnography meant living and working in a community over an extended period.

Lutz stated that 'ethnography' and 'ethnographic' methods had "become increasingly in vogue among educational researchers in the last decade". (Lutz, 1986, p. 107). He attributed this to the:

"commitment to rediscover the individual as an active creator
and constitutor of the social world"

(p. 131)

which had apparently been lost within 'structural-functionalism'.

His objection to this trend was that a consequence had been some
"modification" of ethnography, and it had occasionally been "bastardised".
This had resulted in, he argued:

"the production of poor research and ethnography by people
who, untrained in ethnography, claim to engage in it."

(p. 108)

Thus, Lutz was arguing that, in 'borrowing' from anthropology,
'ethnography' had been changed. Now, this may well be true. Spindler
made a similar complaint, in writing about the popularity of ethnography
in relation to schooling, which he indicated was something of a "fad". He
stated that therefore:

"It is not surprising that some work called 'ethnography'
is marked by obscurity of purpose, lax relationships be-
tween concepts and observation, indifferent or absent
conceptual structure and theory, weak implementation of
research method, confusion about whether there should be
hypothesis and if so, how they should be tested, confusion
about whether quantitative methods can be relevant, unrealistic
expectations about the virtues of 'ethnographic' evaluation,
and so forth."

(Spindler, 1982, pp. 1-2)

This researcher would agree with the idea that confusion exists. However,
any changes in 'ethnography' in the translation to sociological research
cannot simply be attributed only to a lack of training by non-anthropologist/
ethnographers, because many anthropologists have observed that they did not
receive much training either. Hammersley and Atkinson reported that this
had once been a tradition with North American anthropologists. (Hammersley
and Atkinson, 1983, p. 27). It also occurred with British ones.

Frake argued that "the state of the art" was not very "different today
than it was a decade ago". He added that "Training in fieldwork techniques
is, if anything, even more neglected". (Frake, 1983, p. 61).

Wolcott indicated the thinking behind this 'tradition'. He stated that he:

"would hold that ethnography is best served when the researcher feels free to "muddle about" in the field setting and to pursue hunches or address himself to problems that he deems interesting and worthy of sustained attention. Citing Malinowski for chapter and verse, many anthropologists are content to embark on new fieldwork guided only by a "foreshadowed notion" of problem areas that may prove interesting. One of the most satisfying aspects of this traditional approach is that one is free to discover what the problem is rather than obliged to pursue enquiry into a pre-determined problem that may in fact exist only in the midst of the investigator."

(Wolcott, 1975, p. 113)

This view is in part a recognition of the fact that no 'training' can ever prepare anyone for all the situations that may be encountered, so a researcher can never be completely trained, nor as Hammersley and Atkinson argued, can this form of research "be programmed". (p. 28). Frake also pointed out that:

"The conditions of fieldwork are so varied that what works well in one situation may be impractical or even dangerous in another. Being a good fieldworker depends on qualities of sensitivity, adaptability, and insight that are difficult to train for or to identify in advance."

(Frake, 1983, p. 63)

However, it is one argument of this researcher that some form of "advice" or "training" would help reduce the committing of unwitting errors that can detract from the research, and that this 'advice' should be given by those who have themselves carried out 'ethnographic' work. Hammersley and Atkinson argued that there was a need for "pre-field work preparation", and that because research could not be "pre-determined" it did not follow that: "the researcher's behaviour in the field need be haphazard, merely adjusting to events". (Hammersley and Atkinson, 1983, p. 28). Frake similarly argued that there were some things that could be "taught but rarely are". These "techniques of general utility" included:

"ways of recording and organising field notes, elementary mapping, procedures ... interviewing strategies, sampling techniques, etc."

(p. 63-4)

Frake's view was that although:

"There are no shortcuts to good ethnography, there ought to be some way to insure accumulated improvement in the quality of ethnographic work."

(Frake, 1983, p. 63)

Hammersley and Atkinson argued that besides the recognition that research entailed unknown factors that could not be foreseen, there was a second reason for 'non-advice', which was "less legitimate". Anthropologists were told to "go and do it" because of:

"the idea, associated with naturalism, that ethnography consists of open-ended observation and description, so that 'research design' is almost superfluous. Here, one useful research strategy is inflated into a paradigmatic approach."

(Hammersley and Atkinson, 1983, p. 28)

It is another argument of this researcher that 'ethnography' is more than "observation" and "description", though agreeing with Frake that "the condescending "mere" often "used before "description" should be eliminated". (Frake, 1983, p. 60).

Atkinson and Delamont, who were referred to in the first part of this section with reference to their criticism of 'crude stereotypes' of research traditions, and their argument that "ethnography" was concerned with "generalisation", went further, They stated that:

"Anthropologists do document the particularities of given cultures and communities; but they do much more than that. The same can be said of relevant traditions in sociology, such as symbolic interactionism."

(p. 249)

They added that:

"In fact, the development of ethnographic work in sociology and anthropology rests on a principle of comparative analysis."

(p. 249)

In their view, if studies are not built into "more general frameworks", then they will remain "one off" and there will be therefore "no sense of cumulative knowledge or developing theoretical insight". (Atkinson and Delamont, 1986, p. 249) That is why it seems important to relate one piece of work to others, to show how it may support or oppose previous research.

Thus, 'ethnography' cannot just mean 'observation' and 'description', however crucial these are in the first place. As far as anthropology is concerned, Frake commented that, as noted, it was concerned with theory and science:

"The tradition of the profession tells us that data gathering and description is not enough. We must have theory. Anthropology must be science, not natural history."

(Frake, 1983, p. 62)

However 'science' is considered, it is concerned with the growth of knowledge, and any research should have some concern with this, in some way.

Apart from the implication that 'ethnographers' in anthropology were 'trained' while those in sociology were not, and that this was the cause of the "poor research" which he claimed had been produced, Lutz may be partially correct in distinguishing between 'ethnography' and 'ethnographic', even if wrong on the first point.

Whether an 'ethnography' of schools, in the strict anthropological sense, can be produced, is an issue to be considered. Unless they are 'total institutions' schools are not a 'community' in that sense. Teachers and pupils (and researchers) are not related, nor do they live together over a long period and share in daily life outside school. Nor, unless it is a village school, and sometimes not even this, with changes in rural life, is a school necessarily part of a 'community', for not all children in the area may go there, nor may it be a focus for 'community' activities, with everyone having some involvement on equal terms.

Nor is a school owned by the 'community' except in a very wide general sense.

The parents of the children do not own it, nor have much say effectively in what goes on, nor is it the property of the teachers, if a State school. The 'community' itself is ambiguous in a modern industrial society, for it is

not a single whole as it may be in some 'primitive' societies.

Even if a school is considered as part of a 'community', concentration on this one feature might not be 'ethnography' as understood by anthropologists. These are usually, although not always, interested in the whole set of activities. In 'ethnography in educational research' it is not usually possible to both work within the school and study the lives of teachers, pupils and parents outside school, and also study the link between those and 'social institutions' such as work, unless more than one researcher is involved in one project, which is seldom the case. Therefore 'ethnography' in relation to education can be limited by these factors, so Lutz may have a point in calling work of this kind 'ethnographic'.

Lutz in fact argued that "ethnographic" methods in small-group situations or "Face to Face Analysis of Social Interaction", tended to:

"limit the observations/data analysis and the theoretical framework to ... micro-ethnography, as contrasted with the broader notion of ethnography as usually applied in the field of social or cultural anthropology."

(p. 104)

His basic suggestion here is that in so doing, the concentration on a:

"narrow focus, while generating some important knowledge, fails to shed light on the more complex issues that account for much of what goes on (or doesn't go on) in schooling."

(Lutz, 1986, p. 109)

Lutz is assuming here that all anthropological ethnography was the same. However, it was noted that there were differences here, such as that between the use of a few 'key informants' and a relatively short stay, and the greater use of observation and an extensive stay, and a concern with what was actually happening. These are in the Malinowski versus Boas interpretations of ethnography, which Lutz glosses over.

Lutz also appeared to be presenting in this last argument, albeit in a rather different form, the view that 'interpretive' methods in micro situations cannot deal with 'macro' or 'structural' issues. This argument has been rejected when discussing symbolic interactionism. The comments about

the 'ethnography' of a school given above do not imply that there is anything inherent in a concern with 'micro-issues' (or one 'institution') that prevent 'structural' features being also a concern. It was merely pointed out that there are limitations on what one researcher can do, not that these factors cannot be raised in the research. That 'observations' may be limited to one 'micro-situation' does not have a necessary link with "a theoretical framework" that prevents such issues being considered, even if they are not, always. In this research it is argued that features outside a school have an effect on what happens inside it. Teachers and pupils both have a reference system outside the school, and the school itself is the embodiment of 'structural' concerns, since it is situated within a network of local government and central government regulations, and is thus a focus of political and economic interests. The curriculum of schools, even infant schools, which is discussed in Chapter Six, reflects this fact.

Therefore a wider reference than the school itself is necessary. Teachers themselves referred to outside factors which constrained their internal activities. This concern for outside school features does not entail 'positivistic' views, even if that term was unambiguous, nor Marxist or any other 'ism', any more than Lewis borrowing a 'notion' from Levi-Strauss or Freud meant that he was committed to either theory. It is only a recognition that schools do not exist in a vacuum. A concern with 'micro' features, where enough questions are asked, indicates the existence of these, even if a researcher cannot provide a full 'ethnography'.

These methodological comments bring to an end the discussion of anthropology. It has been shown why anthropology was looked at originally and why it was discussed at length. It was shown, as symbolic interactionism does, to have a number of theoretical strands. Some of these have been discussed, and the ideas from them which were seen as relevant for this research were indicated and summarised earlier. The methodological points raised through looking at anthropology have been commented on. These were

the idea of theoretically contaminated methods, first noted in the discussion of symbolic interactionism. It was argued that because anthropology makes use of a particular form of a means of acquiring data, despite its differences, that such an idea was illogical. It was noted, however, that in 'borrowing', ethnography may have changed, but that this was not due to non-anthropologists being untrained, since anthropologists often were not. The argument that training would be useful were presented. The issue of whether an 'ethnography' of schools was possible was raised. It was concluded that in the strict sense it was not. However, this was not because of a concern with 'micro' features and 'face-to-face interaction', it was a matter of lack of time rather than a fault of the method or 'theoretical' framework. The method was not seen as determined by such a framework. The 'wider concerns' are not necessarily absent either for the researcher or the researched.

The next strand of the 'interpretive tradition' to be discussed is phenomenology. One reason in particular for doing so is that Sharp and Green were supposed to have used a phenomenological approach and found it wanting. Certainly in their "Theoretical Considerations" chapter they wrote of "the phenomenology" and "sociological" and "social phenomenology". (Sharp and Green, 1975, pp. 19; 21).

However, the researcher found their discussion of 'phenomenology' a little confusing, because it was not clear what they meant by the term, or how they distinguished it from other approaches. For example, when discussing the "new tradition" then emerging in the sociology of education, they noted that there were various strands, such as "symbolic interactionism, phenomenological, sociology and ethnomethodology". They considered that these had certain ideas in common. (Sharp and Green, 1975, p. 3). This is true up to a point, but there are also differences, which is partly what this part of the section is designed to show. Cuff and Payne also commented, in relation specifically to symbolic interactionism and ethno-

methodology, that these had some "superficial similarity". However, they added that there were "marked differences in ethnomethodology as an approach". These stemmed from "different assumptions about the nature of man and his social world" (Cuff and Payne, 1979, p. 121).

They related these differences to the philosophy of Husserl and to the work of Schutz, both associated with the development of phenomenology. by implication, phenomenology was therefore also different from symbolic interactionism. However, when Sharp and Green were writing about "social phenomenology" in their second chapter, their references included, apart from Schutz, Garfinkel and Cicourel, who would generally be considered ethnomethodologists, and Becker, who is usually classed as symbolic interactionist. (Sharp and Green, 1975, p. 20). Hence, the impression the researcher received from reading Sharp and Green's account, apart from confusion, was that these authors had not really discussed phenomenology, particularly as they mentioned nothing about its origins. Thus, in being critical of "social phenomenology", it was considered by the researcher that they may have been as unfair to this as it was felt that they were to some sociological work in the 'old' tradition. (Sharp and Green, 1975, pp. 2-4).

Therefore, it was decided that phenomenology had to be looked at again. The researcher had done some reading in this area before, but it had not been studied very much, because it was at that time outside her field of interest. Also, it was found quite difficult, a problem which remained on returning to it. The difficulty was found not just because phenomenology is largely philosophical, although German philosophy has its problems for non-philosophically trained English students, but was mainly due to the density of the language and some of the terminology. The researcher is far from convinced that she understands it completely even now, which makes criticism difficult. Some sympathy was felt for Bauman's comment, that:

"The essential definitions of phenomenology surround its territory with a dense line of turrets and moats which render its methodological fortress invulnerable. One

can agree with Fink or Scheler that one cannot understand phenomenology without being a phenomenologist, and that once having become a phenomenologist, one can view with equanimity invasions coming from outside, they are doomed to peter out the moment they break into the fortress."

(Bauman, Z., 1976, p. 48)

He was, however, writing of the problem of criticising phenomenology which arose not through a lack of understanding, but because it was difficult to do so except in its own terms, which set it apart from empirical testing. Quite apart from these problems, another difficulty found was that phenomenology is not a single theoretical/philosophical concern, but one with several strands, not all of which appeared to have sociological connections. So choices had to be made as to which 'phenomenology' should be considered. Wolff noted this problem when he stated that he wrote his chapter on phenomenology "on the assumption that, for sociologists the significance of phenomenology is tantamount to its sociological usage". (Wolff, 1979, p. 499). He based his discussion on the work of Husserl and Schutz. In this section, like Wolff, the more philosophical existentialist phenomenology of, for example, Merleau-Ponty and Sartre is also not considered. The discussion centres upon the work of Schutz, since this was seen as the principal sociological area of interest.

The attempt is made here to trace the development of Schutz' phenomenology by referring to what seem to be the sources of his ideas, and then to state what seem to the researcher to be the main points. It is extremely difficult to summarise either the sources, particularly Husserl, and Schutz himself, because the work of both these two was complex and lengthy, with their ideas undergoing many changes during their lifetimes. Summarising cannot do justice to either. However, it is inevitable given that phenomenology, though important because of Sharp and Green, is only one of the areas considered. Also, the researcher is concerned to show what she has made of it, or failed to make, as with positivism, as concisely as possible.

So far 'phenomenology' has been mentioned without a definition. Pivcevic stated that:

"The word 'phenomenology' derives from phainomenon (phainomai, to appear) and logos (reason) and among phenomenologists much significance is attached to this etymology. Whatever 'appears' in concrete experiences; there is no 'unexperienced' appearing. Accordingly, the aim of phenomenology is described as the study of experiences with a view to bringing out their essences."

(Pivcevic, E., 1970, p. 11)

It can also be considered as being concerned with the problem of the nature of consciousness, of meaning, and of knowledge. These concerns, in their modern form, are generally considered as originating with the work of Husserl, although the term was used by Hegel. Farber noted that "Phenomenology has come to be generally known as referring to Husserl's philosophy, despite Hegel's use of the term". (Farber, 1966, p. 1).

Pivcevic also stated that phenomenology as developed by Husserl meant something different to its usage by Hegel. (Pivcevic, 1970, p. 12).

Because the researcher was mainly interested in Schutz, this discussion does not begin with Husserl. Although Schutz did take many ideas from Husserl and develop them, there were other sources. He actually began with an examination of Weber's idea of the need to take the subjective meanings of actors into account. By himself, and others, he was also said to have been influenced by Bergson. It was later that he turned to Husserl.

Therefore, Schutz's view of Weber's work is first briefly noted, followed by the idea taken from Bergson, then the ideas of Husserl which were later developed by Schutz. Finally what seem to be Schutz's basic ideas are summarised.

Schutz, when he became concerned with social science, turned first to Weber, and the sociology of understanding became his prime concern. He considered that Weber's "interpretive sociology" was "imposing". (Schutz, 1972, p. 7). Wagner, discussing Schutz's "intellectual biography", stated that:

"With the acceptance of Weber's fundamental approach and the core of his methodology, Schutz became a Weberian sociologist. Here was a platform from which he could take off."

(Wagner, 1983, p. 14)

However, Schutz was not uncritical of Weber. He considered that his 'interpretive sociology' was "based on a series of tacit presuppositions". (Schutz, 1972, p. 7). In particular, he considered that Weber's definition of "action as meaningful behaviour" was "vaguely defined". Also, 'social action', defined by Weber as that which was oriented towards others because of the 'subjective meaning' attached to it by the actor, was full of "unsolved problems". (p. 19). Classifying behaviour into various types, in Schutz's view, also "itself presupposes that the meaning of an action is identical with the motive of the action". (Schutz, 1972, p. 19). That is, meaning was seen as subjective on the part of the person acting. But for Schutz, this said nothing about how "the meaningful existence of the other" could be given to other actors. (p. 19). That is, it did not explain how sets of individual 'subjective meanings' could become shared by others. It also did not explain how "the subjective meaning of the behaviour of others" could be studied. (p. 20).

For Schutz, the social world was seen as necessarily intersubjective, and it was how this intersubjectivity was constituted, in terms of assigning meaning to the behaviour of others, that was the object of social science. In this respect, he considered that Weber had not gone far enough into the question of how 'shared meanings' developed, and thus how 'social reality' was established in the minds of actors. Thus, Schutz's first task was to try and "remedy the weaknesses" which he considered existed in Weber's 'interpretive sociology'. (Wagner, 1983, p. 20). As a necessary foundation for this, he thought it necessary to first study what 'consciousness' might involve, and to discover:

"how a conscious subject, in experiences and acts, understands himself as well as the objects of his experiences and actions."

(Wagner, 1983, p. 21)

Schutz considered several possible approaches. According to his biographer, he first considered Kant, and also read Husserl, but at first found the latter not "the bridge to the problems with which I was concerned". (Schutz, U. M., 1959a, in Wagner, 1983, p. 20). Instead, he first turned to Bergson.

Bergson, in philosophy, had distinguished between the inner experience of time as something lived through, and spatial or conceptual time. The first, 'pure duration', the 'duree', he considered as not being open to the intellect, because it was not felt as a series of conscious states but as an unbroken stream of these. As Schutz put it, in the duree there were "no mutual externality of parts, and no divisibility". (Schutz, 1970, p. 60). It could only be grasped intuitively. Spatial time, on the other hand, as conceptual, was open to intellectual analysis.

What Schutz was actually concerned with was not Bergson's work per se, but with, as noted, the roots of subjective meaning and understanding, of consciousness. He was concerned to establish "an empirical science of the Thou", and how experience of this could become known to the "I". (Schutz, in Wagner, 1983, p. 22). Applying Weber's "scheme of social action" to "intersubjective relations", Schutz "treated the Thou as part of the thematic of the solitary experience of the solitary I". (Wagner, 1983, p. 22). The problem was to discover that experience.

While 'experience', or consciousness was a matter of inner duration, the fact that individuals lived in "an external world" meant that they had become accustomed to thinking in concepts of space and time, so that experience of duration had been replaced by the spatial and temporal experience, so that experience pre-existing these concepts could not be grasped, or the 'image before the concepts', or consciousness, could not be seen.

Bergson had attempted to solve this problem of discovering inner experience by moving from "the concepts of the space time world to the images and from the images to inner duration". (p. 22). Schutz, however, decided to move in the opposite direction, going from the "inner experience of pure duration to the concept of space". (Wagner, 1983, p. 22). He developed the concept of 'life-form', which stood for the attitude of the 'I' to the external world, which also included self-awareness. There were six 'life-forms', which were hierarchical. Starting from "pure duration" the next was "memory-endowed duration", which involved awareness of "the ongoing I". Then there was "the acting I", which included bodily movement and "rational action", and an awareness of the outside world. Then there was the "I in the Thou Relation", which was seen as being aware of "an alien duration", the actions of others, where objects, existing in "space-time", were grasped by both the self and others. Such 'objectivity' was the basis of all experience. Next there was "the speaking I", which, through the use of language, 'understood' "meaning relations". Schutz was here indicating the importance of language for intersubjectivity. This 'speaking I' experienced the external world as a combination of "object, action and Thou". Finally, there was the "Thinking and Interpreting I", whose "experiences are conceptual and framed in terms of space and time". (Schutz, in Wagner, 1983, pp. 23-4).

These 'life-forms' were "ideal-typical". Schutz's argument was that there was in reality one 'total I'. The other forms were regarded as an expression of the different 'stances' which this 'I' took towards the external world. He argued "that all experiences of the total I enters into every life-form". (Schutz, in Wagner, 1983, p. 24). However, he argued that they did so symbolically. Each succeeding 'life-form' in the hierarchy was related to the preceding one by "symbol-relations", and the succeeding forms re-symbolised those included in the earlier forms. This system of 'life-forms' was designed as a reconstruction of the bridge from 'pure

duration' to the 'space-time' world of conceptual thinking and meaning. However, it did not escape from the problem noted by Wagner which was that 'pure duration' was itself an "idealisation". According to Wagner, Schutz himself stated that:

"it can only be deduced with the help of the more complex life-forms (memory); "it is impossible to experience pure duration immediately, not even through intuition."

(Schutz, in Wagner, 1983, p. 25)

Wagner stated that Bergson had "admitted as much", and had also noted that;

"even speaking about duration was grossly misleading, since to do so meant describing that which is non-extensive and continuous in terms of a language which "sets out time in space."

(Wagner, 1983, p. 25)

Although Schutz had hoped to escape this 'paradox', Wagner considered that he eventually came to "the recognition of the impossibility of doing so, and thus that "Bergson had led Schutz into a dead end". (Wagner, 1983, p. 20). However, he also considered that there were gains from this period of Schutz's work. First, there was the recognition of the importance of symbolising experience as "an act of positioning meaning", and also of the importance of the interpretation of meaning, and the symbolic function of language in this, where the 'acting I' encountered the 'Thou'. Language was "a world of everybody's experience". It was 'objectively given' but was subjectively interpreted by a listener. In the same way, actions of the 'Thou' were seen as being interpreted by the 'acting I' in terms of the latter's 'experience'. Hence Schutz was developing his theory of the nature of intersubjectivity. He was also developing his theory of the role of the social scientist. In the analysis of linguistic communication, and of "the intricate intertwining of the dualistic "subjective" and "objective" meaning structures of conversations" (Wagner, 1983, p. 31). Schutz added the concept of "the third observer", who represented "a third kind of meaning interpretation". (p. 31). This 'third observer', from a "detached standpoint", that is, one not involved in interaction,

would "understand the listener interpretively". The "third observer" was able to see the listener:

"as a person who, by trying to establish the objective meaning of what has been said, posits himself subjective meanings; he subjectively interprets the spoken words."

(Wagner, 1983, p. 31)

The 'third observer' himself would then interpret these interpretations. This idea also came from Bergson. Wagner observed that, despite the problem of 'duration', Bergson was responsible for many "phenomenological-psychological insights" for Schutz. He added that:

"Among the Bergsonian ideas and conceptions that entered into his permanent stock of theoretical knowledge were: references to ready made ideas, to commonsense thinking, to the significance of an individual's life-story for his present experiences, and, not least, to Bergson's "conscious spectator", introduced by Schutz as a "third observer" and "detached scientist."

(Wagner, 1983, p. 32)

Thus Schutz, despite problems, found Bergson as fruitful source for ideas. However, in order to carry out his aim of going beyond Weber, and develop his ideas of subjective meaning and consciousness, and of sociology, Schutz eventually turned again to Husserl.

As noted, Husserl is generally regarded as the founder of modern phenomenology. As a philosopher, his ideas developed gradually. Farber commented that he "was a slowly maturing thinker". (Farber, 1966, p. 3). However, there seems to be a line of continuity, in that from the beginning he seemed concerned in one form or another with seeking for the basic origins of human knowledge, and its constitution in consciousness, as well as for the 'essential' truths on which both philosophy and science could be based.

As well as Husserl being one of the sources for Schutz, he himself was affected by the ideas of others, as most thinkers are. Farber, in his discussion of the background of Husserl's philosophy, showed that there were a number of sources which in one way or another were influential in

its development. These included Descartes, Kant, Brentano and William James among several others. (Farber, 1967, pp. 7-17; Farber, 1966, p. 2 and Wolff, 1979, p. 502).

Sharp and Green listed Husserl as a thinker in "the idealist school". (Sharp and Green, 1975, p. 18). Phillipson also regarded his philosophy "as a form of "idealism". (Phillipson, 1972, p. 121). Although Husserl did later write of "transcendental-phenomenological idealism", this does not mean that the classification by these authors is necessarily correct. (Husserl, 1931, p. 18). It is interesting that Farber quoted from a letter written to him by Husserl which said that:

"As a young beginner I naturally read much I liked the critical-sceptical point of view, since I myself did not see firm ground anywhere. I was always very far removed from Kantianism and German idealism."

(Husserl, in Farber, 1960, p. 6
and 1967, p. 17)

Ideas do change in the course of a thinker's development. Thus Farber notes that Husserl's later reading of Kant did impress him, although critical of the latter "for failing to achieve a "pure" theory of knowledge", something which Husserl himself hoped to achieve through phenomenology. (Farber, 1966, p. 7). Nevertheless, the earlier comment perhaps indicates that classifying 'thinkers' or 'traditions' for that matter, as definitely one thing or the other is something to be wary of. Moreover, Schutz, when outlining Husserl's "general aim" stated that it would explain why students found problems when seeking to place his philosophy into "one of the customary textbook labels, such as idealism, realism, empirical", for none of these could "be adequately applied to a philosophy that puts them all in question". In Schutz's view, phenomenology's place was "beyond - or better, before, all distinctions between realism and idealism". (Schutz, 1970, p. 54).

Husserl's philosophy was said by Farber to have developed over three different periods. This was in his earlier volume, originally written in

1943. The periods were listed there as:

"broadly speaking, the periods of psychologism, simple descriptive phenomenology (phenomenology in a narrow sense) and transcendental phenomenology."

(Farber, 1967, p. 15)

It was the latter which Husserl saw as: " a science covering a new field of experience, exclusively its own, that of "Transcendental Subjectivity"." (Husserl, 1931, p. 11). Later, Farber distinguished not three, but four, "main periods" for Husserl's development. (Farber, 1966, p. 12). These overlap, but are broadly similar to the three noted above, so it seemed sensible to stay with the latter. The two earlier forms of philosophy in these three were seen by Farber as corresponding to "stages of progress" towards the third and last. (Farber, 1967, p. 16).

The first, the period of psychologism, was that of Husserl's beginning as a philosopher, and reflected his early training. Farber noted that Husserl had originally trained in "mathematics", as well as "physics, astronomy and philosophy". (Farber, 1966, p. 2). Thus, Husserl's interest in the relationship between philosophy and science was a natural development. As a philosopher, he was concerned with clarity, and as a mathematician he wished to be clear about mathematical concepts. At the time he began his work there was a considerable debate going on in mathematics about the nature of number, whether "transfinite as well as finite numbers" were to be "accepted", and whether:

"the existence of a number, in certain cases, can be asserted on the basis of logical considerations only, without one being able to construct such a number."

(Pivcevic, 1970, p. 22)

Husserl's mathematics teacher, Weierstrass, had taught him that "pure Arithmetic" was "a science based simply and solidly on the concept of number", without need for "other presuppositions". (Pivcevic, 1970, p. 22).

Husserl's first book, which was "The Philosophy of Arithmetic", stemmed

both from his mathematical training and from his philosophy training with Brentano, from whom he derived an interest in the latter's "descriptive psychological method of analysis". (Pivcevic, 1970, p. 23). He also received from Brentano the concept of intentionality in relation to consciousness, the idea that: "Each psychical phenomena contains something as an object" (Farber, 1967, p. 12). That is, consciousness is always consciousness of something. (Wolff, 1979, p. 503). Farber stated that this work of Husserl was written therefore while Husserl was under the influence of "psychologism". He defined this as the view that "logic and mathematics are grounded in the psychology of thought processes". (Farber, 1966, p. 4). Husserl, in writing his book, was thus interested in linking the psychological and logical aspects of arithmetic. His particular concern was "to clarify the basic presuppositions of mathematics". (Pivcevic, 1970, p. 23). This meant essentially the origin' of the concept of number.

A contemporary of Husserl, Frege, was also interested in the same problems. There was a difference in their approach, in that Frege "was trying to explain the logical presuppositions of mathematics and was interested primarily in definitions". (Pivcevic, 1970, p. 23). Husserl was critical of this approach, considering that, according to Pivcevic, the concept of number could not "be given a logical definition" (Pivcevic, 1970,, p. 23). In part, therefore, the "Philosophy of Arithmetic : Psychological and Logical Investigation was an attack on Frege's position.

Frege later criticised Husserl's book. Pivcevic stated that it was partly as a result of this criticism that Husserl turned away from 'psychologism'. (Pivcevic, 1970, p. 24). However, Farber argued that what he called "a legend" had grown up around this book. He stated that:

"It is commonly supposed that it was a thoroughly unfortunate attempt, ending in failure, so much that Husserl was finally led to repudiate it completely."

(Farber, 1967, p. 25) -

In Farber's view, this was a thoroughly mistaken idea. He argued instead

that:

"The student of phenomenology would do well to look to this work for the rudimentary beginnings of phenomenological analysis, even though it must be regarded as pre-phenomenological. In fact, it may be said that this work contains the best key to the understanding of Husserl's philosophy."

(Farber, 1967, p. 25)

According to Farber, the book dealt with first a psychological analysis of "the concepts of plurality, unity and number" as they are "given to us really". (p. 26). This seems to mean as they are directly perceived. Secondly, the book considered "the symbolic ideas of plurality and number" and in so doing tried:

"to show how the fact that we are almost entirely limited to symbolic concepts of number determines the meaning and purpose of the arithmetic of numbers."

(p. 26)

As a non-mathematician, the researcher was in no position to know if this was a 'fact' or not. However, the distinction between the 'psychological' and 'symbolic' concepts of number seemed analogous to the distinction between the meaning of words as understood in context and the formal syntactical structure of language, where there can be formal correctness but not necessarily meaning. At least this was how Husserl's two aspects of number were understood, which is an indication that people interpret statements in the light of their own experience.

Husserl began, according to Farber, by analysing the concept of number, and therefore also considered the problem of plurality. The grounds for the abstraction of these concepts were wholes, or "totalities" of "definite objects", such as "a few definite trees" or "a feeling".

Farber stated that when discussing the relationship between the concepts of number and plurality, "the nature of the particular "contents" that are compounded" did not matter, in Husserl's view, because it was not "the individual contents entering into given totalities that are the bases

of abstractions, but rather the concrete totalities as wholes." (Farber, 1967, p. 27). Pivcevic observed in relation to this that "one of Husserl's basic concepts is that of a collective", and also pointed out that the latter:

"comes into being as a result of our focusing attention upon certain objects and associating them into a group. It can contain widely differing members which need not be either temporally or spatially connected."

(Pivcevic, 1970, p. 29)

What did link them together was a "collective association". (p. 29).

In discussing the origin of the concept of plurality, Husserl considered "the theory of relations in consciousness, building upon the uses of this term by Brentano and Mill". (Farber, 1967, pp. 30-31).

Husserl distinguished between "primary relations", those which were "immediately recognised as a constitutive part of the idea that we have of the related terms" and those relations "which ... have the character of 'mental phenomena'." (Pivcevic, 1970, p. 29). These last were based upon "mental, intentional acts".

Husserl regarded the "collective association" as a "mental relation", which involved a "synthetic mental act". This latter mental relation was seen by Husserl as differing from perceptive "connexion". These were "primary connexions", those which, like a "perception" of "the vase of flowers", "while being 'analysable' into parts, possesses nevertheless an original unity". (Pivcevic, 1970, p. 30). In the case of the 'collective association', the unity is imposed by the 'synthetic mental act'.

What Husserl was interested in was the nature of this mental act, the psychological aspect of the process of abstraction from 'totalities'. In his view, the "connecting relations" which linked the "individual elements" of which "totalities" were comprised into "wholes" were the same whatever the different contents were. In discussing how 'unity' was effected, Husserl used the idea of 'partial presentation'. That is, the "totalities" were

presented as a "unity", in which the "presentations of the individual objects were contained as partial presentations". (Farber, 1967, p. 27). That is, in looking at a crowd, for example, this is known to be comprised of a number of individuals, but these only partially present themselves to us as such, it is the crowd as a "unity" which dominates.

It was Husserl's argument that such "partial presentations" were unified in a psychological process, a "synthetic mental act", since if this did not happen "the concept of plurality" could not arise. "Reflection" on "the union of contents" shown by "totalities" led to the development of the concept. (Farber, 1967, p. 27; Pivcevic. 1970, p. 30). This was an early statement of the importance of 'reflection' on mental acts, something Husserl and Schutz developed later in phenomenology.

In the second part of his book, Husserl discussed the symbolic, logical aspects of such concepts as plurality. He raised here the issue of "genuine" and "symbolic presentations". The latter served as:

"provisional substitutes, and in cases where the real object is inaccessible, as lasting substitutes for genuine presentations."

(Farber, 1967, p. 45)

The question of symbolic presentation was considered in relation to the general concept of number, and in particular to the idea of aggregates, and the activity of counting. Symbolic, or logical, presentation were necessary, Husserl considered, in order to go beyond actual aggregates and so extend the number series. (p. 50). It was the fact that people could not mentally deal with "larger direct processes of counting" which caused the development of "logical postulates and concepts". (Farber, 1967, p. 51). These included the system of signs.

Husserl's task in this section of his book was to establish a principle by which "all numbers" could be derived from "actually given numbers", such that "every number will have a definite place in the system".

(Farber, 1967 p. 51). However, the problem was that a notational system based on a "few fundamental signs" had to have a "parallel system of concepts, based upon certain fundamental concepts. (p. 51).

Husserl regarded written signs as superior to word signs, and as essential for the development of arithmetic. This last was:

"defined as the science of the relations of numbers, its essential task consisting of deriving numbers out of given numbers by means of certain known relations obtaining among them."

(Farber, 1967, p. 53)

Such derivation was either "essentially conceptual", with signs as subordinate, or "essentially sensuous", a method deriving "signs from signs by means of rules, on the basis of a system of number signs, the result being the notation for the desired concept". (Farber, 1967, p. 53).

It was this last that Husserl regarded as superior, because it could in his view solve all problems with numbers. It was therefore "the logical system of arithmetic". (p. 53). In Husserl's view it was because there was a "lack of a logic of the symbolic methods of knowledge, and especially of arithmetic" that this idea had not been "recognised". It was also pointed out that it was "significant for mathematics" that "the same system of symbolism can serve two or more conceptual systems which are formally similar while differing in content". (p. 53). Such conceptual systems were said by Farber to have, from then on, to have used "this new concept of calculation". The "technical methods" of this system provided "the pure mechanics of calculation" which were "basic to arithmetic". Then "the art of calculation" was "no longer identical" with that of "arithmetical knowledge". It was "number concepts" and the way these were connected which gave "the foundation" for "arithmetical methods of calculation". It was also the "system of numbers" which enabled the extension of number to infinity. In relation to the concept of plurality in the symbolic-logical sense, if all pluralities were able to be broken down into individual

numbers, then "there could be no actual number without a symbolic correlate in the system of numbers". (Farber, 1967, p. 53). There was only one such correlate because "different number signs necessarily refer to different actual numbers". (p. 53). The four arithmetical operations were seen by Husserl as "arithmetical operations of calculation", because they dealt with both "signs" and were used in "the derivation of numbers" from the countable ones.

According to Farber, Husserl concluded this part of his book by re-stating that a system of numbers was necessary to extend number because of the fact of being restricted for the most part to "symbolic formation". From all of these possible latter formations that were "equivalent to every actual number concept", one would be chosen "according to a fixed principle" and be "given a systematic place". The next problem would be to evaluate, or reduce "all other thinkable number-forms" to that of "the number equivalent to them in the system". However, the solution to this problem, that of "proper methods of evaluation", was "dependent on" the development of arithmetic "in the sense of a general theory of operations". (Farber, 1967, pp. 54-5).

As noted, Farber, and also to a large extent Pivcevic, saw this first work of Husserl as containing the germ of ideas developed in the later phenomenology of Husserl. It discussed certain concepts such as reflection, and the constitution of meaning, and the idea of 'presentation'.

The discussion of 'The Philosophy of Arithmetic' as summarised by Farber and Pivcevic was gone into at some length for two reasons. One was that indicated above. The other was that, as a non-mathematician, the ideas were difficult, and therefore the researcher was trying to make sense of it, as with other theoretical ideas discussed in this and the previous section. It was also considered, having read Farber, that some general writing on the origins of phenomenology, of which Phillipson can be taken as an example, had glossed over the earlier periods and gone

straight to 'transcendental phenomenology'. Phillipson also stated that "the earlier writings" of Husserl were in "reaction to psychologism", as if this first book, written under its influence, had not existed. (Phillipson, 1972, p. 121).

Husserl did turn away from 'psychologism'. One idea, as noted, is that he did so because 'The Philosophy of Arithmetic' was criticised by Frege. The latter considered that Husserl had confused psychology and logic, and also subjective and objective factors in the idea of 'presentations'. Frege also took the view that "numbers attach to concepts", because 'counting' took place through these, which was the opposite view to Husserl's. (Pivcevic, 1970, pp. 30-31). The idea of the "origin of a totality" given by Husserl was also criticised. (Farber, 1963, p. 56).

However, according to Farber, Frege did not criticise everything in the book, and admitted that because of his different, logical, standpoint, it was "difficult for him to do justice to Husserl's merits". (p. 57). Husserl did take note of much of Frege's criticism, as Pivcevic and Farber both show. However, even though Husserl later turned away from 'psychologism' partly as a consequence of Frege's criticism, he did not reject all his early ideas, and may in any event have been turning towards newer ideas, because he was a thinker who was continually developing. For whatever reason, he did not complete his intention of writing a second volume of "The Philosophy of Arithmetic".

Instead he began to study the basis of logic, and this work developed into his concern with phenomenology, first 'descriptive' and then 'transcendental'. As a general comment, if this researcher found the 'Arithmetic' quite hard to follow, it was simple compared to the later ideas. Some sympathy was felt with the unknown fellow student who pencilled in an alternative title to Husserl's "Ideas" as "Yes; there is a private language". Schutz, according to Wagner, spent two years, together

with a friend, in an "intensive study" of Husserl's work. (Wagner, 1983, p. 85). He also met Husserl, and was in regular communication with him until the latter's death.

Lacking both the time to devote to one part of 'theory', and the intellectual status of Schutz, this researcher made what she could of Husserl's ideas, and various commentaries, as indeed she did of Schutz's own work, given the need for comparison.

Husserl, when he began to study logic, was said to have been influenced by his reading of Bolzano, a 19th century philosopher whose main work was with logic and the philosophy of mathematics. (Passmore, 1966, p. 193). This reading directed Husserl's attention to the idea "of a 'pure logic' at a critical time in his development". (Farber, 1966, p. 7). Pivcevic stated that also, at that time "logical theory was in a state of confusion". (Pivcevic, 1970, p. 35). Husserl, in this period, apparently rejected the idea that logic was grounded in psychology, thus partly accepting Frege's criticism. Husserl considered that, according to Passmore, that "psychological laws are no more than inductive generalisations, subject therefore to correction in the light of further experience". (Passmore, 1966, p. 188). Husserl was concerned with the laws of logic. Not only was logic non-psychologically based, it was also not 'metaphysical', nor was it a "regulative", purely 'practical' discipline. Instead, Husserl regarded logic, and the mathematical principles he thought were derived from its laws, as 'a priori' bodies of knowledge, which could, therefore not "be 'grounded' upon inductively derived premises". (Passmore, 1966, p. 188). These 'laws' were, instead:

"grounded in the meaning (or as Husserl also calls it, 'essence', 'content') of concepts such as truth, proposition, object, quality, relation, connexion, law, fact etc."

(Pivcevic, 1970, p. 35)

In the course of his studies of logic, Husserl came to develop ideas

on the theory of 'knowledge' in general, and of the task of philosophy. He came to be critical of 'naturalism', in the first sense of this as applying 'scientific methods' to other areas of knowledge. He was led in this direction by his view, noted above, that logic and mathematics were 'a priori', and because the principles of formal logic (of which he was critical) in particular deductive reasoning, underlay both mathematics and science. Formal logic was considered by Husserl to be "'insufficiently critical' in a Kantian sense - it does not examine the 'grounds' of its own operations". (Passmore, 1966, p. 189). Husserl, in the first part of 'Logical Investigations', considered, according to Pivcevic, that a "pure logic" should have:

"one principal aim; to explore the conditions that make theory and theoretic explanation possible; to clarify the 'essence' of theory."

(Pivcevic, 1970, p. 36)

To achieve a 'pure logic' required the use of "the 'phenomenological' method", or, as Husserl first called this, 'descriptive psychology'. This was not empirical, nor based on:

"the standpoint nor employs the methods of the natural sciences, because it is not possible from that standpoint or by those methods to arrive at a 'pure theory', a theory which will be independent of contingent empirical facts."

(Passmore, 1966, p. 189)

Thus, from being concerned with an attempt to discover the pre-suppositions of mathematical laws, Husserl was moving towards a concern first with those of logic and then of 'knowledge' of the world in general, including science. Thus, Husserl was led to consider the nature of 'consciousness' and of 'meaning', and the manner of 'Being' in the world for individuals. His concern was directed to attempting to discover how 'the natural world' came to be constituted in the 'consciousness' of individuals, that is, with "transcendental knowledge" in Kant's terms. (Flew, (Ed), 1984, p. 34). This was because it was these mental acts of perception which lay behind concepts such as 'judgement', 'truth', 'proposition', 'object', and so on,

which were themselves the basis of theories. These were the pre-suppositions which, in Husserl's view, 'formal logic', and by extension mathematical or scientific theories did not examine. It was the task of a serious scientific 'phenomenological' philosophy to make an 'eidetic' study of 'consciousness', in order to discover the 'essence' of the meanings, judgements, and so on, involved in the conscious, mental, act of perception, on which theories of 'the natural world' were built. These 'mental acts' were the only thing available for the phenomenologist to discover, and because in Husserl's view they were logically prior to theories, his philosophy had to be 'eidetic' rather than empirical, concerned with the 'Idea' rather than the 'Real' with 'essence' and not 'fact', with 'understanding' the nature of what it was that a 'causal explanation' was required for, not such an explanation itself. Hence the absence of "(causal) explanation" in phenomenology. (Wolff, 1978, p. 502). Thus, Husserl was against that form of 'objectivity' which ignored the fact that 'meaning' was based upon human perception. As noted in the first part of this section, however, philosophers and scientists such as Popper and Medawar angrily rejected any claim that 'explanation was the sole concern of science, arguing that 'understanding' was as necessary a part of their work as in the 'humanities', and 'conventionalists', among whom Feyerabend could be included, acknowledged the role of human perception and possible bias both in the choice of problems and in what counted as 'evidence' for a theory. The question might be asked whether Husserl was fair to scientists, but as Pivcevic pointed out, at the time of Husserl's later writing (1936) science did seem to be 'dehumanised'. (Pivcevic, 1970, p. 88). So Husserl's ideas have to be considered in historical context. This is perhaps ironic in view of the claim that phenomenology is 'ahistorical'. If it is concerned with how 'ideas' of the 'nature of the world' come to be established it cannot be, because 'ideas' or 'perspectives' seem logically liable to depend upon the 'situation' or 'experience' at a given time, or different times. This point came up in the fieldwork

when teachers' 'perspectives' were being examined in a questionnaire, the results of which are given in Chapter Six. It was clear that a number of factors influenced their 'ideas', all of which could be summed up as 'experience'.

Wolff observed that the tendency or "predisposition" of phenomenology to "neglect history and time in the sense of historical time" was "to some extent recognised and remedied by Husserl" in his latter writings. (Wolff, 1978, p. 502).

Returning to Husserl's concept of the 'essence' of consciousness, it seems that he turned again, in his search for it, towards Brentano's concept of 'intentionality', that is, that 'consciousness' is always "consciousness of something". (Husserl, 1931, p. 255). In relation to 'consciousness' in this double sense, Husserl therefore distinguished between what he called the "noesis" and the "noema", to avoid using previously "loaded terms". He did not clearly define the former, although he was careful to distinguish what it was not referring to. (Husserl, 1931, pp. 249-50). What he seemed to mean, as far as this researcher grasped it, was the individual as being aware, thinking, making sense of, some 'thing'. In contrast, the "noema" referred to the 'thing' thought about, or "perceived as such" or "remembered as such". For Husserl, all "intentional experience" or consciousness, was "noetic", that is, it was "its essential nature to harbour in itself a "meaning" of some sort, it may be many meanings" (Husserl, 1931, p. 257).

The fact that consciousness was 'intentional' in the sense of harbouring 'meaning' did not mean that the perception involved was necessarily true, it might be "a 'mere hallucination'" (p. 259). Nevertheless it was still on the basis of it that an individual's 'knowledge of' and attitude to 'the natural world' was built up, and such perception was a proper object of enquiry.

In order to discover the 'essence' or "pure consciousness", Husserl developed the methodological concept of "reduction".

Here, the researcher encountered some problems, in terms of confusion, not exactly about the meanings of the terms, but about whether or not two of them, the "phenomenological" or "transcendental" reductions referred to the same thing. Husserl, for example, himself wrote of "The Phenomenological Reductions". Farber wrote that "the expression "eidetic reduction" which is paired with "transcendental reduction" in the complete phenomenological reduction". (Farber, 1967, p. 521). This implies two stages. Wagner, however, wrote of three stages, a "psychological", an "eidetic" and "transcendental". (Wagner (Ed), 1970, p. 6). Wagner elsewhere wrote that:

"Schutz was inclined to view Husserl's phenomenology as a three layered structure, as suggested by Husserl's 'Britannica' article of 1929."

(Wagner, 1983, p. 42)

Although this referred to Husserl's phenomenology as a whole, it directed the researcher's attention to the 'Britannica' . The 14th edition, with Husserl's article, was unavailable, but the 15th edition article on "Phenomenology" did give the "reduction" as the basic method of phenomenology as consisting of three steps, the "phenomenological", the "eidetic" and the "transcendental". (Encyclopedia Britannica, 1987, Vol. 25, p. 636). These three stages, or "reductions", were also given in the "Encyclopedia Americana". (1985, Vol. 24, p. 617). Yet Pivcevic wrote, of "the phenomenological reduction" and "the transcendental phenomenological reduction" as if they were the same thing. (Pivcevic, 1970, pp. 70-73). Lacking other guidance, this researcher decided that looking at "reduction" as having three stages made it easier for her to make sense of it.

The first reduction, the 'phenomenological', also called the 'epoche', was developed by Husserl to stand against the concept of phenomena as used in 'positivism' to refer to empirical sense experience. This reduction

involved not taking the "natural standpoint", which was one which took the "fact world" as being something which existed "just as it gives itself to me as something that exists out there". (Husserl, 1931, p. 106).

In contrast with such a standpoint, the "phenomenological reduction" involved a 'doubt', in the Cartesian sense, about the "natural world". (Husserl, 1931, pp. 107-111; Schutz, C. P. I., 1967, p. 104; Passmore, 1966, p. 194). For Husserl, this meant that the "natural standpoint" of taking the world as "given" was "put in brackets", that is, judgement about its existence were "suspended". This "epoche" did not mean for Husserl being sceptical about the existence of the natural world, only that it "completely bars me from using any judgement that concerns spatio-temporal existence". (Husserl, 1931, p. 111).

As an aside, the concept of 'bracketing' was picked up by the researcher during readings of the 'New' Sociology of Education in the first instance, then from Glaser and Strauss and from Schutz (C. P. I, p. 104) rather than from Husserl's work. This last had not been read when the research started. At the time, it did not seem particularly necessary to read Husserl, something which was later considered to be a mistaken view. It was perhaps as a consequence of this gap that the researcher at the commencement of the research held a perhaps 'naive' view of 'bracketing', largely based on a first interpretation of Glaser and Strauss. It was taken to mean not only putting aside personal experience as a teacher, and not 'taking for granted' what constituted 'teaching' and 'learning', but also of going in without any ideas as to what might be found. Rather as Hammersley described later, 'bracketing' was thought to involve waiting, 'casting out one's net', and information and themes would 'emerge'. 'Natural' events could thus be 'tapped into', and material could be "dredged". (Hammersley, 1984, p. 56).

Since the researcher also had the concept of 'foreshadowed problems', some contradiction was felt to exist between this and 'bracketing', creating

some confusion for her, and a certain amount of floundering about, uncertain about how actually to proceed. Having the idea of waiting for 'themes to emerge', and that questions should be based on what was seen and heard, or 'grounded in the data', originally meant for the researcher being afraid to ask any question which had not originated in this way.

On later reflection during the research this 'waiting for something to come up' was felt to be a not very fruitful procedure. It was also rather unnerving, wondering if anything would emerge to write about. A re-reading of Glaser and Strauss led to the interpretation that not all previous knowledge had to be 'suspended' except on first going in. It was accepted, however, that one's knowledge of having 'foreshadowed problems' should be kept in mind, and the effort made not to let these influence questions, so that these in the first instance should arise from the data. However, it was thought that after this, other knowledge could be utilised to formulate further questions, when such knowledge seemed relevant.

This aside on the first stage of Husserl's 'reductions' has been made to indicate that the process of 'reflection' began early. It was seen as part of 'interpretive' research.

Returning to Husserl, the next step after 'bracketing' the 'real' world of fact was, for him, the "eidetic reduction", which was closely associated with the first stage. This reduction was a means whereby the "pure essence" of a 'thing' could be 'intuitively' grasped with the aid of "the play of fancy". (Husserl, 1931, p. 57). Both Wolff and Phillipson wrote of Husserl using "free variation" as the means of this reduction to the 'essence', though they did not give a reference for checking. (Phillipson, 1972, p. 129; Wolff, 1978, p. 506). The 'essence', or "essential characteristics" of a 'thing' were what remained after 'intuitive' or imaginative variation of it, those 'essential' features which enabled it to be perceived

as a "cube" or "tree", for example.

It was Husserl's contention that 'consciousness' of a 'thing' was always in terms of its 'essence' and not in terms of particulars, as Humean empiricism presumed, according to Passmore. He quotes Husserl as rejecting "what he regards as the 'mere presumption' that we are directly aware only of 'particulars'." Passmore added, directly quoting Husserl, that:

"'the truth is' so he summarised his view in his Ideas ... 'that everyone sees ideas, "essences" and sees them when they think and they also pass judgements about them. But from their theoretical "standpoint" people explain them away'."

(Passmore, 1966, p. 190)

Passmore did not give a page reference for this quotation from "Ideas", and the researcher was unable to place it, despite several searches of what appeared to be the relevant section of that work, 'Naturalistic Misconstructions'.

However, Husserl was certainly critical of empiricist views of 'knowledge', if empiricism is understood to mean the view that "all knowledge shall be grounded in experience". (Ideas, p. 83). Flew, defining 'empiricism', noted that it maintained "that at birth the mind is, as Locke put it, "white paper, void of all characters", and that only experience can provide it with ideas". (Flew, (Ed), 1984, p. 104). This work also noted the claim by some, though not all, empiricists that "the truth of factual statements can only be established inductively from particular experiences". (p. 105).

Husserl, in contrast to these 'empiricist' views, seemed to be arguing that 'consciousness', or the 'essential' capacity to make judgements about 'facts' as they present themselves, is logically prior to 'experience' in the empiricist sense. It is this inward "seeing" or "primordial dator intuitions" which give a "rational statement" about "facts" or "objects"

their "justification". (Husserl, 1931, pp. 83-4). Thus, Husserl did not accept 'experience' in the empiricist sense. He stated that, "Thus, for 'experience' we substitute the more general "intuition"." (p. 85). Husserl criticised empiricism for denying the role of intuition, and for relying on induction, and thus "on the system of mediate modes of inference" (p. 85). He also questioned "the truth of mediated conclusions", whether "deductively or inductively inferred". According to Husserl, empirical philosophers "start out from unclarified, ungrounded preconceptions", whereas he considered himself to begin:

"from that which antedates all standpoints: from the totality of the intuitively self-given which is prior to any theorising reflexion."

(Husserl, 1931, p. 86)

In discussing the faults of empiricism in relation to the 'essence' of consciousness, Husserl might be considered as performing a form of 'eidetic reduction'. By reducing the essence of 'experience' to intuition, he was thus taking the second step for discovering 'pure consciousness'. However, to criticise empiricism in itself was not enough. To discover the 'essential' nature of mental acts of perception, of "the pure sphere of experience", that is, intuition, the third reduction, the "transcendental" was required. This involved the concept of the Ego, the 'conscious' self, as 'intentional', and "living its experiences", so that it was possible for this to be "included in its glance", to be made an "object for the Ego", in the process of reflection. The concept of reflection involved, for Husserl, having, like Bergson and James, the concept of "the stream of consciousness", and also that of "inner time". The "lived experience", the "immanent" now, can be lived in "the mode of unreflecting consciousness". But reflection gave awareness of such experience, and the possibility of its "retention" and recall, of the 'now' and past, or "protention" or "anticipation" of the future. (Ideas, p. 216). The experience could be reflected upon, and also the sense of the Ego as reflecting. Thus, for Husserl;

"Reflexion ... is the expression for acts in which the stream of experience with all its manifold events ... can be grasped and analysed in the light of its own evidence. It is ... the name we give to consciousness' own method for the knowledge of consciousness generally. But in this very method it becomes itself the object of possible studies."

(Husserl, 1931, p. 219)

In effect, the 'idea' of the 'transcendental reduction' seems to be that the Ego, or that which perceives, thinks and so on, performs a form of 'epoche' upon its 'experience', so that it becomes an object, itself for perception. According to Husserl, the "naively interested" Ego, was one "naturally immersed in the world" is, through the "phenomenologically altered attitude" split, so that: "the phenomenological Ego establishes himself as "disinterested observer"." (Husserl, 1977, p. 35). Through this 'reduction', Husserl seemed to see the 'Ego' as thus, through reflection, with the sense of *'inner time' as being able to 'see' not only the 'thing', such as a 'house' or 'joy' itself, but itself as experiencing this.* It was this awareness of the reflecting Ego that seemed to indicate 'pure consciousness'. At least, this is the 'sense' that this researcher made of the 'transcendental reduction'.

Such 'reflection', according to Husserl, would bring about 'modification' of consciousness, in both the noetic and noematic aspects, or 'cogito-cogitatum', which seem to mean the same division. In practical terms, this means that with reflection upon experience; and being aware of themselves as experiencing people may well 'see things differently'. Thus, Sharp and Green should not have been surprised at apparent 'contradictions' in teachers' views, if they were 'doing phenomenology'. In the present research, an example of such a 'modification' occurred in relation to an art lesson, an incident reported in Chapter Six. This was a case of "I never thought about it", until the researcher's question led to reflection.

Another point on 'reflection' is that if people have an 'intuitive' awareness of themselves as 'cogito', then it may be a natural response to

say 'I haven't thought about it', *meaning* they have not yet 'reflected' upon 'experience'. This 'intuitive' sense could well explain the dislike of teachers in the study for "off the cuff" comments being taken as expressing "what they really mean". The problem is that if 'modifications' occur in 'intentional' consciousness, what they 'really mean' can change as they reflect, which makes it difficult, to say the least, for researchers who are trying to establish 'meaning'. However, there is another problem with the idea of people as 'reflecting'. When looking at the concept of ideology, the concern of Chapter Three, it was noted that Richards found that some teachers could state why they did things in terms of their beliefs, that is, they were 'consciously aware'. Others could do so if they thought, (or reflected) about what they had done. Others, however, did not think at all at the time of doing something nor could they explain later. In other words, they apparently did not reflect. (Richards, 1979). This raises the question of whether Husserl was talking about himself as a philosopher, and so aware of himself as thinking, and then attributing this quality to people in general, who may never get beyond the 'natural attitude'.

In his later work, on 'transcendental phenomenology', Husserl developed the concept of the Lebenswelt, the 'natural world' in which people live, and discussed the 'natural attitude' and the apparent distinction between this and 'objective' scientific thought. He pointed out that the drive for objectivity meant that the fact that scientists, and their ideas, have a "historical human context". (Pivcevic, 1970, p. 88). As noted earlier, this was his apparent recognition of, and amendment for, the claim that phenomenology was ahistorical.

Husserl also later sought to tackle the issue of intersubjectivity, that is, of how the existence of the 'other' may be constituted in the 'experience' of the 'Ego'. Husserl basically seemed to be saying that 'others' have a "thereness for me", one "given to me in straightforward consciousness". (Husserl, 1977, p. 90).

Because with reflection the 'transcendental Ego' is separated out from the Ego as immanently experienced, this means that the validity of the existence of others is made evident. Husserl stated that "transcendental subjectivity" meant that:

"I, the transcendental, absolute I, as I am in my own life of transcendental consciousness; but beside myself, the fellow-subjects who in this life of mine reveal themselves as co-transcendental, within the transcendental society of 'Ourselves' which simultaneously reveals itself." It is thus within the intersubjectivity, which in the phenomenological reduction has reached empirical givenness on a transcendental level, and is thus itself transcendental, that the real (reale) word is constituted as "objective", as being there for everyone."

(Husserl, 1931, p. 22)

But Husserl was not considered to have solved the problem of intersubjectivity. Pivcevic observed that the problem was that if the 'Ego' performs the 'epoche' upon its own 'consciousness' and experience, how could the existence of 'others' be known, if they were part of the world put in brackets. There was thus the "danger of transcendental solipsism", that is, that only the solitary Ego actually exists. Pivcevic added that:

"Whatever Husserl may say in his own defence it is his transcendental epoche that cuts us off from the reality of 'other people'."

(Pivcevic, 1970, p. 82)

Wagner observed in his biography of Schutz that the latter considered eventually that Husserl's efforts to solve the problem of intersubjectivity were "failures", even though having earlier been optimistic that in the 5th Cartesian Meditation Husserl was "offering the essential points of departure for their solution". (Schutz, in Wagner, 1983, p. 42 and p. 332).

This concludes the discussion on Husserl's work, as part of considering the development of Schutz's phenomenology. There is much in Husserl which has been omitted, but as with this whole section, the intention has been to show the researcher at work 'making sense' of something. As stated earlier, there are difficulties with Husserl, not least that of language

and terminology which make this particular 'fortress' indeed hard to enter fully without Husserl's own deep grounding in philosophy and science. However, the above discussion seemed, to the researcher, to contain the 'essence' of his 'ideas'.

It is clear that Schutz, in his second look at Husserl, found much that was relevant for his own concerns with the nature of consciousness and his attempt to take further Weber's concepts of 'subjective meaning', 'understanding' and 'social action', and so build a social science to explore the intersubjective nature of the social world.

Schutz obviously respected Husserl's work, as his summaries of the basic concepts of it in "Collected Papers 1" (1967) and "On Phenomenology and Social Relations" (1970) indicate. In "The Phenomenology of The Social World", after his discussion of Weber's methodological concepts, Schutz turned to a consideration of how "meaningful lived experience" was "constituted" within "the stream of consciousness" of the individual "constitutor". In this section, he made use of several of Husserl's concepts, such as his version of "intentionality", and the idea of the "attention" being directed to "lived experience", when "the stream of pure duration" or the immanent Now of existence was subjected to the act of "reflection", which involved "recollection" and "retention". (Schutz, 1972 pp. 45-62).

In his discussion of reflection, Schutz also used Bergson's concept of the duree, but Wagner observed that in this particular work:

"Husserl's presence is dominant. All in all, references to him are about six times as frequent as are those to Bergson."

(Wagner, 1983, p. 39)

Yet although Husserl's work was an important influence for Schutz, the latter's concern was different. Schutz was more interested in the social world, in which individuals were necessarily involved, and so with the nature of intersubjective understanding. In this, he made use of the

term 'life-world'; though in a different sense to Husserl.

Giddens stated that Schutz, although making "due obeisance" to the idea of "the transcendental ego", was really concerned with "a descriptive phenomenology of the life-world". (Giddens, 1976, p. 27).

Wagner similarly argued that Schutz's:

"substantive attention was mainly directed upon phenomenological descriptive operations for the sake of his work on a sociology of the life-world Basically his phenomenology was eidetic."

(Wagner, 1983, p. 306)

Yet Wagner also pointed out that in Husserl's work the eidetic and transcendental levels were difficult to separate, adding that Schutz "did not cut himself off radically from transcendental phenomenology". (p. 306). Schutz himself made it clear that he used the "phenomenological reduction" concept only so far as it was relevant for his own concerns. (Schutz, 1972, pp. 43-4).

These were, basically, with 'understanding' how individuals interpreted their own 'lived experiences', that is, gave them 'subjective meaning', or explained them to themselves and, since the social world was necessarily intersubjective, how they interpreted the 'others' with whom they interacted. Unlike Husserl, Schutz was interested in the 'natural stance', in the 'taken for granted' attitude towards what was seen as existing in the 'real' world. As Giddens observed, this concern inverted Husserl's 'epoche'. (Giddens, 1976, p. 27). For Schutz, the study of this 'natural stance' or "the common-sense" or "everyday world", was a necessary complement to Weber's requirement of the need to take into account the 'subjective meanings' of actors, and his concepts of 'understanding' and 'social action', and thus develop a phenomenological social science. In his views of 'the natural stance' and the social world, and his idea of social science, Schutz used ideas from all his sources, but especially Husserl and Weber. He made use in particular, both in his

view of the 'social world' of actors, and his view of social theory, of Weber's 'ideal type', and "subjective theory of action". (Wagner, 1972, pp. 62-3).

Schutz's concept of the social world is discussed briefly first, then his concept of phenomenological social science.

Schutz considered that social phenomena were based on a world of 'common-sense' concepts held by social actors. According to him, individuals interpreted the intersubjective social world on the basis of:

"a stock of previous experiences of it, our own or those handed down to us by parents or teachers; these experiences in the form of "knowledge at hand" function as a scheme of reference."

(Schutz, C. P. I. 1967, p. 7)

(That is, as a basis for interpretation). This 'stock' of knowledge includes the "definite qualities" of the objects in the world, and the view that this world was intersubjective. This world of "everyday life" was one in which individuals had a "practical interest". It was one which was both:

"the scene and also the object of our actions and interactions. We have to dominate it and we have to change it in order to realise the purposes which we pursue within it among our fellow-men."

(Schutz, 1970, p. 73)

This 'handed down' knowledge constituted the paramount reality for individuals, but each individual was in a "biographically determined situation". This was "a physical and socio-cultural environment as defined by him", which included his spatio-temporal position, his "status and role" and also "his moral and ideological position". (Schutz, 1970, p. 73). This 'biographical situation' was based upon the history of his stock of knowledge, both 'handed down' and his own experience; and it affected the "purposes at hand" or "certain possibilities of future practical or theoretical activities". (p. 73).

The 'natural attitude' was to take the 'stock of knowledge' for granted,

as being 'objective reality'. It was in this sense that the external world, Schutz argued, was experienced as a set of 'typifications', depending at any particular time on the interest at hand, which was the reason for directing 'attention' to a particular 'experience', in the act of reflection.

'Stocks of knowledge', Schutz argued, were not equally distributed, because individuals had different 'biographies'. However, one aspect of typifications was that actors in social situations assumed, in the 'natural attitude', that 'others' shared their interpretations of the meanings they attached to actions, their own and the others involved in the interaction. (In this, symbolic interaction shared some ideas with Schutz's phenomenology. Indeed, he was influenced to some extent by the ideas of Mead, Cooley, Dewey and Thomas while in America. (Wagner, 1983).)

The assumption of common interpretation allowed for the maintenance of social reality, and for predictability and order in social relationships generally. However, because of the unequal distribution of knowledge, social actions could find themselves unable to "reciprocate perspectives". When this happened, it was argued, the 'social reality' was disturbed, and interaction broke down. In this case, new 'typifications' became necessary.

Schutz also saw the 'Life-World' in terms of different relations between individuals. These could be either a 'we relationship' or a 'they relationship'. The former were those 'consociates' with whom individuals had a "face-to-face" relationship, sharing "a community of space and...time". (Schutz, 1970, p. 184). In this relationship, reciprocal perspectives were more likely, or at least a shared understanding of the meanings of actions, because actors in this relationship were aware of each other, and could directly communicate. The 'they' relationship, on the other hand, could include 'contemporaries' who were simply existing at the same time, or 'predecessors' or 'successors'. In this relationship, the 'others' did

not "share in a vivid present". (Schutz, 1970, p. 218). Therefore, their 'actions' were more likely to be viewed in terms of 'ideal typifications', or categorisations. It was these in particular that tended to remain unquestioned, until something happened to disturb 'reality'.

Schutz also used the concept of 'multiple realities', first developed by James, to account for different 'provinces of meaning', such as, for example, the world of dreams, religion, or the sciences, where 'everyday knowledge' was suspended, and which therefore had to be interpreted according to different criteria. In a sense Schutz's idea of the world of social science could be seen as an application of this concept.

Having very sketchily noted Schutz's conception of the 'Life-world' and 'the natural attitude', his view of social science is discussed next.

It was something of a 'shock' to this researcher, when going more thoroughly into phenomenology, to find that the 'typification' that had seen it as concerned only with 'subjective meaning' was false. Schutz, like Husserl, was critical of 'naturalism' (in its first sense). However, he did not reject the idea of objective knowledge, since he stated that:

"the rejection of a purely "objective" or "behaviouristic" social science by the proponents of "meaningful connections" as the goal of social science is unwarranted."

(Schutz, C. P. I. 1967, p. 51)

Further, in the same discussion of "Concept and Theory Formation" in the social sciences, he made it clear that he had several points of agreement with 'positivists' such as Nagel and Hempel. He summarised these as being, first, that:

"all empirical knowledge involves discovery through processes of controlled inference, and that it must be statable in propositional form and capable of being verified by anyone who is prepared to make the effort to do so through observation."

(Schutz, C. P. I. 1967, p. 51)

His second point of agreement was that:

"theory means in all empirical sciences the explicit formulation of determinate relations between a set of variables in terms of which a fairly extensive class of empirically ascertainable regularities can be explained."

(p. 52)

The third area of agreement was that:

"neither the fact that these regularities have in the social sciences a rather narrowly restricted universality, nor the fact that they permit prediction only to a rather limited extent, constitutes a basic difference between the social and the natural sciences"

(p. 52)

Nevertheless, in spite of such agreement, these scientists, in Schutz's view, had misunderstood Weber's "postulate of subjective interpretation" of 'verstehen', and the aim of "the social sciences". The latter was to obtain "organised knowledge of social reality" (p. 53). This last term meant:

"the sub-total of objects and occurrences within the social cultural world as experienced by the common-sense thinking of men living their daily lives among their fellow-men, connected with them in manifold relations of interaction."

(p. 53)

In Schutz's view, "naturalism" and "logical empiricism" took this 'social reality' for granted, so that:

"Intersubjectivity, interaction, intercommunication and language are simply pre-supposed as the unclarified foundation of these theories."

(p. 53)

Moreover, if 'naturalists' such as Nagel restricted "experience" to "sensory observation", and in particular "overt action", then many areas of "social reality" would not be investigated. Schutz pointed out that, for example, "negative actions", or "refraining from acting" were not open to sensory observation. To deal with social reality 'verstehen' was necessary. The argument of Nagel against Weber was, Schutz considered, based upon a failure to "distinguish clearly" three aspects of 'verstehen'. These were the use of it as:

"1 as the experiential form of common-sense knowledge of human affairs, 2 as an epistemological problem and 3 as a method peculiar to the social sciences."

(p. 57)

The first referred to the way "common-sense thinking takes cognisance of the social cultural world". (p. 56). That is, with how social actors routinely interpret their own actions and those of others. It had "nothing to do with introspection".

For the second, or "How is such understanding or Verstehen possible?" (p. 57) Schutz argued that it was a "scandal" that so far a solution to this had not been found. Nevertheless, the solution of this problem was:

"one of the first things taken for granted in our common-sense thinking and practically solved without any difficulty in each of our everyday actions."]

(p. 57)

Such "common-sense knowledge of everyday life" involved ideas such as "mental constructs, syntheses, generalisations, formalisations and idealisations", and philosophers such as James, Dewey, Bergson and Husserl had shown that this 'knowledge' was the starting point for inquiry. Hence Schutz's interest in Husserl's ideas of consciousness for 'understanding' interpretations, and in the idea of 'doubt' as to the existence of 'the social world', as exemplified in the 'epoche'.

For the third, methodological aspect of 'Verstehen', Schutz, while still in favour of a 'scientific' and 'objective' approach on the part of social scientists, argued that, because the 'observational field' of social science was the social world, this required:

"exploration of the general principles according to which man in daily life organises his experiences, and especially those of the social world."

(p. 59)

These principles, according to Schutz, were based upon "the pre-scientific thinking of everyday life in the mode of typicality". (p. 59). By this last term Schutz meant that individuals used "constructs" or "ideal types" in the subjective meanings they gave to their own and 'others' actions, depending upon their 'problems at hand'. (p. 60). For Schutz, the social scientist, in exploring the social world, thus had to "refer to the

subjective meaning of the actions of human beings from which social reality originates". (p. 62). At the same time Schutz agreed that:

"the social sciences, like all empirical sciences, have to be objective in the sense that their propositions are subject to controlled verification and must not refer to private uncontrollable experience."

(p. 62)

He asked how it was possible to "reconcile" the two principles of the need for understanding the subjective meanings of actors, and the need for objectivity. He argued that:

"the basic insight that the concepts formed by the social scientist are constructs of the constructs found in common-sense thinking by the actors on the social scene offers an answer."

(p. 63)

These 'second level' constructs which were "in accordance with the procedural rules valid for all empirical sciences" were based on a form of typification. They were:

"objective ideal typical constructs, and, as such, of a different kind from those developed on the first level of common-sense thinking which they have to supersede. They are theoretical systems embodying testable general hypotheses in the sense of Professor Hempel's definition."

(p. 63)

This seems to mean, as this researcher interprets it, that Schutz's view of social science was not so far removed from the views of 'positivist' scientists as some proponents of 'interpretive' research have seemed to suggest. Phenomenology, in the Schutzian version, thus seems to add the need for 'understanding' the 'subjective meanings' of actors in an extended Weberian sense to the 'ordinary' demand for scientific objectivity, and thus not to be a totally different activity.

Schutz argued that the 'second level' constructs had to "include a reference to the subjective meaning an action has for the actor". (Schutz, C. P. I. 1967, p. 62). He considered that this was what Weber had meant by "his famous postulate of subjective interpretation". (p. 62). In building these 'second level' constructs, the social scientist, according to Schutz,

first "observed facts and events" in "social reality". From these, "typical behaviour or course of action patterns" were constructed, and these were 'co-ordinated' to "models of an ideal actor", for whom "consciousness" was imagined. To this model, were ascribed "a set of typical notions, purposes, goals, which are assumed to be invariant". (p. 64). However, these constructs were not "arbitrary", but had to accord with "the postulate of logical consistency and the postulate of adequacy". (p. 64). The first term meant "the objective validity of the thought-objects constructed by the social scientist", while the latter meant that these second-level constructs had to be such that:

"a human act performed within the real world by an individual actor as indicated by the typical construct would be understandable to the actor himself as well as to his fellow-men in terms of common-sense interpretation of every-day life."

(p. 64)

That is, Schutz appeared to be saying that a social scientist should use 'ideal types' of the 'ideal types' used by the 'real world' actors. There are some problems with this. For one thing, it is not clear to the researcher how this represents a real advance on Weber's use of 'ideal types'. Also, it is not clear whether 'understandable' means that the actor would agree with the social scientist's construct. Even when a 'model' was not consciously used, it was found in this research that the researcher's construct placed on an action was not necessarily accepted by the actor concerned. It seems a rather artificial way of trying to discover the subjective meaning an act may have for an actor, particularly when, in the 'natural attitude', reflection on 'experience' does not necessarily occur. Also, though the social scientist is suspending his own 'taken for granted' attitudes to the social world, it may be difficult to do this entirely, so the 'ideal type' he constructs could be contaminated by his own 'stock of knowledge'.

Schutz, in discussing "action", which could be "by commission or omission", that is, positive or negative, argued that Weber had not clearly

distinguished, in the motives for action, between "because" and "in order to" motives.

The former were causal, related to 'past' time, whereas the former related to the perspective of the 'future'. The 'ideal types' of second-level constructs had, in agreement with the "postulate of subjective interpretation", to explain action in a way that actors in real life would 'understand'. But, as indicated above, actors in real life may not always know the 'because' motives of their actions. If 'motive' is considered in terms of 'intentionality', if the 'because' aspect is unknown to the actor, it is difficult to see how it can be considered as a motive for 'action', or recognised by the social scientist, except by an extensive knowledge of the 'biographical situation' of the individual actor. Given the unequal distribution of knowledge, this seems difficult.

This has only been, again, a sketchy account of what seems to be Schutz's view of social theory. With Schutz, as with Husserl, no summary, let alone this, can do adequate justice. Yet this researcher's 'purpose at hand' was merely to try and discern the main points.

There are a few more general points which merit mention.

As previously mentioned, Sharp and Green considered that 'phenomenology' (which they confused with symbolic interactionism could not discuss 'structural' issues like 'power'. However, Wagner stated that Schutz "in contrast to all "small group" sociologists and some practitioners of symbolic interaction theory" (p. 64) did concern himself with these factors. He stated that Schutz:

"paid great attention to the larger contexts of concrete interactional situations in the sociological constructs of types of such situations. This applies both to "historical dimensions" and to "structural" contexts of the broader social and cultural configurations within which social situations occur."

(Wagner, 1973, p. 64)

Schutz himself referred to 'the state'. He argued that:

"Every "action" of the state can be reduced to the actions of its functionaries, whom we can apprehend by means of personal ideal types and towards whom we can assume a They-orientation, regarding them as our contemporaries. From the sociological view, therefore, the term "state" is merely an abbreviation for a highly complex network of interdependent personal ideal types. When we speak of any collectivity as "acting", we take this complex structural arrangement for granted"

(Schutz, 1970. p. 290)

Schutz also observed in the same work that:

"In the objective sense a social group is a structural-functional system formed by a web of interconnected interaction processes, social roles, positions and statuses"

(p. 310)

While this sounds rather odd from the point of view of those who placed phenomenology firmly in the 'anti-positivist' camp, Wagner pointed out that Schutz's view really only meant that a social system was seen as simply a collection of "intricate "networks" of concretely linked interactional situations". (Wagner, 1973, p. 65). Wagner added that Schutz's phenomenology thus provided a means "for the analysis of social phenomena from small scale situations to large-scale structures". (Wagner, 1973, p. 67).

Since any social situation can involve differential power between those interacting, there seems nothing in principle in phenomenology which prevents it dealing with 'structural' issues, so long as these are not reified, any more than there is in symbolic interactionism. If Sharp and Green had broadened their concept of 'power', and used a version of 'phenomenology' to consider the relationship between the head and teachers, as well as teachers and pupils, they could not have claimed that phenomenology could not discuss power. They seemed only to consider this in terms of 'the state' and 'ruling groups' in 'society'.

The 'phenomenological' approach has been used in other studies of schooling (apart from that by Sharp and Green). The work of Michael Young

and Esland and others, reported in "Knowledge and Control" (1971) was an example, and was one of the earliest works read. This focused on questions of the curriculum, on taken for granted assumptions about the nature of knowledge. This was part of the 'New' Sociology of Education noted before as an influence in the research interest. This particular work also led to ideas of the relativity of knowledge, which was not what Schutz intended for phenomenology.

Best challenged the view that the 'new' sociologists who classed themselves as phenomenologists (meaning Filmer et al, 1972) were following the ideas of Husserl and Schutz. In his view, there was:

"little validity in the claims of 'phenomenological sociology' to be a natural extension... What relation it does bear to their work resides in the fact that it does seek to comply with Schutz's central postulate ... (and by virtue of the very nature of phenomenological introspection is in this limited sense compatible with Husserl); that the search for truth about social reality must take account of the subjective meanings of individual actors. Beyond that, the similarities are hard to find."

(Best, 1975, p. 142)

Best's argument was that some 'phenomenological sociologists' had mistaken 'peripheral' concerns in Husserl and Schutz for central ones. He argued that Husserl's main interest lay:"

"in the pursuit, by a series of philosophical meditations, of the reality of our subjective conscious experience, hoping thus to found a 'bedrock' of knowledge, providing an essential 'a priori' for all sciences."

(p. 135)

Best further considered that presenting Schutz as:

"a radical critic of conventional sociology is, at best, an exaggeration: his postulates for social scientific research merely request a greater cognisance of the individual's subjective meanings than characterises conventional sociology."

(Best, 1975, p. 134)

This comment on Schutz's view of social science seems basically correct, though Schutz's work was not definitive. As Wagner observed,

he was working on his theories as long as he lived. (Wagner, 1983). In so far as Schutz did not hold one model of 'scientific' research, the seizing of 'phenomenology' as a tool to attack 'traditional' or 'positivist' sociology requires explanation, since the authors Best is criticising had presumably (and apparently) read Schutz and Husserl. The explanation may well have been that the 'new' sociologists adopted a 'new' paradigm, ignoring inconvenient aspects, as a means of disestablishing the 'old' practitioners and establishing a new 'status group', thus creating a 'change of elites', as Pareto might have said and Eggleston hinted.

Best argued that Schutz's postulates meant that social scientists who constructed "rational-action ideal type models" were not "misguided" but were "seeking reality in the only scientific way possible", providing they took note of individuals' 'subjective meanings'. (Best, 1975, p. 141). Best was therefore critical of those he termed "ethnomethodologists" who apparently "insist on studying actual situations at a crude empirical level". (p. 141). These were "at worst", seen as "totally unscientific", (p. 141) and at best as clearing up terms, acting as "underlabourers". (p. 141).

Best seems to be going too far here. If the idea of 'the scientific method' is rejected by philosophers of science, as indicated in the first sub-section, there seems no reason to impose one method on the social sciences either, whether 'new' or 'old'.

In this present research, some 'phenomenological' ideas were used. Like Schutz himself, the researcher took what seemed relevant for her concerns. However, there was no attempt to construct "'ideal type' models", so according to Best's view of Schutz's theoretical approach the research was 'unscientific'. However 'objectivity' was sought, so it attempted in one sense to be 'scientific', since this feature is generally accepted by Schutz and 'naturalists' as being part of a 'scientific' approach. The

researcher tried to suspend belief; or her own 'natural attitude', in the 'reality' of terms such as 'schools', 'teaching' and 'learning', perhaps with in the beginning a mistaken view of such 'bracketing'. She also tried, in the course of the research, to 'reflect' upon the research 'experience' as it was taking place, and to become 'aware' of her own assumptions or ideas about what was seen. Also, in the research, the 'subjective meanings' of the other actors in the research 'situation' were sought by the researcher. These formed the basis for subsequent conclusions. That is, the research was based upon a study of "an actual situation". Nevertheless, it was not confined to a description of the subjective meanings of the 'others' involved, nor did it ignore the researcher's own interpretations. The researcher does not believe that it is a researcher's task to provide a 'better' account than that of members, if that were possible, but to provide one which both places the subjective meanings of actors in a situation in a wider context and also subjects the 'meanings' attributed to those actors by the researcher to the same scrutiny as those of the actors themselves. The attempt in this research was to meet 'the postulate of subjective interpretation' and present an 'adequate' account, but also to present an honest one, and a valid one so far as this could be done.

As a general comment on 'phenomenology', whatever the particular form, it seems doubtful if this has really solved Schutz's problem of how an 'objective' account of 'subjective' meanings is possible, a point which will be referred to again when discussing 'methods' associated with 'interpretive' research.

This concludes the discussion of Schutz's phenomenology. Inevitably, the discussion has omitted much, and, given the 'spectacles' of individual interpretation of the work of others, has resulted in mistakes or distortions obvious to those with deeper knowledge. However, the account is what the researcher made of it, both initially and later.

The next, and final, one of the 'interpretive' strands discussed in this section is ethnomethodology.

Ethnomethodology is considered by some to be an off-shoot of phenomenology, though its practitioners, or some of them, dispute this. (Mennell, 1974, p. 52). It might also be considered as partly 'symbolic interactionist'. The basic theme of ethnomethodology shares concerns with both of these strands, for it is the study of exactly how social actors make sense of their world, of the 'ground rules' by which they do so. Much more than phenomenology, it has focused on small scale interaction, and it is thus more 'micro' than other 'interpretive' approaches.

Ethnomethodology is quite hard to actually define in terms of its field. It is generally attributed to Garfinkel, who gave his name to this "school of thought" of which he is considered the founder. (Mennell, 1974, p. 51). Wagner discussed the work of Garfinkel, and the latter's correspondence with Schutz, and stated that "ethnomethodology" was "an approach that does not deny its Schutzian origins". (Wagner, 1983, p. 242).

Gouldner stated that the main interest of ethnomethodology was as a swing towards the idea of sociology as a 'happening'. He argued that:

"Influenced by Alfred Schutz' phenomenology, his attention is focused largely on the structure of the shared and tacit ... that is, ordinarily unutterable-rules and knowledge that makes stable interaction possible. For Garfinkel then, the social world is held together ... by a dense collective structure of tacit understanding (what men know and others know) concerning the most mundane and 'trivial' matters, understandings to which no special importance ... is normally attributed, if indeed, they are noticed at all."

(Gouldner, 1971, p. 390)

Douglas distinguished between "linguistic ethnomethodologists" and "situational methodologists". (Douglas, 1974, p. 32).

The former, according to him, focus on "the concrete statements made by individuals". (p. 32). Harvey Sachs was principally responsible for this development. He was concerned with 'conversational analysis', thus

acknowledging, like others, including Schutz, the importance of language in the construction of social meanings.

These 'linguistic' ethnomethodologists used the idea of 'indexicality' in their analysis of conversations. This means the idea that:

"All conversations contain countless terms which are not explicitly defined in the particular situation. Sentences cannot be understood merely from the dictionary definitions of the words comprising them. The participants have to achieve 'operational' or working definitions of all such 'indexical expressions', drawing on not only their stock of knowledge, but also on their explorations of the situation at hand. This process is called 'glossing', and the working definition 'glosses'."

(Mennell, 1974, p. 53)

What this means is that two - or more - individuals in a conversation draw on a background of common 'knowledge' or 'experience', if they know each other well, so that not every idea is clearly stated, but is 'understood' by the other through this 'stock of knowledge'.

Anyone having experience of conversations with friends or family would agree that this happens. Teachers and children in a school may also build up over time such 'shared knowledge', as indicated by the 'Strawberries' incident related by Walker and Adelman in Stubbs and Delamont. (1976, pp. 138-9).

In research, therefore, a researcher needs to be aware of this feature of linguistic communication, and be ready to ask questions, not take conversational words for granted.

'Situational' ethnomethodology is similarly concerned with the underlying rules of situations, such as being a member of a family (or school class). It is concerned with the 'meanings' of actors and how these are related to "the members' (situational) actions". (Douglas, 1974, p. 33).

Garfinkel's idea for exposing these underlying rules was to persuade his students to go into familiar situations and act like a 'stranger', such

as a lodger in one's own family. This would upset the situation. According to Garfinkel, this 'upset' would bring out the 'rules'. He stated that:

"to produce disorganised interaction should tell us something about how the structures of everyday activities are ordinarily and routinely produced."

(Garfinkel, 1967, pp. 37-8)

Thus, Garfinkel was concerned with the mundane, taken for granted, even 'trivial' features of 'everyday life', and with the rules of social order, and with the 'glosses' or the "contingent achievements of organisations of common practices". (Garfinkel, 1967, p. 33). That is, with the accepted frameworks or ways in which people behave. Thus, ethnomethodology is basically a concern with practical reasoning used by social actors, and with the attempt to understand this, and so what it is that is routinely done in 'everyday life', on the most mundane level, in terms of a 'happening' or accomplishment.

If 'symbolic interaction' and 'phenomenology', as well as 'social action' are also concerned with 'meanings', then ethnomethodology's special feature is that it seeks to get at these by exposing them as an underlying feature of interaction. Thus, it is practical in intention.

It has been used in some studies of schools. For example, Payne analysed language in classrooms, and showed that it was something at which classroom members had to work in order for the lesson to be made to happen. (Payne, 1976).

Ethnomethodology was not used in any particular or detailed way in this research, although language as used by teachers to convey 'rules' was an interest. However, the researcher tried to act in a partial sense as a 'stranger' by endeavouring to present herself as a 'naive observer', not one who 'knew' what went on in schools. Unfortunately, this was undermined by having declared, as a means of gaining entry, that she had been a teacher. At least one teacher expressed some irritation with the

researcher's 'Why' or 'What'; directed at 'routine' classroom events. It seemed a problematic procedure for researchers to really be a 'stranger' in Garfinkel's understanding of that term.

This brief discussion of ethnomethodology brings to an end the discussion of the various 'perspectives' or 'strands' collectively known as the 'interpretive approach'. Thus, so far, this part of the section has summarised separately the main strands of this 'tradition' which were seen as most relevant, indicating which ideas were found useful, taking an eclectic view, based on the researcher's 'purpose at hand'. The summaries were also intended to show the researcher in the act of reflecting upon the various theories, and 'making sense' of them according to her own understanding, with its limitations. In doing this, the difference and similarities between the various strands were pointed out, as the researcher saw these. It was found that in some cases, such as social anthropology and phenomenology, there was less difference between 'interpretive' research and 'positivist' than appeared to be the case according to some critics of 'positivism'. There were also differences between the strands, or between practitioners in the same approach, such as symbolic interactionism, in the degree to which 'causal explanation' was considered to be a goal of research.

However, in spite of some differences, the underlying theme common to the various strands was that the 'subjective meanings' of those actors in the social situations being observed had to be studied, and any research had to take these into account, although 'structural anthropology' was perhaps an exception.

How these 'meanings' were to be studied was differently conceived. There was the concept of 'ideal type' as a model, or simply living with those studied over a greater or lesser period of time. This last approach seemed the most general. What is termed either the 'interpretive' or

'ethnographic' research has borrowed ideas from social anthropology, discussed in this section as part of the 'tradition' as was noted in discussing symbolic interactions, particularly the 'Chicago School'. The main idea borrowed, despite 'ideological differences', was the use of 'participant observation' as the principal strategy by which a researcher seeks to gain 'knowledge' about the particular group being studied, and their 'subjective meanings' that they attach to their actions.

Participant observation as such is described in more detail elsewhere in this chapter, along with the methods of actual data collection and analysis and their related problems. What is noted here is merely that it is not an unambiguous term.

McCall and Simmons, for example, stated that it could entail more than one 'method', such as social interaction with those studied, direct observation, and formal and informal interviewing, with perhaps also some counting, and use of documents. (McCall and Simmons, 1969).

An anthropologist described participant observation as:

"the observation of social life, as far as possible, from within, that is, at first hand, but it is also designed as a method of research, to obtain systematic information. Research consists of listening to conversation, quarrels and discussions, as well as asking questions, observing what people do as well as what they say should be done, and what they think of other people's behaviour."

(La Fontaine, 1985, p. 20)

Thus, participant observation seems to be, as Woods stated, "a combination of methods" though he also called it "a style of research". (Woods, 1986, p. 33). Kluckhohn defined it more generally as:

"... conscious and systematic sharing, in so far as circumstances permit, in the life activities, and, on occasion, in the interests and affects of a group of persons. Its purpose is to obtain data about behaviour through direct contact."

(Kluckhohn, 1940, p. 331)

Thus, whatever 'participant observation' is, it is not Best's study of situations at a "crude empirical level", the comment previously disagreed with. Research aimed at discovering, among other things, 'subjective meanings' seems necessarily to have to consider the real situations of actors. However, the last purpose mentioned by Kluckhohn might be also that of researchers in the 'other tradition'. Lundberg, for example, wrote that:

"Some of the most enlightening fieldwork has been carried out by means of a far more intimate and informal set of relationships between the investigators and the investigated, namely through the so-called "participant observer" technique. It consists of the observers becoming as nearly as may be a member of the group to investigate. The degree to which this is possible will vary with the characteristics of the investigation in relation to the culture of the community he has chosen to investigate."

(Lundberg, 1942, p. 375)

This last point is a salutary reminder of the fact that becoming a 'member' of another group is not an automatic or 'easy' procedure, and the extent to which it is in any case desirable depends again upon the 'purpose at hand' of the researcher.

The use of participant observation in surveys, usually attributed to the 'traditional' sociology, was also referred to in the previous sub-section. In any event, no researcher, whatever the method used, can totally avoid direct contact with those being researched to some degree at some stage.

The basic problem with all 'interpretive' approaches is that of how a researcher can actually be certain that he/she is getting at the 'inside information' of the situation as actors see it. This involves the problem of meaning. In the previous sub-section, as noted, Marsh discussed the difficulties of getting at this, even where surveys used participant observation. (Marsh, 1985).

Blackledge and Hunt have pointed out that in any case the term 'meaning' is itself "complex". (Blackledge and Hunt, 1985, p. 234).

Schwartz and Jacobs also argued that it could be difficult to really discover how 'reality' is constructed by actors. They stated that there might be no way to "accurately reconstruct the 'truth' or 'reality' of one's social existence". (Schwartz and Jacobs, 1969, p. 14). There seems no way in which it is possible to be absolutely certain about the information one gets access to, when this refers to the subjective interpretations of others. Outhwaite, for example, held that gaining:

"... some form of direct access to the content of other people's minds ... is not only empirically but logically impossible."

(Outhwaite, 1975, p. 27)

People may systematically set out to deceive, at worst, but even where this is not the intention, not everyone is able to recall what they have done, nor, if they do, to be able to explain it.

And it is unlikely to be feasible, in a research setting, to practice Garfinkel's conception of becoming a 'stranger' exactly as his students tried to do. Disrupting a school's situation in this way, to discover hidden 'roles', could end in a fairly rapid end to the research.

Even where this extreme is not practised, 'subjects' tend to react adversely to 'probing' of 'sensitive' areas by the researcher as was discovered in this research.

King said that the answer to the problem of meaning was "just ask". (King, 1978, p. 6). But the answers may not necessarily be 'truthful', or a person may quite logically hold one thing to be true at one point, and then change his view with 'experience'.

Phenomenology, at least, as observed, finds nothing inconsistent in this, but it makes the researcher's task, which is no bed of roses anyway, that much more difficult.

The only way to try and overcome, at least partially, the problem of meaning, seems to be in the first place to be as careful as possible in discussing with social actors why they do things, and with the meanings that a researcher attributes to them, referring these back wherever possible. Secondly, over time, discrepancies between statements and acts are likely to become visible if they exist, so that long time as against short periods of observation is more likely to get closer to what members 'really' mean.

A range of methods provides useful cross-checks for the researcher's interpretation, as discussed later in this chapter.

To conclude this section, the general finding is that there may well be less difference between the 'two traditions' than opponents of 'positivism' supposed. The idea of 'the' scientific method has been rejected, because 'positivists' also sought 'understanding', while not all 'interpretive' practitioners rejected objectivity and a 'scientific' approach. 'Structural functionalists' were also interested in subjective meanings, and 'phenomenologists' considered 'structure' (and 'functional' concepts perhaps). The 'power' issue was one with which 'interpretive' sociology could deal.

Thus, neither in interests nor methods are the 'two traditions' entirely and absolutely different. There is nothing in 'interpretive' sociology to suggest that one particular method has to be followed. As Eggleston stated:

"No one methodology is likely to be sufficient to explore any major area of enquiry, and any attempt to prescribe a single strategy can only lead to unnecessary restriction."

(Eggleston, 1975, p. 8)

In a similar vein, Farber, though specifically referring to phenomenology, suggested that:

"the best interests of phenomenology will be served by a fresh recognition of the principles of the co-operation

of methods, and, indeed, this is amply justified by the development of phenomenology as a whole."

(Farber, 1967, p. 545)

As indicated earlier, the researcher is basically in agreement with these views, having earlier pointed out that the idea that methods have to be rejected because they may be 'tainted' by theories is implausible, since participant observation, itself associated primarily with the 'interpretive' approaches, is 'contaminated' by association with 'structural functionalist' social anthropology.

Also, the researcher agrees with the principle of 'eclecticism' in respect of theories as well as methods. Schutz took what he considered to be useful and relevant for his concerns from many sources, rejecting those aspects which were for him unimportant. This seems a sensible procedure, using 'what will work' in the particular situation. The researcher has tried to indicate what theoretical aspects in the various 'interpretive' strands she found useful in this research.

The remainder of the chapter deals in detail with the overview and timescale of the research, choice of schools, the establishment of field relations and methods of data collection and analysis, discussing the various problems these entailed, and the way in which she attempted to overcome these.

SECTION THREE : AN OVERVIEW AND TIMESCALE OF THE RESEARCH PROCESS

This section briefly sets out to provide an overview of the research as a whole and the timescale, to show what was done and when, and to place the researcher in context as well, as befits an *interpretive study*.

The initial research was begun in February 1980. In the first instance the researcher registered for an M.A. in Education in October 1979. However, in October 1979 plans had to be altered for financial reasons, mainly that no grant was going to be available. This resulted in a part-time M.Phil. being undertaken instead. That was later upgraded to a D.Phil.

The first stage of the research included some general background reading on ethnographic methods, theoretical considerations and literature relating to primary schools. This period of reading was interrupted by having to return home after one month because of having to leave the initial accommodation and failing to find an alternative.

In February 1980 the researcher was able to return to University after having found some accommodation. An 'exploratory' pilot study, or what Woods terms a "trial run" (Woods, 1983) was begun in February 1980 and concluded in July 1980. During this period the researcher also worked in an hotel for ten hours each day including mornings and evenings. This was necessary given the lack of finance.

Five schools were visited, as noted in 'Setting the Scene', the following chapter. In order of visiting these were Briarfield Infants' School, Rushside Infants' School, Fairfield Infants' School, Stone Street Primary School, and finally a one day visit to Ashley Infants' School.

The length of time spent in each school varied from three days to one week. Most of the observations took place in the afternoon, writing up observation notes in the early evening as the researcher was working in an hotel, as noted, during the mornings and also at night, usually until midnight.

There were two main reasons for doing a pilot study. First it was done in order to gain a general picture of the organisation of infants' schools in the area. Secondly, it was undertaken in order to get used to the 'role' of being a researcher, learning how to observe, ask questions and record information.

During these visits observations were made in each classroom and a series of informal interviews conducted with each teacher. During these interviews the researcher raised issues which had come up during observation in particular classrooms. At the end of each period of observation

in each school the head teacher was interviewed. The nature of these observations and interviews is discussed in a later section.

On the basis of the observations and interviews in the above schools a report was written at the end of the first year. This outlined the main themes derived from the pilot study.

Towards the end of the period in the pilot schools, during which lodgings had to be changed again, a half day visit was made to Moorlands Infants' school. During this visit each classroom in the school was visited briefly and a short interview conducted with the head teacher. This school subsequently became the one in which the main part of the research was carried out. However, Moorland was not the first choice as is indicated in the following section.

The main research, undertaken at Moorland Infants' school, began in September 1980. By this time the researcher had found more permanent accommodation and given up working in the hotel because it interfered with the research. However, the researcher did a cleaning job which although it only took up a day each week, was very tiring physically, and took time to recover from. A decision had to be made to apply for financial assistance from the DHSS in order to continue research.

One school academic year was spent at Moorland plus part of a further term. During the first two weeks of September 1980 a brief period was spent in each classroom in the school, and also in the nursery attached to it. Overall about twenty visits were made to each classroom. Three days each week were spent in each, though not always full days. Some periods were spent 'helping out', for example hearing children read, supervising various activities, preparing materials and joining in with children in their activities. Time was also spent in the staffroom during break times, and some staff meetings were attended. Other periods were spent talking informally to teachers about various aspects of their work. During the year the head at Moorland was interviewed on several occasions.

Through the head and the teachers various documents were made available, such as the 'Report to a Managers' Meeting', the school record book and children's school records.

Towards the end of the field work at Moorland a questionnaire entitled 'Aims and Practice of Teachers in Infant Schools' was distributed to all the teachers including the head, at the latter's request, but administration of this by the researcher was denied. This is discussed in a later section in this chapter. A letter was also sent to the head teachers at Briarfield, Rushside, Fairfield and Ashley Infants' schools asking if they would be willing to complete a questionnaire. The heads of Briarfield and Rushside both declined stating that it was a busy period and teachers would therefore not have time to complete the questionnaire. Fairfield replied that they were willing to complete it as did the head at Ashley.

During the *summer term* at Moorland an adviser who was visiting the school referred to a school called Larkway which she considered to be "an example of good infant practice". She recommended that the researcher should visit it. A visit was duly arranged in the *summer of 1981* and one day was spent in the school looking at the classrooms; three reception, one middle and two top infants' classrooms and also talking briefly to the head teacher. The initial impressions were recorded together with some general details about the school.

During October 1981 the researcher left Moorland in rather difficult circumstances. This aspect is discussed later in this chapter. In general this was a period of intense stress and upset during which the research was nearly abandoned. It was only after having gone to Larkway that some degree of confidence was regained.

The research at Larkway started in February 1982 and ended in March 1982. Approximately six weeks were spent in the school. Some time was spent in the three reception classes, and a middle infants' class, and in a

top infants' classroom. A further two weeks was then spent in two of the three reception classes, and also two weeks in the Deputy's top infants' classroom. During this period observations were made in the classrooms and informal interviews held with the teachers. Each teacher was interviewed at least twice, apart from informal chats. The head teacher was also interviewed and contact was maintained with her after the field work had finished. The head at Larkway provided written statements on certain issues which had not been discussed in detail during the actual field work at Larkway.

Whilst at Larkway the researcher also had lunch with the staff and children, went into the staffroom, attended a staff meeting and went to a parents' workshop arranged by the reception teacher for parents of those children about to start school the following term.

As stated a questionnaire was completed by Moorland staff at the end of the research in the school. At Larkway teachers also completed one but at the beginning of the research there, for reasons which are explained in a later section.

By the end of April 1982, that is after just over two years in schools, the field work was completed. After completion of the field work the researcher began to go through the data, sorting it into categories and putting the material on to cards. However, the process of analysis actually began during the field work and continued well into the 'writing up' stage. It was a long, arduous process, and is discussed in more detail in a later section.

During the period between May and October 1982 a seminar paper was prepared and presented. It looked at some of the general problems in doing field work and also dealt more specifically with the particular problems faced by this researcher in doing her field work. Much of the material in this paper was incorporated later when writing the 'Methodology' chapter

although developed in far more detail, since many issues were not considered in the paper.

In 1983 the researcher continued to code all the material on to cards and completed the first chapter, 'Setting the Scene'. At the same time work began on the history chapters. A period was spent looking at the historical development of infant schools. The reasons for doing this have been briefly mentioned in the Review of the Literature and are also discussed again in a later section. The historical analysis involved not just a search of the literature relating to the historical development of primary schools but also an examination of archive material such as school record books dating back to the early nineteenth century and a local historian's account of the development of infants' schools. During the field work a report of the catchment area of Moorland was made available to the researcher which provided further references such as newspaper articles dating back to the early 1930s. These articles were examined. They provided further information about slum clearance and the development of estates such as Moorland.

A first draft of the history chapters was completed in 1983. At the time it was not considered wholly satisfactory and was subsequently completely restructured.

In October 1983 a second seminar paper was prepared and presented in December of the same year. The themes for this paper were developed from the fieldwork data and also from reading King's research on infant schools. The themes were 'teacher control and pupil compliance in the infant classroom'. This paper subsequently formed the basis of a chapter of the same name in the thesis.

Work on the research in 1983 was slowed down by a period of illness early in the year. A headache which persisted for three months prevented the researcher from doing much reading or writing during that period.

During 1983 the M.Phil. was upgraded to a D.Phil.

In 1984 work began on two further chapters, concerning the perspectives of head teachers, and teachers' social perspectives. The head teachers' chapter was completed in December 1984. However, it was completely re-structured at a later date.

Progress during this period was slowed down by further illness; back trouble and bronchitis. The former entailed physiotherapy three times a week for a period of six months and resting the back by lying down for two hours each day.

During 1985 work continued on the chapter on teachers' social perspectives and a first draft of the chapter was completed. The heads' chapter had contained sections on their 'social' and 'educational' 'perspectives'. It was decided that a similar format should be used for teachers' perspectives. Having done a chapter on teachers' 'social perspectives' work then began on one concerning their 'educational perspectives'. At the same time the 'Review of the literature was started, although some reading had been done at an earlier stage. A first draft of this chapter was completed by mid summer.

In July 1985 the researcher attended a course on qualitative methods at the University of Warwick. This gave insights into various aspects of data collection and analysis. More important than this, since the researcher had finished data collection there and also much of the analysis, were the views of other research students on their research training, which gave new ideas about the 'research role'. Informal chats with other students also revealed some of the problems and stresses involved in doing research. It was reassuring to learn that other people had experienced problems too.

Later in 1985 work began on a second draft of the history chapter. It was at this stage that it was decided to divide it into two separate chapters, one on the social discipline and social welfare aspect of the

development of infant schools, and a second on the historical development of educational ideas affecting the development of infant schools in the nineteenth and twentieth centuries. More references related to infant schools were incorporated at this stage. Searching the literature for such references was extremely time consuming.

In October 1985 the researcher started a City and Guilds course. It was necessary to do this in order to satisfy the requirements of the DHSS, and ameliorate the constant pressure by them to find work. Failure to satisfy their requirements could have resulted in loss of benefit, accommodation and having to leave the area altogether and return home. As government regulations were tightened up this was to become an increasing worry over the next two years during the most critical time of writing up. This stress did little to improve the researcher's general health.

In 1986 work continued on the restructuring of the history chapters. The second draft of both these chapters was completed by late spring of that same year. Work also continued on teachers 'Educational Perspectives'. This chapter took several months to complete as it was a difficult one to organise. Progress was slowed down by having to prepare for the City and Guilds examination in June 1986 and the chapter was not completed until the Autumn of 1986.

In October 1987 work began on a new chapter 'Control and Autonomy in the Infants' classroom', an account of what teachers and children do in the infants' classroom. At the same time the researcher began the second year of the City and Guilds course.

The above chapter was completed by April 1987 and the whole first draft of the thesis, apart from the methodology chapter which had not then even been started, was handed in to the research supervisor.

The period between May and June 1987 was spent revising for the City and Guilds examination, and also having a rest on the advice of the supervisor.

In July 1987 the researcher called at the university to find out what progress had been made with regard to reading the thesis. The supervisor handed back the thesis and wanted to discuss it there and then. As the researcher had just come from a five hour cleaning stint she did not think that this was feasible. At this meeting the supervisor informed the researcher that he was leaving the university in August. The researcher was told to go away and read through the thesis again in the light of the written comments made by the supervisor and then to arrange an interview at a later date to discuss any remaining problems. One major problem was that there was so much to read and it could not be done quickly. The other more important problem that needed discussion and was not even raised then, was that a methodology chapter had not even been begun.

In August 1987 an application for an extension had to be worked out as the initial registration ran out in September 1987. After having read through the thesis it was considered that major restructuring work was required on most of the chapters and it was stressed that the methodology chapter had not been written. The supervisor considered that most of the work would be revision rather than restructuring work and that as regards the methodology chapter a seminar paper on the problems of doing field work and a 'stream of consciousness' account could be used as the basis for it. The latter however, had been more of a 'wail' written soon after the completion of the field work at Moorlands, whilst in a very depressed state, in order to get it out of the researcher's system. Neither the seminar paper nor the second account dealt in detail with the nature of the methods used, analysis, and most important of all the theoretical underpinnings of the methods used. An extension was granted until the following September of 1988.

In August 1987 work began on detailed reading for the methodology chapter. At this time the researcher began to feel generally unwell and experienced a feeling of extreme lethargy. Consequently it was very difficult to keep awake so that much of the time was spent sleeping. This made

trying to read about rather complicated books on theories and methods very difficult.

In September 1987 the final year of the City and Guilds course was begun. At this time the researcher first developed a slight cold, and pains in the joints and spine and then suffered a complete loss of taste. It became impossible to eat most food. This resulted in a severe weight loss from around seven and a half stone to under six stone. The illness also affected the researcher's ability to concentrate. Even holding a pencil was an effort. These symptoms were accompanied by a dreadful sense of depression. It was inevitable that this would affect the progress of the thesis. In fact it was about six months before it was possible to resume work properly and the City and Guilds course had also to be abandoned. During this period the researcher was trying to come to grips with some very involved and complex series of arguments relating to theories and methods. The illness at such a critical time severely slowed down progress in 'writing up' the thesis. The illness was to continue to do so for the remainder of the writing up period. The virus infection was later to be identified as being glandular fever or a similar kind of virus infection affecting the nervous system. This type of infection has long lasting effects. It can take literally years, not months or weeks, to recover from it. It can also re-occur at intervals, even though subsequent attacks tend to be less severe than the initial one.

By January 1988 work was resumed on the methodology chapter. Due to the illness some of the literature on theoretical and methodological aspects had not been assimilated or understood properly, so much of the reading had to be done again. Work also began on planning the structure of the methodology.

It was difficult to concentrate on one chapter because other chapters also required major revision or restructuring. In February 1988 the Review of the Literature was re-read. As noted the first draft had been written

in 1985. On re-reading this chapter it was considered that there were gaps in information and that some of the arguments were over-simplified. Since 1985 other research had also been done on primary schools and so many of the references needed to be updated.

In March 1988 further revisions were made to the head teachers' chapter and 'Setting the Scene'. The former was completely restructured and some new material incorporated. This revision was carried out in order to sharpen up the chapter. The original chapter contained many ideas. The significance of these had not really been brought out clearly enough. Whilst work was being done on this chapter other revisions were made to 'Setting the Scene'. This chapter was completely re-ordered.

By April 1988 the second draft of the head teachers' chapter was completed and work began on a completely new chapter. This contained a discussion on various terms such as 'ideology', perspective and shared ethos. These terms had been initially considered in the chapters concerning head teachers and teachers. Upon reflection it was decided to discuss these terms separately in order to avoid repetition and to make the points stand out more clearly. This chapter was completed in April 1988.

During May and June work resumed on the second draft of the Review of the literature and was completed by the end of June 1988.

At the end of June 1988 work continued on the Methodology chapter in particular on section two concerning the theoretical traditions and associated methods. In July 1988 a first draft of the Overview and 'Choice of Schools' sections were completed and by the end of July 1988 the sections on 'gaining access' and field relations had also been written.

In August 1988 work started on the sections concerning 'methods of data collection' and 'recording data'. This work continued until September 1988.

In September 1988 a first draft of the section on theoretical traditions

and associated methods was completed. However, this section was later revised and elaborated upon quite substantially. Work also began on the 'Analysis of Data' section.

The first extension period finished at the end of September 1988. A further extension was considered and granted until the end of March 1989.

During October 1988 all the references were checked in the Review of the Literature and the chapter was prepared for typing. However, the typing of this chapter did not begin until December 1988. During October and November various aspects of layout and presentation were discussed with the typist.

It was noted earlier that many of the chapters needed major revision. During October and November 1988 both Chapters Five and Six on teachers' social and educational perspectives were re-read. It was considered that while many points had been made that these chapters appeared very muddled and that valuable material from the pilot schools had been left out. Work therefore began on the restructuring of these chapters.

At the same time work continued on the methodology chapter, developing Section Two and grappling with the analysis section.

During the period between January and March 1989 work continued on Chapters Five and Six and was completed. Work also continued on the 'analysis of data'. It was a very long process as was noted in a diary entry.

"It took all day just to write one page.
It's a very slow process."

(Diary : January 1989)

A draft of the personal reflections section was also written.

In January 1989 the researcher had to return to college to complete the City and Guilds course. Some students who had done the course the previous year had said that the final two terms were fairly relaxed. The

researcher considered that it would not interfere with the writing up of the research. However, in fact it was to place considerable demands upon the researcher and to interfere to some extent with the thesis work.

During February and March 1989 Chapters Two and Three were typed and proof reading done. This was an extremely slow process as the chapters had to be proof read several times. In fact amendments to the early chapters were still being made in July 1989. This slowed down the whole process of typing. In order to allow for the typing to be completed a further extension was granted until September 1989.

The whole process was also slowed down by another bout of illness between February and March 1989 when the researcher suffered an attack of laryngitis. There was also a re-occurrence of the lethargy and extreme tiredness which was experienced the previous year.

Between April and July 1989 work continued and was completed on the analysis of the methodology, aspects of validity and personal reflection as well as revision to Chapters Seven and Eight and the Conclusion. At the same time further chapters were typed and proof read.

During the most critical period of 'writing up' increased pressure from the DHSS meant that the researcher had to find a job. Work on a nursery plant started in June 1989. Although the job took only two full days per week this did reduce the time that could be spent on the thesis. Trying to resolve these conflicting pressures was extremely difficult.

The final three years of writing up which included virtually re-writing the whole thesis and writing the Methodology, was completed without supervision.

This overview has attempted to show that the whole process of doing the research and in particular the writing up, was a difficult and far from orderly one, and that the personal situation of the researcher imposed further difficulties which hindered progress during all stages of the research.

This overview has attempted to show not only the progression of the research but also to indicate that the situation of the researcher is as much a part of the research as anything discussed during the process and cannot be divorced from it.

SECTION FOUR : CHOICE OF SCHOOLS, AND ASSOCIATED PROBLEMS

In the review of the literature it was indicated why infant schools were chosen as a site in which to do research. This first part of the section looks briefly at the reasons why particular infant schools were chosen.

As stated in the previous section a pilot study was carried out prior to starting the main study in September 1980.

The first four schools were chosen using a telephone directory and a map of the areas. Schools were chosen from different environmental settings, for reasons noted in the next chapter. One school was situated near the centre of a city. One was located in a suburban area, one in a semi-rural area, and one in a village. Ashley was also a school situated on the outskirts of a city. The choice of schools was thus not totally random. The main reason for choosing the pilot study schools in this manner was to get a general picture of the form of organisation adopted in various types of schools, whether all through primary, junior or 'first' and 'middle' schools. An alternative way of finding out information about organisation policy could have been to contact the Local Education Authority but as stated earlier the pilot study also served as an introduction to the research role.

During the summer term of 1980 a decision had to be made as to which school should be the site of the main study. Rushside was the first choice. It was under new headship and it was hoped to focus on changes occurring with her arrival and staff relationships. Teachers at the school had provided views on the 'old head' and differences between her and the new one.

The head refused permission to observe in the school for reasons which are discussed later in the section when considering access.

Another school, Moorland, was suggested as an alternative to Rushside by the researcher's supervisor as one which would be interesting. The school had already been referred to by the head in one of the previous schools visited. A visit to Moorland was subsequently arranged and it was agreed that the main research could be done there. Moorland was 'chosen' because on first impression it appeared different from the other schools visited. It was considered that there would be less temptation to make judgements based on previous experience as an infant teacher. Moorland appeared to be different from the school where the researcher had taught. This was noted in a diary kept in conjunction with observation notes.

"My first visit left me with the feeling that this was a school very different from other schools ... visited and from the school I had taught in."

(Diary : July 1980)

As stated, Moorland was not the first choice and from the first visit there was a certain amount of unease regarding the school. This unease was noted in a diary.

"... at Moorland I immediately felt ill at ease. Children stared at me ... some glances were almost hostile. Some ... asked me, in not altogether friendly tones 'what you doin' ere' and 'why are you here'."

(Diary : July 1980)

This initial feeling of unease was, however, ignored. In retrospect perhaps it should not have been. At the time, however, this feeling was considered as being 'normal' and part of the research process. Junker spoke of the 'reality shock experienced by field workers'.

"Shortly after entering the field the novice field worker may experience reality shock as never before or again."

(Junker, 1960, p. 149)

It was assumed at the time of the first visit to Moorland that what the researcher felt was just such "reality shock".

After the research at Moorland, Larkway was visited. As noted, while at Moorland an adviser had recommended Larkway. Larkway was presented as a school which was situated in a very different area to Moorland and one having few problems. Another school, Morley Infant School with a similar catchment area to Moorland and situated near to Moorland had been considered in order to see whether teachers there held similar views about the children and the area as teachers did at Moorland. In the end this choice was rejected on the grounds that the head of Moorland was on the Board of Managers whose circuit included this particular school. Due to the fact that departure from Moorland was not entirely happy it was decided that choosing a school known by the head of Moorland would not be very wise. Therefore Larkway was chosen as the site of the second longest period of study. The visit there was also undertaken, as noted in the personal section, to re-establish some degree of confidence.

The next part of this chapter looks at the issue of access. First, it considers how other writers see this issue and its related problems. Next it looks at how access was gained to the schools in this research. Personal problems experienced by this researcher in relation to access are dealt with in the last section. This section does however refer to problems generally associated with gaining access.

SECTION FIVE : THE ESTABLISHMENT OF FIELD RELATIONS

This section is divided into two parts. The first deals with the gaining of initial access in the research schools, noting in the course of this some general issues which relate to this which are discussed by other writers. The second part discusses the establishment of a 'research role' amongst those associated with participant observation, and allied to this, the development of 'friendly relations' or 'rapport', again with reference to other research.

1. Gaining Access

The first hurdle to overcome in the attempt to provide an 'account' of an

infant school is to gain entry to the setting. This is an important stage in the research process. Johnson, for example, argued that successful initial access was not only a "precondition of the research" but also that how it is achieved may affect how the research is defined by people in the setting. (Johnson, 1975, p. 51).

Ultimately the way access is gained may also affect the "reliability and validity" of the data. (Burgess, 1984a, p. 45).

Gaining access is not a once and for all process only gone through at the start of the research, but a matter for continual negotiation. Also, entry has to be sought not just to the site but also to individual areas within it. Since initial access is usually through a 'gatekeeper' who then acts as 'sponsor', subsequent access may also be affected by the views others have of that person or body, whether favourable or unfavourable. Burgess noted the problems of trust this involved in the hierarchy of LEA - head - staff - pupils. (Burgess, 1984a, p. 39). Fuller also noted that as the head was her 'sponsor', it was the respect that other teachers had for him that made them willing. (Fuller, 1984, p. 108). Dua also noted similar problems of being associated with one group in the eyes of others. (Dua, 1979, p. 124). Thus, gaining access is a complex matter, and the implications may not be fully grasped when seeking it, through inexperience.

There are no hard and fast rules which set out how to gain access and maintain it. In a sense, one has to 'play it by ear' and learn while doing. Mistakes are very much a possibility.

In any event, even if 'rules' existed, what works in one situation may not succeed in another, as Agar pointed out. (Agar, 1980, p. 28). This seems true, not just for different researchers in different settings, but also for the same researcher within one setting, with different people.

This part of the section, in looking at how initial access was gained, notes the role of the 'gatekeeper', which in the case of schools is usually

the head teacher. It also notes the importance of 'impression management' as part of 'the presentation of self', in gaining entry, concepts derived from Goffman. (Goffman, 1969). Initial permission to do research in the schools visited was obtained from the Local Education Authority. This was done through the university. The next stage involved contacting the head of each of the schools chosen. As Fuller observed, this procedure "is directed by established etiquette". (Fuller, 1984, p. 105).

The initial contact was by telephone in all cases. Johnson considered that verbal communications possessed greater potential for re-negotiations than written ones. (Johnson, 1975, p. 69). The advantage of telephoning was considered to be that it offered immediate contact, and that this 'direct contact' would reduce the chances of refusal. This proved to be a good tactic as only one head teacher did refuse entry. This was on the grounds that staff were very busy because it was the end of the summer term. At the time, and in the light of previous experience as a teacher, this seemed a legitimate ground for refusal. However, a parent of a pupil at the school said that another 'student' had previously done some research in this school, which had aroused concern in the staff. In retrospect, this would seem to have been a more likely reason for refusing entry to this school.

As stated in the section on 'Choice of Schools', Rushside was the first choice for the main study. Whilst there was no problem in gaining access for a short period, the head refused permission for a longer period of observation on the grounds that the teachers were in the process of getting used to a new head, and also that, because of their experience with the previous one, were suspicious of any outsiders, particularly those who 'watched what teachers were doing'. This was unfortunate, because the position of having a new head, with staff adjusting to this, would have been an interesting focus for the study. Such a refusal by a head indicates the importance of the 'gatekeeper'. Gatekeepers can be defined as:

"those individuals in an organisation that have the power to grant or withhold access to people or situations for the purposes of research."

(Burgess, 1984a, p. 48)

As stated, telephoning the other schools was found to be a successful tactic. The initial call to each school was brief. In it the researcher established that she was a former infant teacher interested in doing some research in infant schools, comparing her own experience with other schools, and also that she was from a university. This was followed by an enquiry as to whether the head had any objection to the idea of research in 'her' school. In the case of two of the pilot schools, Briarfield and Fairfield, both heads said that they were used to visitors and that the researcher was welcome. They did not require a written letter outlining the research for the short visit projected, but suggested that the researcher should visit the school, talk about the proposed research, and have a look around. In the case of Moorland this talk was more extensive because the researcher had 'chosen' to do the main research there. At Larkway, the preliminary visit was made on the basis of a telephone call. When it came to the main period of research there, the school was again first contacted by telephone, to remind them. The head and deputy then requested a letter setting out the ideas for research there. This was sent, and was followed by an extensive interview giving more information.

It has been noted that 'impression management' was seen as very important, particularly during the initial access stage. Vidich also argued that the images the researched have of the observer are: "central to the definition of his social position", and, further, will shape the conditions in which the researcher works. (Vidich, 1965, p. 78). An important aspect of this 'presentation' is personal appearance, including the way the researcher dresses. (Hammersley and Atkinson, 1983, p. 78). Bogdan and Taylor argued that if the researcher wants to gain "the confidence of the administration", then he should "dress more formally" (p. 45).

They stated, however, that dress was important in blending "into the setting" as part of establishing 'rapport'. (Bogdan and Taylor, 1975, p. 65).

Woods also stated that dress is important for establishing credibility. (Woods, 1986, p. 66).

On initial visits to the school the researcher wore a conservative outfit to interview the head. This consisted of jacket and skirt, stockings and heeled shoes. In all but one school this matched the attire of the head. At Moorland, however, the head was dressed more casually, usually in a shortish denim skirt and blouse, jumper or cardigan. Here the researcher felt out of place dressed 'formally'. On the following visit, having ascertained that this would be acceptable, trousers instead of a skirt were worn, as it was winter. Staff in the school also wore trousers on occasions, so the researcher felt she 'blended' in more when dressed casually.

Another aspect of 'impression management' is the establishment of the researcher's credentials, in order to convince the 'gatekeeper' that one is a 'worthy' and can safely be admitted. This can create some problems for the researcher in matters such as stating connections and establishing 'status', and thus for the kind of 'reciprocal relationship' which can be established.

Schatzman and Strauss stated that information should be given concerning the researcher's "organisational affiliation" (Schatzman and Strauss, 1973, p. 23). However, announcing this may not necessarily elicit a favourable response from the 'gatekeeper' or later, others, depending on their previous experience, which can affect their perceptions of the researcher.

Schatzman and Strauss themselves noted that a host "may hold a low opinion of the researcher's affiliations". (Schatzman and Strauss, 1973, p. 27). These may be suspicions regarding the organisation, and there can be other problems. For example, stating that a researcher is a university

student may evoke for the host the image of other students who were not respected. (Hammersley, 1984, p. 49).

If the status is obviously higher, such as senior member of the university, this may cause the host to view the researcher more differentially, at the cost of some constraint on the relationship because the relationship will be that of superior to inferior. Burgess cited the "Dr. King Miss Pink" relationship as an example in noting this. (Burgess, 1987, p. 79). However, there is no evidence on how King actually presented himself to the heads and teachers he visited, and he himself made no statement on this. Burgess's main comment on difference was based on his own observation of how higher education staff were treated by heads and teachers.

Stating that one is a 'researcher' may also cut no ice with the hosts. Hammersley noted that teachers in the school he researched:

"had an ambivalent attitude towards research and researcher. They were very dismissive of all 'experts' and there were comments ... about the irrelevance of theory and the importance of practice and experience."

(Hammersley, 1984, p. 49)

Woods similarly stated that some teachers could be "anti-research" as well as "difficult in other ways". (Woods, 1980, p. 28). Walford also noted "a deep suspicion of educational research in general and of this sort of research in particular" in his account of doing research in a public school. (Walford, 1987, p. 65).

In the light of what happened later in the present research this comment might be thought true of some teachers in other types of school. Thus a researcher may be seen as inferior, which can also have the effect of reducing communication.

If presenting oneself as a student of a university, or researcher, presents problems it might be thought that these would be overcome by presenting oneself as having had experience as an 'infant teacher', 'one

of themselves'. Woods noted that previous teachers' experience could work in this way. (Woods, 1979, p. 26). But intimating that one has been a teacher also has its problem side.

As is noted later in Chapter Four, interviewing a Head can feel strange if the teaching experience has been as an ordinary classroom teacher. In other words, there is the problem of inequality of status in reverse from that of the position of senior university staff and heads. The researcher may feel deference or unease. This can restrict communication, as Porter also noted. (Porter, 1984, p. 156). Also, teachers themselves, later in the research, may assume that "you know how things are - you've been a teacher". Adopting the position of 'naive stranger' can be very difficult. Also, teachers can wonder why you are not still teaching.

The researcher may also fail to notice some events just because the classrooms are familiar, in the sense that most infant classrooms contain very similar materials and activities there also have similar patterns. Burgess notes this problem of familiarity. (Burgess, 1904a, p. 22).

Other aspects of status which can affect 'impression management' either favourably or adversely, are age, gender and marital status, especially where there are differences in these respects between the 'gatekeepers' and the researcher. A male researcher, for example, might not have considered it necessary to ask permission of a female head to wear casual dress. These matters affect relationships, generally. (Burgess, 1987, p. 79).

It has been pointed out so far that presenting one's 'credentials' as part of 'impression management' is not a simple issue, but one with several difficulties, especially in the case where status inequalities exist between the researcher and the host.

Impressions the hosts have of the researcher may not accord with the researcher's own personal perceptions. Presenting oneself is a two way process, with the possibility of mismatch in perception/expectations between the participants.

In the case of initial interviews with head teachers in the pilot study schools the researcher's affiliation to a university did not appear to interest them particularly. In the case of Moorland and later Larkway, head teachers did ask for details of this kind. The difference was probably due to the fact that it was clear to the pilot study schools that the visit was short term, while in the other two the stay would be longer.

Also part of 'impression management' is the giving of 'explanations' about the research. This can present problems, which may continue. Dua, for example, stated that she found explaining her study and role difficult "almost throughout the period of my fieldwork". (Dua, 1979, p. 119). Also, explanations are not necessarily understood. Boissevain, reporting that he used a parish priest to pass on his explanation, stated that people did not always ask the priest, "and not all those who did understood the explanation". (Boissevain, 1970, p. 72).

Dean, Eichhorn et al argued that a plausible explanation must be given "to those being researched". (Dean, Eichhorn, et al, 1969, p. 18). The explanation has to be satisfactory. That is, the reason for the research has to seem sensible and understandable, and the proposed research must not frighten the subjects right at the beginning if the research is to get underway.

Bogdan and Taylor advised that in giving an explanation the researcher should "tell the truth" but added that there was no need to give "elaborate details". The explanation should be in general terms, and not use "social science vocabulary" (p. 34). Their evidence was that "to be honest, but vague and imprecise" (p. 35). They also stated that a researcher must make it clear that the confidentiality of the various subjects and the institution will be respected. They regarded this as part of the research 'bargain' made with the gatekeeper. (Bogdan and Taylor, 1975, p. 35).

During the initial interviews with head teachers in the Pilot Study schools and at Moorland the researcher was asked to 'explain' the nature of

the research. At Larkway this last, and the researcher's affiliation was set out in a letter and later discussed in an interview. This is referred to later.

During the initial interviews in the pilot schools the researcher explained that she was a former infant teacher, hoping to do some research in infant schools, and that the researcher would be interested in how an infant school worked and the daily life of infant schools. This appeared to be accepted as a sufficient reason for visiting the school.

In the interview with the head teacher at Moorland the nature of the proposed research was explained on similar lines to those presented to the head teachers in the pilot schools. It was also stated that the researcher was interested in how teachers worked in an infant school and what teachers and pupils do, and the daily life of an infant school. It was also stated that there was a need for visits over a long period. The head teacher said that she did not object to the research but that she would have to consult the staff. Permission was eventually given, although it was stipulated that the researcher should not come into school at the beginning of the term because she thought this would give the teachers and children time to settle down. On arrival at the school in the following September it was found that teachers in the school had not been informed about the proposed research. This meant that some explanation had to be given to each teacher.

The procedure for getting into Larkway for the main period of research there was rather difficult. A letter was sent to the head asking if the staff would be willing to complete a questionnaire on teaching approaches. (The use of a questionnaire will be discussed in a later section). The letter outlined the researcher's university affiliations. It recalled the earlier visit and also asked whether a longer period of observation was possible. The head had said previously that the researcher was welcome to visit the school again. The deputy of Larkway replied to the letter and

stated that: "We will be pleased to see you again and give you any help we can". She asked for further details about the proposed questionnaire, requesting a copy of this together with further details concerning the timescale of the research and what the researcher wanted to do. She asked, for example: "Would some of it be just your own observations, seeing children working in the classroom?" (Deputy, Larkway).

After a further letter stating that it would be interesting to observe in the classroom an interview was then arranged with the deputy. The process of gaining access to Larkway involved more negotiation than at any of the other schools. During the initial stages, however, the deputy laid down certain ground rules which in fact were re-negotiated and,

"She laid down the length of time I was to stay in the school I settled for four weeks but I hoped I would be able to extend this period after I had been in the school a little while."

(Diary Notes, 1982)

In effect, the deputy at Larkway acted as a 'gatekeeper'. However, in view of her relationship with the head, one of friendly co-operation, it was unlikely that in granting access she was making a decision which would be disapproved of by the head. It was more a matter of delegation. As it turned out the researcher stayed in the school until near the end of term. The researcher also emphasised that she would try to remain unobtrusive.

"I will try to keep in the background as I know from experience that teachers are very busy people."

(Interview : Deputy, Larkway)

A timetable was discussed and the researcher said that she wanted to visit certain classrooms and, "if possible, your own" (Interview : Deputy, Larkway).

An outline of the research was given to the deputy to pass on to the head. This provided certain information about the researcher's background and teaching experience, how a research project in infant schools had arisen and the nature of the proposed research and a timescale. The deputy dis-

cussed the research outline with the head and informed the researcher the same day that she could come to the school, thus indicating that she was acting with the approval of the head. She also informed the staff in the researcher's presence during lunch-time in the staffroom. They were thus able to ask the researcher any questions. The nature of the research was explained individually to teachers if they came and requested more information.

It was stated earlier that negotiation is a continuous process. Thus having gained access to the school the next stage was to gain access to classrooms. This was always done through the head in the first instance. As noted earlier in this section, introduction by the head is not necessarily beneficial to the researcher. After each initial interview the head teachers conducted the researcher round the school and introduced her to each member of staff. In the case of the Pilot schools one head permitted free access to any classroom at any time provided individual teachers did not mind. Two other heads stated which classroom could be visited initially but after that allowed free access, again provided teachers didn't mind. In only one school in the Pilot Study did the head lay down which classrooms to visit. In one instance the class was the responsibility of the deputy and the head considered that it would be polite to visit this classroom first.

In the case of Moorland the head showed the researcher round the classrooms and thereafter permitted free access as long as the teachers were consulted first. In the case of Larkway there was an initial tour and 'introduction' to each teacher. The researcher was then left to find her own way to the classrooms she wished to visit.

As stated, the research was explained to each teacher.

In the Pilot Study schools, the statement that the researcher had been an infant teacher was accepted as a reason for interest in the schools.

The teachers came up and talked to the researcher, and were interested enough to ask questions and not in an unfriendly fashion.

At Moorland the reaction was somewhat different. Initially, although polite, they seemed relatively incurious. This contrasted rather with the attitude of the pupils. They wanted to know something about the researcher such as a name. One boy was much more direct. He asked bluntly, and not in a very friendly fashion: "Who are you - what you doin' 'ere?" On the whole the teachers at Moorland did not make any direct, overt comments about the research. They did not openly question it. During the first few weeks only one teacher actually asked what the researcher was doing and said that she still did not really understand what it was about. It was only towards the end of the research that their alleged uncertainty became known, through the comments made by the head and another informant. The researcher had no means then of checking the accuracy of these comments, and whether they referred to one, or more than one, teacher, in fact.

Dean and Eichhorn stated that the researcher should not be too reticent in discussing the nature of the research, for talking about it, they said, "allays fears and suspicions". (Dean, Eichorn et al, 1969, p. 69). This assumes that those researched express some interest. It also assumes that the explanations given will be understood by those researched in the same terms as the researcher.

However, a written statement was later given to each member of staff, as the head considered that if the teachers knew what the researcher wanted, they would be more inclined to talk. It did not seem to make much difference, especially with one teacher, Mrs. Dale, and the Deputy, Mrs. Martin, was very quiet in any case. In time, however, with other staff though relationships became easier over time and in the nursery there was never a problem.

In retrospect, the relative incuriosity of Moorland staff, and the difficulty of talking to some, could have had other explanations than simply the inexperience of the researcher. It was noted earlier that presenting organisational affiliations is not necessarily beneficial. Now the Moorland area had been the subject, a few years before, of a survey of tenants' views and needs. A number of ideas had come from this, and one was the need for play facilities for children. A questionnaire had been given to a sample of parents about parental involvement with the school and possible play facilities at the school. At the time of entering the school it was not known by the researcher that the questionnaire had been given to the school. The survey report itself had been given to the researcher by her supervisor just after starting at the school. Only a comment made in the early stages, that a teacher could not be seen after school one day because there was going to be a staff meeting on the questionnaire, alerted the researcher to the possibility of some connection of the school with the survey. The researcher interviewed the head the following day, and then saw her supervisor, and asked about the questionnaire. There was some worry about continuing at Moorland, because it seemed so difficult, but the full implications of the relationship between the supervisor and the school and the researcher's possible position were not seen. The immediate concern of the researcher was to 'get started', having already experienced some difficulty in finding a school for the main research. Thus, even when alerted to a possible problem, the researcher 'glossed over' it. But the researcher could have been seen as a spy. Apart from this, there were internal problems in the schools which also were not known at the beginning.

2. The Establishment of the Research Role - The Development of Rapport

As part of discussing the researcher's role, the section first considers the question of 'choice' of a research role. The problems associated with this question are noted. These are, first, that within the general

heading of participant observation there are said to be a range of possible roles. Secondly, these roles are not necessarily clearly distinguishable from another. Thirdly, choice is related to purpose and within the overall aim of 'providing an account' there can be secondary lower level purposes. Fourthly 'choice' may be influenced by those being researched. Examples are given from other research.

The section begins, therefore, by discussing these problems.

It continues by noting some of the advantages and disadvantages associated with some of the possible roles in relation to those utilised by other researchers in schools, although some reference is made to those concerning the above problems.

The main part of the section is devoted to a description of how, having successfully negotiated initial access, the researcher set about establishing her own roles and attempted to establish 'friendly relations' or 'rapport' with the hosts.

Within 'participant observation' there are various roles which can be adopted by the 'fieldworker'. (Gold, 1958; Junker, 1960).

Both these authors distinguished between four field roles. These were: complete participant, participant as observer, observer as participant, and finally complete observer. London added two more; adviser and informed. (London, M., 1978). Burgess stated that "the first two of these four roles are most often adopted by participant observers". (Burgess, 1984a, p. 80).

The 'complete participant' role is one where the field worker's activities are concealed and the researcher shares in the life of the observed.

The 'participant observer' role is one in which the researcher's activities:

"are not wholly concealed, but kept under wraps
... or subordinated to activities as participant."

(Junker, 1960, p. 51)

The 'observer as participant' is one where the role of the observer is made public at the beginning of the research. (Junker, 1960, p. 37).

The 'complete observer' role is one where the observer has no contact with those observed. (Gold, 1950, p. 37). He/she may even observe through a "one way mirror". (Junker, 1960, p. 37).

Junker considered that in practice these roles could not clearly be distinguished. This appears to be an accurate comment. For example, Woods claimed that there was no such thing as a 'complete observer'. He pointed out that even the total non-participatory observer "though not sharing any roles under observation is nevertheless part of the scene". (Woods, 1986, p. 39). Minocha said of her research in an Indian women's hospital in Delhi that it was impossible to be "a mere spectator". (Minocha, 1979, p. 215). Junker himself said of this role that it was "... more imaginary than real". (Junker, 1960, p. 38).

The lack of clear distinction is even more evident in relation to the roles of 'complete participant' and 'participant observer'. It could be argued that there is no such thing as 'complete participation'. Even though the researcher in this instance is sharing in the daily life of the researched group, he still must maintain the role of researcher and observer, or otherwise run the risk of 'going native'. As Hammersley and Atkinson stated:

"Whilst ethnographers may adopt a variety of roles, the aim throughout is to maintain a more or less marginal position."

(Hammersley and Atkinson, 1983, p. 100)

The difficulty of distinguishing completely between these two roles is shown by the two following examples.

The first example is that of Sullivan. Hammersley and Atkinson described

this as one of 'complete participant'. Sullivan et al described the role played by Sullivan as "participant observer".

Sullivan was a research officer in the American Air Force. In order to evaluate a training programme it was decided by his superiors that he should 'enlist' as a trainee himself. This was done in order to:

"gain insight into the motivation and attitudes of personnel (in training) as reflected in both their military and social behaviour."

(Sullivan et al, 1970, p. 91)

The researcher's role was hidden from his fellow trainees. However, his superiors knew, and the "co-operation of key personnel in the highly structured military establishment" (p. 97) was necessary for the research to take place successfully. The 'trainee' also had to make reports, hence he had, necessarily, to maintain some degree of 'marginality'. (Sullivan, 1970, p. 97). Spindler also stated that an anthropologist must keep his own identity, and even observe himself, and distance himself from those being observed. (Spindler, 1970, p. 288). Lofland, however, noted that "marginality" had "attendant feelings of lowness and anxiety and its continued fears about acceptance by people in the setting". (Lofland, 1971, p. 97). Hammersley and Atkinson likewise noted that "this is not an easy position to maintain, it engenders a continual sense of insecurity". (Hammersley and Atkinson, 1983, p. 100).

The second example is that of Huntington, who, in a study of a Hutterite community, took her whole family, husband and three children, to live in a community. The research role was described by Huntington as "participant observer". (Hostetler and Huntington, 1970, p. 211).

The Huntington family did everything that members of the Hutterite community did in their daily lives. They took part in the 'correct' work activities according to age, sex and status. They were 'encouraged' to wear the same type of clothes as community members. The one child who was old enough to attend school did so, her mother wrote that:

"She participated fully in the programme for the school children, remaining under the direction of the German teacher for virtually all her waking hours. She ate her meals with the school children and did the community work required of them."

(Hostetler and Huntington, 1970, pp. 206-7)

This child also reproved her mother for not wearing the correct dress.

Huntington claimed that because of the degree of participation, they were able to learn about various aspects of Hutterite life. Also, she stated:

"Because of our family structure, we were able to participate in every age level within the colony except that of adolescents."

(Hostetler and Huntington, 1970, p. 207)

The rules were made more explicit because of the presence of the children, one of whom 'rebelled' and found it difficult to suppress his individuality. (p. 209). Thus, in effect there was more than one researcher present, so more aspects were covered at different levels.

To all intents and purposes, the Huntington family were 'complete participants', even though their status as 'researchers' was not hidden. Notes had to be made whenever possible, as they had little privacy from the rest of the community. Yet some degree of 'marginality' had again to be maintained. They were researchers, not Hutterites.

If the distinction between 'complete participant' and 'participant observer' depends on whether the role is 'hidden' or not, Hammersley and Atkinson's comment seems appropriate. They said of the other distinction between 'participant as observer' and 'observer as participant' that "whether [it] is of any value is a moot point" and added of Junker's typology in general that the problem was that:

"it runs together several dimensions of variation that are by no means necessarily related."

(Hammersley and Atkinson, 1983, p. 96)

It is not clear, for example, why 'complete participation' requires that the role be hidden, as the Huntington example indicates.

As noted previously, Junker did say that in practice it was difficult to distinguish clearly between the various role positions. He also added that the researcher may well shift from one to another in the course of the research. (Junker, 1960, p. 38). Burgess makes a similar point. (Burgess, 1984a, 0. 84). This seems inevitable because at different stages of the research the researcher may have different concerns, and one position may be more appropriate than another for pursuing these.

King, for example, stated that his observer role allowed him "to observe uninterrupted and to make notes of my observations". (King, 1978, p. 4). Bellwinkel also stated that:

"Watching the scene as a neutral observer had supplied me with a mass of data that was concrete but not truly objective"

but added that only when he became an active participant:

"that an objective appreciation could emerge. In the process of such involvement, asking questions increasingly became mutual communication...."

(Bellwinkel, 1979, p. 147)

London stated that all or most of the roles listed might be played in the course of a single day. (London, H., 1978 (Appendix)), while acknowledging, like Hammersley, that the role adopted by the researcher largely determines what he/she can do, where he/she can go and with whom he/she can interact, also stated that:

"If he cannot freely define his own role, the observer must be careful to assume that role which is most strategic for obtaining information that is central to his scientific concerns."

(Straus, 1964, p. 29)

Apart from small problems such as the use of 'he' for researcher and 'scientific', this comment is making valid points. He noted that 'strategy' must relate to concerns. However, Straus's comment also highlights another problem. As he seemed partially to acknowledge, it is not always open to the researcher to 'adopt' or 'assume' a particular role. The people being studied can affect this, as Walford revealed.

Recollecting his initial interview with a headmaster who made it clear that "open ethnographer" was not a role acceptable to him, Walford remarked that:

"This was the first occasion when it became clear to me that possible research roles were structured by the definitions of others."

(Walford, 1987, p. 50)

This may be true not only initially with the head, but also with different individuals during the research. Thus the researcher does not 'adopt' one role, but rather 'adapts' to circumstances.

These circumstances include the view that others have of you. Minocha, for example, related that patients in the hospital where she was researching classed her as a doctor. She said that it was difficult to account to them for her presence if she was not a doctor. Accordingly,

"It seemed more convenient to assume the label of doctor and try my best to live up to the role assigned to me."

(Minocha, 1979, p. 209)

She was also used by the patients as 'adviser and informant', in London's terms (p. 209). She also stated, however, that in her study she became more involved with the study of patients than she had originally planned, because of the unwillingness of the doctors and their attitude to "sociological investigation", of which they were sceptical. (p. 205).

So far, some of the problems associated with the 'choice' of research role have been noted.

The section next discusses, with reference to accounts of interpretive work in schools, some of the advantages and disadvantages of the research roles that were employed.

One of the problems about undertaking research in schools is said to be that there are not many roles available, and those that are may not be particularly useful for research that must be of limited involvement.

(Fuller, 1987, p. 102 citing Wolcott, 1975).

It is certainly true that schools are a divided society in more than Woods' (1979) sense. They are divided into a hierarchy of teachers, of teachers and ancillary staff, and of teachers and pupils. But they are not alone in being so divided, and having relatively few roles that a researcher can actually 'take' as a member. Most social groups or organisations have some hierarchy. There are positions 'graded' in status according to age, or gender or other factors. (Burgess, 1987, p. 78). Anthropological studies show that a legitimate role is hard to find because of, for example, kinship divisions, which restrict communication between groups. (Hart, 1970, pp. 151-2).

In schools, researchers cannot actually be pupils, although they may be given the status of "honorary pupils", (Fuller, 1984, p. 103), or attempt to follow the pupil role, as did Spindler, (cited in Burgess, 1984b, p. 83).

Given this problem, researchers in schools have chosen between two basic options. They have either elected to be mainly observers in order to remain 'neutral' in relation to the groups of teachers and pupils or, with the necessary experience, have chosen to participate as teachers to a greater or lesser extent. Both options have problems.

King as noted, took an observer role in his study of infant schools. He gave as his reasons the wish to observe without interruptions. Thus he avoided children trying to treat him as a teacher, through keeping standing, referring children to teachers, and avoiding "eye contact". (King, 1978, p. 4). He described his intended relationship with the teachers as: "an interested, non-judgemental observer", (King, 1984, p. 123) while having noted earlier that some "would have preferred a closer relationship". (p. 121). King pointed out that keeping some distance in relationships avoids problems with friendships. Trying to remain 'neutral' avoids

being drawn into conflicts and perhaps being manipulated, a problem noted by London. (London, 1978). Becoming involved in the hope of establishing greater rapport does present difficulties. As Varadachar stated:

"Rapport is not a permanent credit card. In fact, it waxes and wanes in the field situation."

(Varadachar, 1979, p. 130)

A researcher may establish 'rapport' with some people, but this may mean that others in the research setting, because of internal differences with those same people, may then regard the researcher as an 'enemy'.

On the other hand, lack of involvement can result in some information not being made available, through mistrust of 'neutrality'. Vidich stated that neutrality was not only difficult to maintain, partly because:

"By virtue of his research ... the investigator must react to the actions of his respondents ..."

but also because:

"neutrality ... is a form of reaction and not only will be seen as such by all parties to the conflict but also implies specific attitude towards the issue being above it, outside it, more important than it, not interested in it. Whatever meanings respondents attach to neutrality will, henceforth, be used as a further basis for response. Failure to make a commitment can create resentment, hostility and antagonism just as easily as taking a stand. In both cases, but each in its own way, relationships will be altered, and hence data will be affected."

(Vidich, 1970, pp. 169-70)

Delamont also stated that she did not regard herself as a participant observer, since she "did not participate in any meaningful way. Instead I 'lurked' and watched". (Delamont, 1984, p. 27). The difficulty with 'hanging around' and observing in this way is that it can add to the problems of explaining the research, if the researcher appears to be doing nothing. Walford stated that while the role of 'observer' "is recognised within classrooms" it is not so acceptable everywhere, but only "within tightly defined circumstances". (Walford, 1987, p. 60). Faber noted, for

example, that she found "no acceptable way 'of hanging around' in the boys' lavatories". (Fuller, 1984, p. 103). This latter problem affects infant teachers less, though.

Moreover, the 'neutral observer' role is usually dropped some of the time, as noted when discussing Junker's typology of roles. A researcher becomes a participant when talking to others, whether formally or informally, whether a conflict situation exists or not.

Several research studies record that their authors utilised former teaching experience to become participants as teachers, for example, Hargreaves, 1967; Lacey, 1970; Burgess, 1983 and Walford, 1987.

Some research has also been done while remaining as a teacher in the same school, for example Pollard (1987) and teachers involved in Action Research. There are problems with this form of participation, as well as some advantages.

Woods argued that the teacher returning to do work in the same field:

"is the 'participant observer' par excellence, for the terms of his participation in the first instance were exactly the same as other participants."

(Woods, 1977, p. 17)

Except, of course, as for the pupils.

When a participant in this way, a researcher possibly:

"gets access to inside information and experiences the world in ways that may be quite close to the way other participants experience it."

(Hammersley and Atkinson, 1983, p. 97)

However, while Smetherham also argued that the strength of being a participant "anchors him [the researcher] within particular realities in the school" (Smetherham, 1978, p. 97) he also pointed out that the activity of doing research:

"separates him from the thoughts and interests of those inhabiting the observed social world."

(p. 98)

This 'separation' within a role can be "psychologically very difficult". (Gans, 1968, p 303). It can bring problems of role conflict, as Woods noted. (Woods, 1979, pp. 262-3).

Pollard, for example, noted that the teacher/researcher could obtain knowledge about some activities of pupils that as a teacher he might be expected to deal with, and the "ethical dilemma" this could create. (Pollard, 1987, p. 101).

Also Hargreaves stated that, while having a teaching role could help to foster a good relationship with staff, it could adversely affect relationships with pupils. (Hargreaves, 1967, p. 203).

It does not necessarily mean either that 'good relationships' will be possible where there are divisions between staff, or different attitudes and interests among them, such as the "academic" and "pastoral" divisions at Beachside. (Ball, 1981) or the "Newsom" group and others at Bishop McGregor. (Burgess, 1982).

Being a teacher may thus mean that other teachers may view the researcher in a particular way, in his 'teaching role'. Heads especially may see a researcher as 'teacher', as Woods noted. (Woods, 1979, p. 262).

In relation to pupils, even if suspicions are overcome, it is possible to revert to a 'teacher role' in a 'research situation' and make comments that would be appropriate in the 'teacher situation', almost without thinking.

Undertaking a teaching role may restrict research opportunities. For example, teaching and preparation demand considerable time, even if only part time. Walford gave as his reason for reducing his teaching commitments that it was taking up too much of his time.

Teaching full time in a school is even worse in this respect. This

role, which Burgess termed the "teacher as researcher role" can thus restrict access, both in terms of people and the type of research possible, because the teacher is: "in the school to teach rather than conduct research". (Burgess, 1980, p. 166). Some researchers in this case:

"have found that their teaching duties have limited their investigations to the pupils and teachers with whom they work."

(Burgess, 1980, p. 169)

There is also the problem, again ethical, of whether such research is 'covert' or open, as Burgess and Pollard, pointed out. (Burgess, 1980, pp. 166-7; Pollard, 1987, p. 104).

Also, if teaching in a school, it becomes more difficult to "become a stranger" which Smetherham argued that a researcher does when electing to do research. (Smetherham, 1978, p. 99).

A researcher may be able to stand back and adopt an objective view of the familiar world but, as Burgess pointed out, not only may some problems be overlooked through familiarity but even if this is overcome, being the 'naive stranger' becomes more of a problem. Burgess stated that:

"several teachers have encountered difficulties in asking naive questions about the aims and objectives of a school which any investigator has to ask."

(Burgess, 1980, p. 169)

This is a problem when admitting just to having had relevant teaching experience, and therefore seems even more likely to arise if teaching in the school being researched, particularly if full-time.

In this part of the section various 'roles' adopted by other researchers in schools have been noted. The remainder of this section of the chapter focuses on the present researcher's 'role' and the attempt to establish 'rapport'.

Overall the role 'taken' in this research was that of 'involved

observer', rather like that of Woods, who stated that:

"I preferred to think of myself as an involved rather than participating observer. I did not take an accepted role in the institution."

(Woods, 1979, p. 261)

Gold argued that there were problems with this method. For example, it would result in superficial observation. The observer might be more likely also to misunderstand informants, or be misunderstood by them, than when acting as a complete participant or participant observer. (Gold, 1988).

This may be true, but the researcher may not have much choice about taking a participatory role in the institution. In the present instance, 'total participation' as a teacher was not possible, despite having been a teacher, although some limited and very occasional opportunities occurred. For example at Moorland on two occasions the researcher was used as a substitute teacher, when two of the staff were away for the day, and the regular 'supply' was unavailable. On other occasions the researcher became involved in 'helping out' unofficially in the classroom at Moorland, and at Larkway.

During the initial stages of the research at Moorland, and in the pilot study schools, the 'observer' role was more akin to 'total observer', although never completely so, as this is not possible, as noted previously.

Initially, the main aim was to remain as unobtrusive as possible in order to let both teachers and pupils become used to the researcher's presence.

The usual tactics adopted in the classrooms was to wander round, or sit in various places, including the library corner and Wendy House. Both teachers and children moved around themselves, and it was considered that for the researcher also to move would make her less conspicuous. However, there was some difficulty in doing this in one classroom at Moorland since

the teacher there did a good deal of classwork. In this particular classroom it was thus more difficult not to seem too visible.

Woods argued that the researcher should give some consideration to his or her "own observability" in order to blend in with the scenery and disturb the action as little as possible. (Woods, 1986, p. 49).

King stated that he stood initially, in order to maintain social distance, and allow teachers and pupils to get over their curiosity. (King, 1978, p. 4). Later, he was able to sit down, though:

"often using the furniture or even the unoccupied
Wendy House [in] an attempt to remain unobtrusive."

(King, 1984, p. 123)

It was perhaps easier for the present researcher to 'blend in', being small and female, than it might be for a tall man, particularly in an infant classroom.

The initial 'total observer' role was only adhered to for a short time. In the pilot study schools this ranged from half an hour to slightly longer, depending on whether a second visit was possible to a particular classroom. At Moorland the first few days were spent like this, and also at Larkway.

Like King, this researcher avoided contact with the children, and the teachers, during this early stage, for similar reasons. Nor were notes made in the classroom, to help put teachers at their ease. The teachers were asked if they minded the researcher's presence, however.

Apart from the classrooms, some of the early period was spent looking round the school, and becoming familiar with the layout, and going into the staffroom.

After the initial 'total observation' period, the researcher began to become more involved, and started to talk to children and teachers. In

part this was a conscious decision, for reasons that were not connected to the obvious need to ask questions. It was felt that teachers, especially at Moorland, found it disconcerting to have an adult in the classroom who did not speak. This is usually the pattern adopted by an inspector. Also, getting involved was seen as part of the attempt to establish 'friendly relations'.

Burgess stated that:

"developing trust and establishing relationships is a crucial part of a researcher's involvement in the social scene."

(Burgess, 1984a, p. 92)

Hammersley and Atkinson stated that part of this establishment of relationships is showing that a researcher has something to offer those being researched, and is not simply 'taking'. (Hammersley and Atkinson, 1983, p. 81).

In giving an 'explanation' of the research it had been stated that the researcher was a trained infant teacher in order to remain credible, and to show common interests. As part of putting teachers at ease and reducing stress, offers of help were made if they needed it. This was also done to present the researcher as a co-operative person, in the hope of reducing any hostility to the researcher's presence in the classroom.

In the pilot study schools, because of the short time involved, such help was not really possible. At Larkway, too, because of the relatively short stay, opportunity was limited. However, in one classroom there the teacher asked the researcher to supervise a child doing a new maths activity. In another she was asked on one occasion to read the class a story. This might have been a test, because a student teacher the previous day had done this, and in the class teacher's opinion done it very badly. The researcher definitely had the impression of being put on her mettle by the invitation. At other times at Larkway the researcher had dinner at the school, and sat with the children.

During the first term at Moorland help was volunteered in such classroom activities as 'hearing reading' and putting out materials. Subsequently, actual supervision of small groups of children doing a range of activities from writing, to art, craft, to project work. It was useful for this that the researcher had access to a college library and could bring in books.

During PE children were helped to change their clothes. Help was also given with preparations for Christmas. This helped the researcher get to know the teachers.

Such help did vary between classrooms at Moorland. In part this was a consequence of the degree to which 'friendly relations' were possible.

In Mrs. Dale's class, for example, the researcher felt constrained, because this teacher gave the impression that she was not happy with the researcher's presence, although nothing was actually said along these lines. So the role here remained more the observer, although the children were talked to. It was quite difficult to talk to Mrs. Dale at anything other than a superficial level.

In Mrs. Neaves' class, the role was much closer to a participant, partly because she seemed uncomfortable with the role of 'observer', and partly because she seemed glad of help. Each time the classroom was visited the researcher was involved in some way at some point with the children, and with the teacher. There was also a good deal of such 'participation' in Mrs. Knowles' class. There was rather less in Mrs. Martin's classroom. Here there were more 'class activities', and Mrs. Martin was quieter. Also, being the deputy she did not ask for help, while both Mrs. Knowles and Mrs. Neaves did. This shows that the role a researcher 'takes' varies according to situation, and that establishing friendly relationships may

not solely depend upon the researcher. Not all the hosts will be willing to be friendly.

Becoming more of a 'participant' was also useful in getting to know pupils, and in talking to them more easily. They did not seem inhibited because the researcher sometimes acted 'like a teacher'. As King noted it is difficult to interview young children but they do talk to you and they are quite useful providers of information in the 'normal' classroom situations as well as elsewhere. Neither with teachers nor children, though, was the 'observer' role wholly abandoned.

The researcher 'helped out' in other areas than the classroom, such as in the school tuck shop. On occasions the researcher also made the tea in the staffroom.

As part of trying to develop relationships, time was also spent talking to teachers and, to some extent, children, listening to and joining in conversations where possible. In the case of the teachers 'ordinary' conversational openings were attempted to make the relationship more 'normal'. As Hammersley and Atkinson pointed out:

"It may be very threatening to hosts if one pumps them constantly about matters relating directly to research interests."

(Hammersley and Atkinson, 1983, p. 82)

They added that a researcher thus:

"... often has to try and find some neutral ground with participants, where mundane small talk can take place."

(p. 82)

Thus, the researcher sometimes spoke of her own general interests such as gardening or cycling, or tried to add comments to topics put forward by others. The basic intention was to establish the researcher as "a fellow human being". (Woods, 1986, p. 60).

This 'social intercourse' was more difficult with some teachers than others. This was particularly the case at Moorland, as teachers there

tended either to talk about school matters, or homes and children of their own. While the former talk provided useful 'verbal cues' for later questions, the latter meant that it was difficult to establish a common ground on which to 'chat', although some people were more forthcoming than others. Perhaps teachers are aware that researchers may not really be 'off duty', and that, as Denton stated, about anthropologists: "rarely just chat. There is always in each chat information to be recorded". (Denton, 1970, p. 104).

The issue of such 'conversations', and the use of ordinary talk as a preliminary to 'interviews' is discussed in the following section. It is noted here, however, that these situations involve participation as well as observation.

The fact of having had teaching experience, and using this as a possible means of establishing 'rapport', meant that it was very difficult to act as a 'naive stranger'. Teachers, again more particularly at Moorland, seemed to find it rather odd that the researcher asked about 'what was going on'. They seemed to consider that, having been a teacher, and having 'seen' their classrooms, it was obvious. Mrs. Martin actually said on one occasion with reference to a particular question put by the researcher on classroom routine: "Well, you know what it's like, you've just seen it." (Notes, Moorland), which was not quite the response hoped for. It was the kind of invitation to "draw on background knowledge" noted by Hammersley and Atkinson. (Hammersley and Atkinson, 1983, p. 120).

Although this part of the section has indicated that the role could mainly be described as 'involved observer', but that some participation occurred, it is important to point out that there was no one role.

Within the overall position, the actual role at any one time depended on the particular interest. It was necessary, even after becoming in a small degree a participant in parts of classroom life, to resort

sometimes to the more 'observer' aspect. Participation, also, did not mean that the 'observer' eye (or ear) was missing, particularly in 'conversations' or when listening to staffroom talk, or pupil talk.

The role also depended on other people. It was noted, for example, that Mrs. Dale seemed unwilling to allow a more participatory role in her classroom.

This discussion of the researcher's role and of the section, is now concluded.

Overall, the section has been concerned with the establishment of 'field relations' and has discussed the gaining of initial access, and the various aspects of 'impression management' necessary for this, including the 'presentation of self' and an 'explanation' of the research to the hosts. The problems associated with these were pointed out, with reference to the researcher's own 'presentation' and 'explanation'.

The second part of the section attempted first to indicate the range of roles possible under the heading of 'participant observation', although pointing out that these are not necessarily easily distinguishable. It was also pointed out that the 'choice' of a role might be affected by those researched and also that in practice no one role was possible.

The roles played by other researchers in schools were noted, and the problems of combining teaching and research discussed. It was pointed out that, whatever the actual role at any one time, a researcher has to remain in one sense 'marginal', to retain the identity of a researcher. This necessity can create both psychological stress for the researcher, and also some ethical problems.

The section also pointed out the importance of establishing friendly relations. It was indicated not only that there might be difficulties at a personal level in doing this with all hosts, but also that 'friend-

ship' can be a barrier to research, if there were divisions between those being researched, and the researcher becoming associated in the minds of others with one group, or becoming too involved with them.

The efforts of the researcher to establish 'friendly relations' were noted and the difficulties encountered stated. In particular, it was noted that 'relationships' depend upon some degree of reciprocity. A researcher cannot establish rapport on a one sided basis, and not all hosts are willing to be friendly even though attempts are made. This attitude on the part of the host may reflect their personal difficulties, or the competence or its lack, of the researcher, or may reflect difficulties in the research setting, or any combination of these. It is not a simple process nor, like gaining access, one for which there are infallible sets of guidelines. But it is an extremely important process, since it affects the maintenance of access, both to the research site as a whole and to individual settings within it, and thus to the collection of data which is representative.

The next section of the chapter discusses the methods of data collection in the light of the problems noted in this.

SECTION SIX : METHODS OF DATA COLLECTION AND RECORDING

This section describes the main methods of data collection used during the study. These were: observations; interviews of various kinds, including conversations unstructured and semi-structured interviews; and the use of various documents, including school record books, class records, notes for teachers and the use of a questionnaire and historical documents.

It is noted here that these methods were not used as part of a linear progression which moved from observation to interview and then the use of documents. As Burgess pointed out, observation, talking to people and using

documents are used continually. (Burgess, 1984, a.). This is almost a circular process, going constantly from one to the other. In this section, however, each method is dealt with separately as far as possible, in the interest of clarity.

First, though, a note on the importance of the context in which methods were used, as this can have an effect on the data collected. Context here means the 'situation'. This includes the manner of use, such as open or hidden, whether formal or informal, the people, the time, whether morning or afternoon or over a period, the place, such as the classroom, the staffroom, or elsewhere. These features are associated with the issue of 'sampling', and therefore the question of representativeness. 'Contextualising' the research in this way is referred to at various points in this section.

Observations are considered first, in terms of how these were made and their purpose, followed by when and where these took place.

'Interviews' are then discussed, in terms of their nature as used in the research, the kind of question asked in different types, and the time and place in which they were asked, and their purpose.

The use of documents is then discussed. The types used are noted. These included for the school-based fieldwork items like record books and teachers' notes, and a questionnaire. The purpose of producing the latter is stated, and also how it was designed and implemented. Besides these items, others which related to the school area were used, such as newspaper articles and a local survey. Apart from these documents, all related in some way to the school, this part of the section also notes the use of historical sources for the two history chapters, which were mainly, though not wholly, secondary.

The recording of data and the main means used for this in the research are then discussed.

As stated, the methods of data collection were used continuously, not consecutively. This was done in order to study the same data from different directions, as well as gaining different kinds of information. 'Triangulation', the attempt to verify information either by asking different people for their version of the same event, or by using different methods, is therefore discussed, and the general questions of validity and reliability

1. Observation

This part of the section discusses observation as a method of data collection. It does not present details of the content of the observations, however, since these form the substance of the chapters together with material from interviews and documents.

Observation is perhaps the most important method of data collection, for it is the first source of data. From observations questions are generated, and so interviews. Observation is also a means of checking the responses to questions, or comparing statements to practice. Observation is also part of analysis, since in the act of observing, the human brain is almost automatically classifying, in order to make 'sense' of what is seen and heard. However, observation can also be the source of bias, again, simply because the researcher is human, and part of the situation. Thus, something which could be significant may be missed altogether, or misunderstood and so wrongly evaluated.

Observation is also difficult, because a researcher observes something, so the questions is always 'what'? and then other questions such as 'who', 'when' and 'how'. Or, in other words, observation involves sampling, or choosing between various people or activities or places. But before a narrower choice can be made, the researcher needs a general picture, otherwise any 'choice' is difficult.

At the beginning of the research, before actually going in to schools

in the pilot study, Schatzman and Strauss (1973) was used to gain some idea of what 'observation' meant, and some idea of what to do. This was in preference to Glaser and Strauss (1967) which at this stage was found rather confusing. (In passing, the researcher was in good company here, [Hammersley, 1984, p. 43. and King, 1978, p. 7]).

Glaser and Strauss (1967) argued that data should be used to generate theory. They argued that the process of data collection, after the initial stages, should be "controlled by the emerging theory". The researcher might begin:

"with a partial framework of "local" concepts, designating a few principal or gross features of the structure and processes in the situations that he will study."

That is, it is known, for example, what people and procedures are likely to be found, but does not know the relevance of these "concepts to his problem". The 'problem' has to "emerge".

They termed "theoretical sampling" that process:

"whereby the analyst jointly collects, codes and analyses his data, and decides what data to collect next, and where to find them, in order to develop his theory as it emerges."

(Glaser and Strauss, 1967, p. 45)

At the time, this did not seem particularly helpful for the researcher's problem of what observation entailed, and how to go about it. Schatzman and Strauss were much clearer.

Schatzman and Strauss stated that after gaining entry, a researcher needed first:

"a working conception of the relevant dimensions of the site, including its outer boundaries and inner locales; also the classes of things, persons and events which inhibit these locales."

(Schatman and Strauss, 1973, p. 34)

They said that to get this 'conception' a set of "maps" was needed. These were "social, spatial and temporal". (p. 34). This "mapping operation" was:

"a tour of limited discovery - a first reliable and extensive (not intensive) look at the things, persons and activities that constitute the site."

(p. 35)

Following this 'mapping', the researcher then "can narrow his focus". (p. 44).

In the pilot study schools this was the main guide. However, before starting at Moorland, and later at Larkway, Spradley (1980) was available and was also used as a guide.

Spradley distinguished between three types of observations. These were, first, "descriptive observation", where the researcher tried to "catch everything that goes on". Secondly, there were "focused observations", more narrow in scope, where the researcher was "looking for differences among specific cultural categories". (Spradley, 1980, p. 128).

The researcher considered that 'descriptive' was misleading if denoting one stage in observation, since all observation is descriptive in a sense, it is just the focus which changes, from wide to narrow. This stage of Spradley's was therefore taken to mean wide and general, and to be akin to Schatzman and Strauss' "mapping operation".

Spradley also distinguished between "surface" and "in-depth investigation". The former meant "identifying and partially studying as many cultural domains as possible", while the latter involved neglecting "many important features of the cultural scene" and focusing on specific problems or areas. (p. 101).

Spradley stated that the 'surface investigation' had disadvantages since:

"... cultural meaning is complex and if we only skim the surface we will never know how informants really understand things."

(p. 101)

He argued that in practice 'ethnographies' comprise "... and study a few selected areas whilst maintaining surface understandings of the scene as

a whole". (Spradley, 1980, p. 101). However, in a sense, sociologists working within an 'ethnographic' approach are not going to know 'how informants really understand things', because they do not, as noted in the second section of this chapter, immerse themselves in the 'total culture' of the people involved. In a State day school teachers and pupils, and the researcher, go home at the end of the day. So in one way any study, focused or not, is only 'skimming the surface'. We do try to do something more, however.

As noted previously, Schatzman and Strauss and Spradley were the main guides used for observation during the 'fieldwork' stage of the research. Since then, however, more work has been available. Most make the same distinctions between a wide or general first stage, followed by 'focusing', so hopefully the researcher did not stray too far from reasonable practice.

For example, Hammersley, in an Open University text published in 1979 though not available to the researcher until 1982, stated that in the initial stages of fieldwork the "ethnographer" uses:

"a discovery based approach" [and] "adopts a relatively wide focus ... since he is guided by very general theoretical presuppositions and foreshadowed problems."

(p. 137)

The researcher at this point is "attending to many different aspects of the scene". (p. 137). Hammersley stated that the research becomes "progressively focused" and as it does so:

"the fore-shadowed problems are developed, transformed, refined and specified ... demanding greater selectivity in observation"

(Hammersley, 1979, p. 138)

Delamont and Hamilton (1986) stated that the ethnographer starts with:

"a wide angle of vision" [then] "... zooms in and progressively focuses on those classroom features he considers to be most salient."

(Delamont and Hamilton, 1986, p. 36)

The problem with all attempts to say what the process of observation is, is that they seem to relate to an 'ideal situation'. In practice, the procedure can be more confused. Hammersley himself, in a later account, stated that his own research "deviated from the model proposed by Glaser and Strauss", particularly in relation to their idea of 'theoretical sampling'. Hammersley said that ideas about analysis "did not significantly affect my data collection" and described his approach as more akin to:

"'dredging'; choosing a site and collecting all the data available in it relevant to particular foreshadowed problems."

He stated that one reason for this was:

"a naive view that ethnographic research should involve the tapping of 'natural' events, of 'what's going on' in the school."

(Hammersley, 1984, p. 56)

He also added that his 'dredging process' was, in his view, "more common" and "grounded theorising" less so "than is usually admitted". (p. 57).

To some extent the researcher would agree in relation to the present research. There are times when, in the state of anxiety which seems an inevitable accompaniment of 'interpretive' research, a researcher is so concerned to 'get some data', that the significance of what is seen or heard does not emerge for some time, perhaps even not until the final analysis stage.

However, in commencing observation, the first aim was to get a 'general picture', and so a 'mapping operation' or 'descriptive observation' was the first stage in all the schools. Although Glaser and Strauss was not seen as particularly helpful at the start of the fieldwork, one piece of advice was taken. In the early stages of the research, especially at Moorland, an attempt was made to 'bracket off' those 'foreshadowed problems' obtained from reading Sharp and Green (1975) and King (1978), and previous experience (and at Moorland, from observation in the pilot study schools). Glaser and Strauss (1967) stated that:

"An effective strategy is, at first, literally to ignore the literature of theory and fact in the area of study, in order to ensure that the emergence of categories will not be contaminated by concepts more suited to different areas."

(Glaser and Strauss, 1967, p. 37)

Thus, the attempt was made to become a 'stranger', although this presented some difficulties, since, as noted in the previous section, the 'hosts' knew that the researcher had been a teacher.

After the 'mapping' or 'descriptive' stage, efforts were made to 'focus', as ideas and 'patterns' emerged.

There were, however, differences between the observational process in the pilot study schools, Moorland and Larkway.

Visits to the former ranged from one day in the case of Ashley, to a few days to a week or a little more. Therefore during this period little beyond 'mapping' could be attempted. This stage was speeded up so that some 'focused observation' could be attempted. For example 'contrasts' between schools was looked for. The 'daily routine' was also focused on.

At Moorland, however, all the stages were attempted, while at Larkway there was less attention to 'mapping' and more on 'focused observation'.

Although observations can be divided into stages, they are not necessarily totally separate. For example, the observations made in the research were at a number of levels, but even at the first, or general, some focusing occurred necessarily. Since seven schools in all were looked at, although for very different periods of time, the 'mapping' stage caused 'focusing' in that some 'contrasts' between schools were obvious, as well as similarities. Differences in the type of building, its size, the internal layout and materials provided, the numbers of staff and pupils, and organisation, and the catchment area were recorded. These particular 'mapping' observations are reported in "Setting the Scene", the chapter which follows.

This making of comparisons, also occurs when 'mapping' the classrooms within a school, as well as between those in different schools.

However, as a general statement, the observations made could be classified in these three stages. Thus, it was attempted first to become familiar with the spatial, social and 'temporal' aspects of the school as a whole, then of all, or as many as possible, of the individual classrooms. Attention was then concentrated on those aspects within each classroom, at the general or 'descriptive' level. At the second focusing level, differences between classrooms were noted. At this level also, broad 'categories' like 'teacher talk' and 'work' and 'play' activities were recorded. At the third, 'specific' level the different items within these categories were collected, that is, 'sub-categories'.

The first stage of general observation could be described as a form of reconnaissance.

It was stated in the previous section that an initial interview was held with the head teachers of each school. During this, the researcher was seeking information about some general details of the school, such as its age, the number of staff, and pupils, as well as looking at the design of the building. At Ashley, this was all that was possible. During the first visit to the other schools, following this, the researcher looked round the school and drew a general ground plan, to learn where everything was. This was done for the classrooms, as they were seen. These plans showed the physical layout, and the location of furniture, and detailed the various items of equipment. The number of pupils was noted. An attempt was also made to get an 'overview' of the catchment area.

To some extent a 'comparative method' was used, in that an attempt was made to compare schools, and, within schools, classrooms, noticing similarities and differences, although this was not done to arrive "at a typical ... teacher and classroom", (King, 1984, p. 127) or 'typical school'.

Apart from noting the layout and materials in the early visits to classrooms the researcher was trying to get a general picture of the social life - that is, what the teacher and pupils were doing. An example from Briarfield shows the type of observation, though only a small section. About the children's activities, the notes recorded that:

"On one table two children are colouring squares ...
They each have a rectangular card on which is written
'Colour 20 squares. Use different colours. Count how
many of each colour'."

On the teacher's activities it was recorded that:

"Mrs. Tatler calls out 'Can I have the pink card group'.
Three or four children go and sit on a mat. Mrs. Tatler
takes a set of pink cards with words on them. She holds
up a card, points to it and asks the children 'What is
it?'."

Staffrooms were visited, and such features as size and furniture were noted. The researcher was both listening for any 'verbal cues' and so in a sense engaging in covert observation, although not actually hiding, and also attempting to join in general conversations, as part of establishing 'friendly relations'.

At the beginning of this section the importance of the context of observations was pointed out. It was also stated that 'contextualisation' was connected to the idea of sampling. This involves the choice of who, what, when and how to observe.

In terms of sampling, these early observations were an attempt to see 'everything' and so gain an overall picture. Of course, this is not actually possible, since a researcher can only be in one place at any one time. Also, everything that happens in a research site has some 'history'. They are "situated in time" a Delamont stated. (Delamont, 1976, p. 27). This idea is referred to again in the next chapter. The point is that some activities involving people cannot be 'understood' except by reference to their 'historical context', and an observer takes time to 'see' this.

In spite of problems, the idea of this first stage was to try and see all the people, and register all the activities, in both formal and informal situations, at various times, listening to talk and observing the use of space and equipment.

In relation to the pilot study schools, apart from Ashley, there was, however, a general problem, and one specific one.

The general problem was that, through having to work in the mornings, the majority of the visits to these had to be made in the afternoons, with only a rare morning free.

The specific problem arose in one school, Rushside. Here, there would have been a problem of time in any case, since there were nine classes. The problem was, though, that even if there had been time, not all the teachers were willing. This was because of the immediate 'history' of the effect of the previous head, something noted in a later chapter. Although the researcher was shown round the school and thus did see inside all the classrooms, three staff did not wish to be seen further. So only six classrooms were really visited, and one of these for only a short time. Because of the organisation of the school, the pattern of activities would appear to have been similar for the other three, however.

In the other pilot study schools and Moorland and Larkway, all classrooms were seen in this 'mapping' stage.

To overcome the 'afternoons only' problem in part involved the asking of questions, in order to find out if the pattern of activities varied from that of the morning. However, in one school, Briarfield, the problem solved itself, even through the actual observation gave rise to a question.

It was noted in the first interview with the Head that two of the three classrooms had children of mixed ages from 5½ to 7+, while the other classroom had one age group of 4½ to 5 year olds. This was the 'reception' class.

The observation notes recorded that in two of the classrooms children were engaged in many different activities, such as 'reading' or 'writing' or 'number', 'art and craft' or using 'games', during the afternoon period.

In the 'reception class', on the other hand, it was noted that:

"This class is organised on a different basis to other classes in the school because it only contains one age range, and the pattern of the day seems different. I observed that the children were only doing 'writing' and 'number' activities."

(Observation Notes, 'Briarfield')

These observations generated the questions in relation to the first two teachers. "Do you follow this pattern all day or just in the afternoons?" and to the third "Why are the children just doing these things?" These questions received basically the answer 'Yes' to the first, and to the second, "Well, it takes the boys longer to settle down in the morning" This last point is picked up in the next part of the section. The point is that attempts were made to overcome the 'afternoons only' visiting, and get a picture of events that would be 'representative'.

As noted, the intention behind 'mapping' was to see everything. Whether it is possible actually to do this or not, the attempt is exhausting, as well as producing problems for recording, for, as Schatzman and Strauss stated:

"After a bare few minutes ... the researcher is "threatened" with a crush of observations and interpretations."

(p. 95)

Both in the brief period in the pilot study schools, and in the first few weeks at Moorland, it became clear that trying to observe for the whole period, either all an afternoon or the whole day, which included the dinner hour and play times, was an impossible task, and also not particularly fruitful. There was a tendency to compress observations and so lose important details. This was especially the case with teacher talk. For example, it was recorded:

"... the teacher says she wants two groups 'horses' or 'sheep' to bring out their work books to two tables ... Mrs. --- explains what the two groups have to do."

(Observation Notes, Moorland)

This recording missed out what she actually said and, as Hammersley and Atkinson pointed out, "The actual words people use can be of considerable analytic importance". (Hammersley and Atkinson, 1983, p. 1530. Therefore it was decided to adopt a form of 'time sampling'. In the case of some of the pilot study schools, even with the limited time available, attention was paid to observing what happened during different parts of the afternoon. Thus particular periods were focused on at different times, such as the start of the afternoon, or 'clearing up'. This was particularly the case with schools such as Briarfield or Fairfield which were visited more than once. Thus at Briarfield the first visit to one particular classroom occurred during the middle of the afternoon when all the children were already engaged in various activities. At a subsequent visit, however, the 'start of the afternoon' procedure was observed. It was seen to involve a short 'discussion' by the teacher about what the children would do.

At Moorland, and to some degree at Larkway, attention was focused in this way on mornings or afternoons, and then time periods within them, so that the activities of the teachers and pupils, and the interaction between these people, could be observed more specifically. Or particular 'events', such as 'assembly' or staff meetings, were the subject of particular observation.

In the pilot study schools, after a short 'mapping' stage which in some cases was not much more than one hour, attention was focused on the daily routine, the nature of activities, such as 'reading', and the content of these. Observations were also made of how the teacher talked to the children, about what, and when, and the children's talk to the teacher and each other, and the general interactions within the classroom.

As stated in the first section of this chapter, one of the concerns of this research was to discover if there was a distinctive approach in infant schools. The result of observation alone in the pilot study schools was that there appeared to be differences, both between schools and sometimes within schools, in the daily routine.

At Stone Street, mostly Art and Craft activities took place in the afternoon, although some children in the top infants class were heard being told to finish some 'number' work started in the morning.

Again, in the reception class, although drawing and painting was the main activity, some children were observed to be writing on stylised sheets with pictures printed on them. The afternoon ended with 'story time' when the teacher read to the class as a whole.

At Rushside, in four of the six classrooms visited, art and craft were the only afternoon activities, apart from 'Story Time' at the end of the session. In two classes there were differences. In Class One, for 6+ to 7+ children, apart from Art and Craft, there were 'number' games, to which the teacher directed certain children, and 'topic work', and other 'number work' such as adding 10's. This was the deputy head's class.

In Class Five, which contained children from $4\frac{1}{2}$ to 6, although some children were observed doing Art and Craft, others were engaged in 'number', or 'reading', or 'writing' tasks.

At Briarfield, in two of the classes, writing, 'mathematics' and art and craft activities were all going on at the same time. Children appeared to have some choice as to which. In the reception class only, the afternoon was taken up with '3R's' work.

At Fairfield, in all the classrooms, all activities seemed to be going on at the same time. Children here also seemed to exercise choice as to which they did.

Having observed what seemed to be the general pattern of activities in these schools, there were questions to be asked in each, in order to check that the pattern was as noted, and usual, and why, if this was the case, the teachers did organise in this manner. That is, there was a concern with what their 'views' were about children and pedagogy. Questions also arose as to the influence, if any, of the Head.

Research began at Moorland in the third week of the Autumn term following the Pilot Study. 'Mapping' here was, of course, for a longer period. The intention was to observe in all four classrooms, but not the attached nursery apart from an initial visit, since the research was about 'infant schools'. This last decision was later reversed.

Towards the end of October, a basic picture had been built up of the main features of the school. Apart from the classrooms, the hall activities such as assembly and wet day PE lessons were observed. It was noted how teachers and children came into and went out of this area. The staffroom was observed, although no notes were taken in there, nor in the hall. What was listened for in the staffroom was who used it and for what and the type of conversation. The playground was also observed. It was noted what types of activities children entered into, and how 'play' contrasted with 'classroom play'.

The head and all the teachers had been talked to, at least informally.

The general routine of classroom life had been noted down, but in two of the classrooms it was still not exactly clear what the pattern was. These were the classrooms of Mrs. Neaves and Mrs. Knowles. In these cases more general observation carried on into the second term.

At the beginning of each term such 'descriptive observations' were made again in all the classrooms, however, to check if any changes had occurred in the routine.

Schatzman and Strauss stated that by the end of the first stage of observation the researcher, having assembled the "data pertinent to his maps" could then start to analyse. He could begin:

"to co-ordinate some facts and inferences, and develop some urgent propositions along with plans for checking them out. Propositional statements, by patterns or hunches about processes ... can be linked to each other and to the starting framework of concepts about organisation, communication, control, socialisation"

(Schatzman and Strauss, 1973, p. 38)

These authors did add, though, that understanding might still be "shaky" and "lack validation though not plausibility". (p. 38).

In the researcher's experience, 'understanding' at this stage was indeed 'shaky'. Though ready to start more selective focusing, the researcher was not prepared to 'develop propositions', even if some ideas were present.

During the first few weeks, apart from seeing, the researcher had picked up certain "verbal indications". For example, both teachers and children had been heard to refer to "work", "play" and "choosing time". For example, "Clear away now. It's time to do some work", (Class 2) and "It's writing practice, not choosing time". (Class 3). It was decided to focus on these emerging categories, and record what activities came within them. 'Work' was found, for instance, to include 'writing'. It was then observed what came under this heading. As the notes stated:

"Further observations show that the term 'writing activity' is too general and does not adequately describe the sorts of activities which come under this category. It includes writing a story, phonic work and copywriting. All come under the single category 'writing'."

(Observation Notes, Moorland)

Thus, time was spent observing children doing 'writing', the content of activities this included, and how 'writing activity' was organised. The same kind of observation was carried out for other 'work' activities such as 'number', 'oral work' and the category 'play', as well as specific areas

such as PE and Art and Craft. Thus 'sub-categories' were established for all these, and for 'choosing time'. This last category seemed unique to Moorland, since it had not been heard in the pilot study schools.

Children did choose in some of these, such as Briarfield and Fairfield, but there 'choice' had not been a 'timetabled' activity. Children were not told specifically 'Now it's choosing time' or 'It's not' but this direction was heard at Moorland.

Apart from the 'verbal cues', general observation of activities showed that some activities were done as a 'class', others in 'group', and others individually.

This led to focusing on which activities were organised in these ways, when, and how teachers directed activities, as well as how children interacted with teachers and each other.

Apart from classroom activities, there was another area on which it was decided to focus. In the first week teachers had been heard talking about "these children", and comments had also been made to the researcher. They had also mentioned "poor home background" and "families in this area". This had not been heard in the other schools, although references had been made to individual children in relation to particular family circumstances. Thus this area was not an initial concern, but one which arose simply because it seemed part of the Moorland teachers' views or 'perspectives' about the children at the school.

Although observations continued in all four classrooms at Moorland there were differences in the amount, as noted in the previous section.

One teacher (Mrs. Dale in Class 1) seemed unhappy at the researcher's presence in her classroom, although nothing was ever said directly to the researcher. As a consequence of this 'feeling' less time was spent in this classroom, although observations were not abandoned, nor were attempts

to discuss these with the teacher. These were hampered, however, during the year, by the presence of another student in the school, who based herself mainly in Mrs. Dale's classroom.

In a second classroom the teacher (Mrs. Neaves) while not objecting to the researcher's presence, seemed inhibited by being just observed. Therefore, as stated in the previous section, the time was usually spent both sitting with the children and also helping the teacher, since she asked for help. This time was spent making notes in the classroom. This classroom was important because it contrasted with the other classrooms in terms of children choosing what activities to do and when to do them.

As will be related in Chapter Six, a new pattern was developed in this class following Christmas preparation, and continued in the next term. Children gained more 'choice' over when to do things than in the other classrooms.

The majority of observations at Moorland, after the initial period, were made in Class 3, which was the deputy head's class; Class 4, which was Mrs. Neaves, and Class 2, Mrs. Knowles. In this last, it was easier to record observations at the time.

During the summer term, it was decided to pay more attention to the nursery. This was in spite of the earlier decision to leave it alone after one visit. There were two reasons for this. First, the head of Moorland, Mrs. Warner, said that she regarded the nursery as part of the school, not as a separate unit.

Secondly, Schatzman and Strauss (1973) argued that a researcher should make use of initial sensibilities. (p. 53). In the case of Moorland nursery, the first visit had left the impression that the routine there was very different from that in the infant school itself. On reading through the observation notes made in the three previous terms,

it was decided to concentrate observation in the Autumn term on the nursery and the reception/middle infants class in the school proper (Class 2) and, when possible, in Class 1 (Mrs. Dale) which also contained reception children, although some were still carried on in Classes 3 and 4. The intention of this last period of observation at Moorland was to focus on the similarities and differences between the nursery and the classes in the school proper which contained reception children. Therefore attention was focused again on the amount of teacher direction, the timing of activities, and the content of these, as well as teacher-pupil and pupil-pupil interaction. This period of research ended a few weeks before Christmas.

Observations indicated that in Moorland infant school children's activities in the classroom were more directly controlled by the teachers than appeared to be the case at Briarfield and Fairfield. Also, like Stone Street to some extent, and most of the Rushside classrooms seen, activities at Moorland were divided between morning and afternoon. The 3R's were mostly done in the morning, and Art and Craft in the afternoon. In the nursery, however, because there were two separate groups, one in the morning and one in the afternoon, all activities went on in each period, and, apart from 'snack time', children were not directed. The nursery seemed to approximate more to the ideal of a 'Progressive child-centred' pattern than other classrooms.

Thus, as with the Pilot study schools, the observations generated various questions, both as to the detail of activities, and to teachers' reasons for the organisational pattern. There were also questions raised about their views of the children and the area, and what actually influenced these.

From both the pilot study schools and from Moorland, certain ideas had been developed. The visit to Larkway was in part organised as a check on some of these. Because of the earlier research, observation at Larkway was

more focused from the beginning. However, a short period of 'mapping' took place. During the first visit of the main research period here all the classrooms were seen as part of looking around the school to get a general impression. During subsequent visits observations were carried out in two reception classes and one top infants' classroom, thus particular age groups were concentrated on. The reasons for this were stated to the deputy when negotiating access. The researcher stated:

"I am interested in the beginning stage and in the end period in the infant school, in the former because that is when I feel that children get used to the idea of school. I am interested in this, and in what skills teachers think the children bring from home, and how the home prepares them for school, and also how reception teachers see their task.

.. The top infants are near the junior stage. I wish to find out whether this has any influence on how top infant teachers see their task."

(Observation Notes)

The observation at Larkway was different from that at Moorland in other respects. The observations were guided by a schedule, designed to indicate areas of interest. The issues were clearer, having observed at Moorland. Also, a questionnaire was given at the beginning of the period. It was used thus in part to base observation on what the teachers' responses to this were.

Observation focused on classroom organisation. This included whether children were taught as a class, in groups, or as individuals or if there were times when all these occurred, and if so, when. Thus, particular periods of the day were looked at specially, such as the beginning of sessions. A main interest was on the degree of choice children exercised in moving from one activity to another, and the amount of direct teacher control. There was an interest in what 'classroom rules' there were, and how explicit these were. The role of the Had was also important.

During the first visit to the school it was observed that at Larkway

as at Briarfield and Fairfield, children appeared to choose from the range of activities provided. These activities seemed available throughout the day.

Later observation showed how this choice occurred. The discovery was similar to that noted by Berlak and Berlak. They began observation in one classroom on a Wednesday, and found that the children in that class appeared to choose without direction and to control their own learning. However, they discovered on the following Monday that the teacher "set work minimums in each subject of the week". (Berlak and Berlak, 1975, p. 288).

Likewise, when 'focusing' on the beginning of sessions at Larkway, it was observed in the class being visited that 'Discussion Time' took place first thing in the morning and first thing in the afternoon, when the children were brought together. Although this time was used to discuss a range of things, it was also used, especially in the mornings, to tell children the 'work' which they should cover during the day. This class was a 'Top Infants', and further observation in other classes like this, showed a similar pattern.

However, further observations in the school, this time in reception classes, indicated that a rather different pattern existed in these. In the top infants classes, although a group might be called on by the teacher to start work with her, the rest were free to carry out the tasks in any order they chose. In the reception classes, however, the groups were told which activities they had to do, and a check was kept through the day to see what was being done. Choice in the reception class was restricted to an activity called 'Choosing', although, unlike Moorland, this was an activity on offer all day. An individual or group was told to "go and do 'choosing'". This 'choosing' in the reception classes meant activities like playing with toys, the Wendy House, a rocking horse, a 'rocker' (an item like a curved see-saw) or doll's house, as well as sand or water play.

By the top infants' classes, 'choosing' referred to all activities, thus, from a single area, observation showed an extension of 'choice'. But in these classes, the activities on offer included fewer toys than in the reception classes. Certain items such as the Wendy House and bricks were only twice seen in the context of 'top infants' activities. However, the range of activities seemed wider, with topic work seen which involved a number of 'basic skills', in discovering information or making models.

Thus, observation in classrooms, as extended, showed differences and helped in the focusing, when talking to teachers, on the reasons for these.

Apart from observation in classrooms, assembly in the hall was attended. It was noted initially that it seemed to be conducted in a different way to Moorland. Observation then focused on how the head teacher acted, that is, her general manner and the way in which she spoke to the children, and how the children behaved, as well as the general assembly routine.

It was only possible to see one staff meeting, but during this the researcher noted how it was conducted, and observed in particular the part played by the deputy head. It was noted that she appeared to act as a mediator between the head and the staff.

In the initial interview the deputy had made the researcher aware of some of the responsibilities attached to her position, and something of these was evident during the visit. However, the role of the deputy head in an infant school was not explored in detail as it was with the heads, partly through lack of time in the pilot study schools, and at Moorland for different reasons.

The comments on Larkway observations are the last.

This part of the section has concentrated on how observations were carried out, from general to more focused, and on some of the aspects of

infant schools which came to the attention of the researcher as a result.

It was noted that from observations a number of questions were generated. These became the subject of interviews of various kinds. These interviews are discussed in the next part of the section.

2. Interviews

After observation, interviews are the most vital method of data collection, since it is from interviews of various kinds that members' accounts are produced, and the researcher's observations and accounts are checked. There is a close relationship between observations and interviews. For example, Agar stated that simple observations may spark off questions which might not have occurred otherwise in discussion. (Agar, 1980, p. 108).

Burgess similarly noted that it is necessary to observe "before a detailed conversation can occur". (Burgess, 1984a, p. 103). One is, in a sense, asking questions during observation, as Spradley pointed out. He stated that:

"Ethnographic field work begins when you start asking questions. That seems evident enough when conducting interviews, but even the simplest observations and field note entries involve asking questions."

(Spradley, 1980, p. 31)

Such observations involve "implicit questions". (p. 31).

An example of such 'implicit' questions, those asked and answered in the actual writing down of observations, is taken from the researcher's field notes. It was recorded that:

"At one-thirty children came in in ones and twos. Some go on the climbing frame. Some go to and start to play with toys laid out on the table. Some children put on aprons, helped by the nursery teacher and another adult, whom the nursery teacher had previously identified as a nursery assistant. The assistant put pieces of paper on two easels. At 2.30 pm the nursery teacher tells the children to put all the toys away and sit on the floor."

(Observation Notes, Visit to Moorland Nursery, 1980)

The 'implicit' questions behind this observation were: When do children come in and how? What are the materials? What are the children and adults actually doing? What is the routine?

At some stage of the research these 'implicit' questions had to be made explicit, in order to obtain either confirmation of observations, or a different interpretation of these, as actors in the situation stated what they thought was going on, and gave their explanation of events. Their 'explanations' were important for, as Hammersley and Atkinson, for example, pointed out:

"It is a distinctive feature of social research that the 'objects' it studies are in fact 'subjects' and themselves produce accounts of their world."

(Hammersley and Atkinson, 1983, p. 105)

These authors also stated that not all information is obtainable by first-hand observation. This was the case in the present research, when unsolicited information came from teachers in the course of a 'conversation' on another topic, or when documents were given 'out of the blue', leading to other observations and for interviews.

Thus, interviews at some stage became necessary, not just to provide a check for observations but also as a complement, to extend 'understanding' by eliciting new information. This was then used as a starting point for further extension or development either by the same or a different method of data collection. These, as noted previously, were not, and cannot be, used in isolation. However, interviews are more 'personal' and open to subjective interpretations of each other's intentions than observations, and the extent to which 'new' information can be extracted from the situation, or 'old' extended, depends very much on the nature of the relationship between researcher and researched. Some information can be given, however, unwittingly, in the sense that a statement made in an interview may show up a discrepancy between that and something observed.

'Interviews' can be, and are, classified in various ways by different writers. Some of the distinctions made overlap, and some are confusing.

At the start and during the fieldwork stage of the research, the researcher used Lofland (1971), Schatzman and Strauss (1973) and Spradley (1980) and Patton (1980) for ideas about interviews. The statements of these authors are discussed, in order to make the researcher's position clear.

Lofland defined the term 'interview', stressing the syllables separately, as "the act of perceiving as conducted between two separate points ... between two separate people". This seemed an important comment, since it stressed the fact that people are involved in interviews, people who can have different viewpoints and purposes. So an 'interview' cannot be taken for granted.

Lofland distinguished between "structured" and "unstructured interviews". The former involved a relationship between 'interviewer' and 'interviewee', whereby the latter had to choose "between rigidly formulated answers attached to rigidly formulated questions" in a "schedule" or "questionnaire". (Lofland, 1971, p. 75). This type of 'structured interview' was associated with quantitative research, as noted in the second section of this chapter.

The alternative, "unstructured interview", was considered by Lofland to mean "'intensive interviewing with an interview guide'". The aim was to:

"elicit from the interviewee what he considers to be important questions relative to a given topic ... to carry on a guided conversation and to elicit rich detailed materials"

(p. 76)

The examples that he cited still seemed quite 'structured' in the sense that there were lists of 'topics' to be covered, and these also seemed quite wide in scope.

Schatzman and Strauss distinguished between "situational conversation" and "the lengthier interview". The distinction lay in the place and time and the amount of detail. The former related to questions asked "on-the-spot" about things "seen and heard" as well as about the people themselves. These questions were those which occur 'naturally'. (p. 71). This type of 'interview' was of shorter duration than the second kind.

The 'lengthier' interviews, in contrast, took place usually "in less public settings". (p. 72).

Schatzman and Strauss' main point was that an 'interview' was in fact "a lengthy conversation", (p. 72), in that interviews were not formal, for:

"Whatever it may be that the researcher is intent on getting ... the researcher's mode approximates conversation."

(p. 72)

Thus, a researcher was seen as "flexible", and as someone who:

"does not use a specific, ordered list of questions, He may have such a list in mind or actually to hand, but he is sufficiently flexible to order it in any way that seems natural to the respondent and to the interview situation."

(p. 73)

Calling an 'interview' a 'conversation', implied the idea of an interview as a "shared event". (Schatzman and Strauss, 1978, pp. 71-73).

This seemed an appropriate view of the situation in terms of an interpretive approach, with the researcher as one participant whatever the actual 'role'.

Spradley distinguished between the "informal" and "formal interviews". The former occurred "whenever you ask someone a question during the course of participant observation", (p. 123), whereas the latter:

"usually occurs at an appointed time and results from a specific request to hold the interview."

(Spradley, 1980, p. 124)

Spradley's 'informal' interview seemed to be akin to the 'conversation' of

Schatzman and Strauss, and the 'formal' like the latter's 'lengthier conversation'.

Patton distinguished between types of "qualitative interviews". He classified these as the "informal conversational interview", the general "interview guide", and the "standardised open-ended interview". He stated that the difference between these lay in "the extent to which interview questions are determined and standardised before the interview occurs". (Patton, 1980, p. 197).

The 'informal conversational interview' was seen however "... as part of on-going observation". (p. 198). During this situation, the people being interviewed might not realise that they were being. Patton argued that this type was "the phenomenological approach to interviewing", and was a situation where the interviewer "has no pre-suppositions about what of importance may be learned by talking to people", and thus would be able:

"to pursue information in whatever direction appeared to be appropriate, depending on the information that emerges from observing a particular setting or from talking to one or more individuals in this setting. Most of the questions will flow from the immediate context."

(p. 198)

This type of 'interview' seemed to correspond to Schatzman and Strauss' 'situational conversation' and Spradley's 'informal' interviews.

The second type listed by Patton, the "general interview guide", was an interview situation where the issues to be explored were decided in advance, in terms of:

"topics or subject areas within which the interviewer is free to probe, and ask questions that will elucidate and illuminate that particular subject."

(Patton, 1980, p. 202)

This seemed similar to Lofland's "unstructured interview", but a little more 'open'.

Patton stated that the third type of interview to be listed, the

'standardised open-ended interview':

"consists of a set of questions carefully worded and arranged with the intention of taking each respondent through the same sequence and asking ... the same questions with essentially the same words ..." [it] "is used when it is important to avoid variation" [it] "reduces the possibility of bias ..." [it] "may be particularly appropriate when a large number of people are to conduct interviews on the same topic ..."

(p. 198)

This type of interview seemed similar to Lofland's "structured interview", or survey methods, rather than 'qualitative research', especially with its reference to 'a large number of people'. Thus it did not seem useful for the researcher's overall purpose.

From reading, the researcher built up an idea of the general shape of the style of interview preferred, in the light of the basic approach.

Although it could not be envisaged beforehand exactly what questions would occur, since the researcher was trying to 'bracket off' any 'fore-shadowed problems' and go in with an 'open mind', it was realised that some form of 'interview' would take place at some time, and that some 'interviews' might be more formal than others.

The initial 'access' meeting, for example, has been noted as being more contrived, or 'formal'. Both parties to this had some questions which were prepared, in mind at least, and so could be termed 'guided'.

But in realising that interviews would take place, the researcher took Schatzman and Strauss' point, and did regard an interview, even the initial access one, with the head or 'gatekeeper', as a form of 'conversation' between people involved, in however temporary a fashion, in the same 'situation', and therefore as having a two-way interaction. There was thus no intention of having the type of interview using a 'schedule' with the interviewee having to respond to fixed questions. However, the 'conversations', whether "on the spot" or later, were not seen as

'natural'. The researcher was aware that, as noted earlier, researchers (or anthropologists) "rarely just chat", and are always 'on duty' to some extent, and know this. Therefore it was considered that 'interviews' are always 'structured' in some sense, though not that of Lofland, by the researcher's purposes.

The researcher then, regarded interviews as 'conversations', which would be either shorter, or more 'informal', or longer, more 'formal' with some general guide or 'agenda'. The shorter conversations were seen as likely to arise in situ or shortly afterwards, and be based on immediate observation and thus be on a particular point, or points. The longer conversations were seen as likely to arise partly out of observation, or from reading documents, or from shorter conversations, where either a point might need to be explored in greater depth, or a number of points might require explanation. This type of interview then, was seen as 'focused' just as later observations were. Both types of 'conversation' were seen as likely to be 'structured' to a lesser or greater extent, simply because they were not really going to be 'natural'. However, because of the two-way interaction, they were not seen as going to be closed, but open-ended. That is, even in 'longer' or more 'guided' conversations, something would perhaps emerge which had not previously been considered by the researcher, which could provide a cue either for more observation or more interviews, or for looking at a document. The latter, as noted earlier, also provide a source for observations or interviews.

Some of the ideas developed about 'interviews' for the research have been expressed by other writers, who were not available while the field-work was in process.

Burgess, for example, noted that:

"There is a long tradition in social science research where interviews have been perceived as 'conversations with a purpose'."

(Burgess, 1984a, p. 102)

This was listed as the "unstructured interview".

Woods considered that 'interview' was an "inappropriate term, implying a formality the ethnographer seeks to avoid". (Woods, 1986, p. 67). He preferred instead to use the term 'conversation'.

Earlier Woods, in discussing the methods used in his own study of a school, stated that participant observation involved, as much as observation "interviews, discussions, conversations: in short, some form of 'talk'." He added that in such 'talk' he favoured:

"naturalistic or behavioural talk, as opposed to reported talk, that is, talk heard and noted by me in the ordinary course of events."

(Woods, 1979, p. 263)

However, as Hammersley pointed out, in the later stages of fieldwork, a researcher might "require information on the same topic from a number of different informants". He added that "However long one waits such information might not emerge and one may have to ask directly". He stated further that by this time:

"the research problem has become quite narrowly focused ... the ethnographer has developed an agenda of questions to be answered."

(Hammersley, 1979, p. 153)

Hammersley and Atkinson also considered that "All interviews are structured by both researcher and informant". (Hammersley and Atkinson, 1983, p. 113). That is, both parties to an interview have their own ideas and purposes, in the researcher's view. These authors further stated that "The most important distinction ... is between standardised and reflexive interviewing". (p. 113). Questions in the latter are not decided in advance, although the general issues hoped to be covered are known. The authors also stated that ethnographic interviewers do not "restrict themselves to a single mode of questioning". (p. 113).

They pointed out that questions could be "non-directive" or "directive", depending on the researcher's purpose. In this study a range of types of

question were used. Often they were of the non-directive type, such as "Can you tell me of your daily routine?" rather than "Do you do 3R's in the morning?" Sometimes they were of the clarification seeking type, as when a teacher described herself as "formal", and the researcher asked, "What do you mean by formal?" Sometimes a question was expressed in the form of an observation statement with which the respondent could agree or disagree, on the lines of "It seems to me that" Sometimes the 'playing innocent' type was used, as when a teacher said that the head left teachers alone. "Does that mean that you can teach in any way you choose?" Sometimes questions were direct, as to a child, "Can you choose what to do?" or "Do you group the children?" Also, summaries were used, as with the head, "So what you're saying is that ...?" Another type of question, also direct, was asking for corroboration of an observation. "Did you really mean them to choose?" Simple information seeking questions such as "Why do you think that ...?" were also used.

What Hammersley and Atkinson, and Woods (1986, pp. 79-80) both pointed out, is that in ethnographic interviewing, as distinct from the 'standardised objective' type, a wide range of question techniques are necessary, because the researcher has to take account of both the interviewee's views of himself, and his/her views of the interviewer, and react accordingly, 'playing it by ear', in the light of circumstances.

Thus, the researcher's position on interviews was that these were 'reflexive', though this term was not applied to the general style adopted.

Thus, when the fieldwork stage began, there were some general ideas about what an interview might be, and an awareness that any interview was a two-way interactional process, not a simple question-answer situation. It was recognised that, in fact, an interview was a form of relationship between people, and thus that personalities might enter into it.

In the pilot study schools there was less time to follow up ideas, or

have very 'long' conversations, but there were interviews of both kinds. At Moorland, and to a lesser extent at Larkway, there was more opportunity.

There are some difficulties with the people to be interviewed in an infant school, the pupils and the teachers.

King noted that 'interviewing' young children was difficult, because children "seem to be almost incapable" of reflecting on their own ideas, something which he said teachers were "unused" to doing.

Also, as King also pointed out, given say thirty or so children, there would be this number of "subjective meanings" to "a given course of action or interaction". (King, 1978, p. 8). Nor do young children form recognisable classroom 'groups', such as Pollard's 'jokers' and others, to get over this problem. (Pollard, 1985).

Therefore, the children in the school were not 'interviewed' as such. They were talked to naturally, as and when they came up to the researcher and said what they were doing, or when the researcher sat at one of the tables in a classroom, or when a small group was worked with. The researcher also stopped and chatted to children whenever possible, again in the most informal way. Thus, the researcher did try to obtain children's views about the school routine, and their knowledge of what was expected of them, and so 'check accounts' by comparing their ideas with observations and teacher accounts.

The problem with interviewing the infant teachers was that they did not have a great deal of time available to devote to a researcher's concerns. In the classroom the children tended to occupy their time, because the teacher was always involved, either with a group or with individuals. Infant teachers rarely just sit at a desk, they are always either directly engaged with some children, or generally monitoring activities. Also, infant teachers do not have free periods, when 'longer'

conversations might be possible. Lunch-times and after school times were usually used as preparation periods. Also, since the infant teachers seen were mainly women, mostly married, they tended to have family commitments and did not want to stay too long after school.

Clearly, the depth to which issues can be discussed is influenced by the length of the 'interview' or 'conversation'. In the classroom, this could vary from a minute or two to five or more, depending on the exact situation - that is, the nature of the activities and their organisation and the behaviour of the children. In the 'longer conversations', the time varied from fifteen minutes to about an hour. In order to get over the problem of time for these longer interviews, more were held at Moorland. This was not possible in the Pilot Study schools due to the short stay in each of these.

In these schools, the head and each teacher visited was 'talked to' at least once, apart from the classroom questions.

At Moorland, the head was, apart from short comments, interviewed for longer periods on six separate occasions, and the other teachers, again apart from the classroom conversation, were seen between four and eight times. The nursery teacher was 'interviewed' on seven occasions.

At Larkway, the Head was interviewed on the first visit, and once again on the second. Further information and answers to questions were, however, given later in writing. The deputy head was 'interviewed' during the classroom observations in her class, and twice for longer periods. The other two classroom teachers were seen three and four times each respectively apart from the classroom.

Extending the longer conversations over a period enabled the researcher to refer to the same topic again and again, but the extent to which 'probing' was possible again depended on relationships being established.

The majority of 'conversations' did, of necessity, take place during break times or after school at the end of the day. In the longer dinner breaks or after school, teachers in all the schools were usually engaged in preparing work either for the afternoon or the following day. When they were talked to in these situations it was felt by the researcher that their being occupied might mean that they were more relaxed. Also, it was seen as useful to have some insight into this aspect of 'the teacher's day'.

Thus, these 'interviews' were in their own classrooms. In relation to the 'place' of interviews, Woods stated that 'interviewers' should if possible be allowed to choose the time and place, and added that:

"This is not only a matter of convenience but may give them a sense of control and confidence."

(Woods, 1986, p. 70)

This is what the researcher felt instinctively in the case of teachers in their classrooms.

However, Hammersley and Atkinson argued that by choosing the time and place, interviewees could manipulate interviews to their own advantage. (Hammersley and Atkinson, 1983).

A researcher does not necessarily have much choice in the matter. For example, in the majority of the schools in this research, the staffroom was not really suitable for conducting interviews. Infant schools tend to have small staffrooms in any event, and most of these were, as noted in 'Setting the Scene'. In lesson times, they were sometimes used for other activities especially at Moorland. At lunch times teachers occupied it, and relaxed in it sometimes at the end of the day. This was fine for 'listening in', joining in general conversation and getting ideas, but not for more private 'conversations' with individuals.

In the case of head teachers, they generally have an office, however small, and as they spend considerable time there, this is where a researcher

usually is expected to go. In the present research all the interviews with heads did take place here, apart from chance meetings in the hall or corridors. Because they were busy, with some teaching commitment beside their numerous administrative responsibilities, they usually said when they would be free and available for interview.

Having set out the basic view of interviews, and outlined the number of longer ones with various teachers, and the times and places where all the interviews were held, the section now gives examples of the 'interviews' or conversations held. It must be stressed that this is only a very small selection in relation to the available material, chosen simply to show the type of interview and the emergence of themes. Others, and some, of these, are referred to in the empirical chapters.

The shorter conversations or informal interviews, it was stated earlier, were those which occurred either in situ or very shortly afterwards.

These 'conversations' were initiated by the researcher either because of something observed which raised a question, or, on occasions, because a teacher volunteered information. This led, where it happened, to new ideas for either further observation and/or further interviews or a look at a document.

The questions raised during observation covered various issues, from the materials provided to the organisation of activities, and children's 'choice' in these.

The researcher was interested to discover what account the teachers gave for using various materials and providing different activities, even though from experience some knowledge was possessed of what might be a reason.

Other questions related more to what the children were doing, and to questions of 'choice' and 'grouping', as part of classroom organisation and teachers' views on this.

For example, in Class C at Briarfield the children were observed to be doing different activities. A little group of children came up to the researcher, and started to tell her, quite 'unsolicited' how much 'work' they had done, both that day and the day before. In the context of this conversation, the researcher asked, "Can you choose what to do?" There was some disagreement between the children. One boy said, "Well, the older ones can". There were various other replies.

During the fifteen minute break the researcher told the teacher what the children had said, and asked, "Can they choose what they want to do?" The teacher did not answer this directly, but replied that every morning after assembly there was a "class discussion" which might last "only five minutes or as much as twenty". After this, she added, "I tell the children their tasks for the day", and they then spent the rest of the day doing these.

From another observation in a reception class at Briarfield a child was heard to ask the teacher, "Are we going to do our work now?" When the teacher had a moment free the researcher asked, "Why did he say that?" This led to the teacher explaining then and there that this was because 'work' occurred during the afternoon, and other activities such as "playing with games", took place in the morning.

After the session, the researcher remarked that she had noticed a rather different pattern in the other classrooms in the school. The teacher agreed that in her classroom things were different, and went on to volunteer her explanation for the difference. This was that boys took longer to settle down than girls in the morning, but were ready to work by the afternoon. This point could not have been observed, but led the researcher to look for any differences between boys and girls in activities, something noted briefly in Chapter Five on teachers' social perspectives.

Similarly, observations in the classroom at Rushside led to conversations which provided 'new' information.

On one visit to the deputy head's classroom at Rushside, children were observed to be doing art and craft and 'number' activities. At the start of the afternoon the teacher was heard telling a group of children to do a number game. Later this group was told to fetch their books, and she told them what to do. All of this group were then observed to be all doing the same activity, which was adding numbers up to ten. Some other children in the classroom showed the observer their 'number books'. It was noted down that:

"Some of the books show the same work. This suggests that children may be grouped for number work."

(Observation Notes, Rushside)

There were two questions specifically which the researcher wanted to ask. The first was whether number, as well as art and craft, always went on during the afternoon. The second was whether the children were grouped for number. The researcher stopped to talk at the end of the afternoon.

The Deputy told the researcher in answer to the first question that those activities did usually occur together, and added as her explanation, that she operated "a version of the integrated day". This was an unsolicited term.

She added that her version of this did not necessarily correspond with that used by other teachers. She gave examples of other teachers whom she thought did use a different version, as well as those whom she thought did not operate at all in this way. She stated that she had started to organise her classroom in the way she did because "I worked in an informal setting before".

This question and the response gave the researcher the chance to ask other teachers in the school about this 'integrated day', and led to the view that this term had a range of meanings, when the term came up in other schools.

At Rushside, for example, alerted by the Deputy Head's comment, the researcher asked the teacher in Class Three if she did operate an 'integrated day'. She said that she did, but that she had only recently started it, for she had taken remedial classes before and this was the first time that she had had a class of her own. This classroom was one where 3R's work went on in the morning and Art and Craft in the afternoon, not one of the two which, as noted in Observations, had a different pattern.

The differences between classrooms was a topic raised in an interview of longer duration with the Head.

On the second question relating to number activities and grouping, the Deputy, asked if she 'grouped' pupils replied:

"Yes, I do. There are five number groups arranged according to ability. The bottom group, the ones you saw me talking to, are working on bonds to 10. The top group are working on numbers between 20 to 100."

(Deputy, Rushside)

This confirmed that the observer's interpretation was correct.

As noted, the Deputy stated that she operated a version of the 'integrated day'. Children's 'freedom to choose' is a feature associated with the 'ideology' of this. A sidelight on choice came from another period of observation in the Deputy's class. The researcher came into the classroom one afternoon to find a student there, and some of the children. A small group were standing talking, and another small group had gone over to a 'dough' table to make things. The Deputy came in as the rest of the children were entering. She told one group to do number. After all the children were sitting, except for the children using the 'dough', who stood around their table, the Deputy asked the rest of the children if they wanted to do the "bird project". From the preceding remarks before this request it was clear that some children had started on this in the morning, because as hands went up, they were told to "carry on", and went and got

out some books, from which they were making lists of birds. Other children, who had not started, and were not doing either number or the other activity, were asked if they were going to do the 'bird' topic. The teacher was heard to say, "You don't have to do one if you don't want to". Some more children started to work on the birds. During the rest of the afternoon the Deputy went round, and children not appearing to be doing anything, including those who had finished their number game and their number work, were again asked if they wanted to do the bird project. She again said that they need not do it.

The researcher's observations recorded the subsequent outcome, and the interpretation placed on events.

"By a quarter to three everyone in the class had 'chosen' to do a bird project. It seemed to me that although the children have been asked if they wanted to do the project, and told they needn't if they didn't want to, they haven't really had a choice. It seemed to me that they have all been 'gently pressured' into 'choosing' what the teacher wanted"

(Observation Notes, Rushside)

At the end of the afternoon the researcher said to the Deputy that she was curious about this, and showed her the above notes. Fortunately she had a sense of humour and laughed when the researcher asked, "Was I right? Did you really want them to choose not to do it?" She replied that she had really wanted the children all to do the same thing, "but you have to let them think they've a choice". (Deputy, Rushside).

This observation and the resulting 'conversation' made the researcher aware of 'choice' as a category having different meanings attached to it by teachers.

At Fairfield, similar patterns of observation and short 'conversation' provided other examples where one question might be answered completely or might, by the response, lead to further on the spot questions or longer interviews.

In the classrooms at Fairfield it was noted that all the children appeared to be engaged in different activities, as mentioned when discussing observation.

In one of the classrooms the teacher was asked directly about this, whether the children did work individually. She stated that while she did "phonic work" with groups, and sometimes "maths and writing practice with a small group" on the whole children did work as individuals. She then went on to say that in a "family grouped class" it was impossible to teach as a class, in her view, because the children were of different ages, and, consequently at "different stages". This viewpoint of 'developmental stages' could not have been elicited by observation alone.

In another 'family grouped class' at Fairfield, having observed that children were also doing numerous activities, the researcher asked the teacher at the end of the day if she would describe the "daily routine" she used.

The teacher said that at the start of the day she encouraged children, although they could choose what to do, "to start with a writing or number activity, not art and craft or water activity". She added that amongst the work the children had to complete by the end of the day was "a piece of writing, some number work and a story", but she described her system as "flexible".

The researcher then asked directly if she considered that the way she worked corresponded to "an integrated day", and if so, what she thought this term meant. She first agreed that she did, and then added that she had developed her own idea of what 'integrated' meant, and that other teachers in the school did not necessarily share this. This comment corroborated the information gained in the interview at Rushside on this topic.

In another classroom the teacher, when asked about her 'daily routine'

also replied that although children were "free to choose", she encouraged them to start with writing and number 'work'.

This teacher was in the staffroom after school on another day, and in chatting to the researcher about her class, remarked, "I don't like this class - even now they aren't settled".

The researcher had commented that the classroom atmosphere seemed different, even though the organisation seemed the same as others in the school. The teacher remarked that it was different, and put this down to the fact that:

"It isn't a proper family grouped class. It's made up of members of other classes, mostly boys and middle infants."

She thought that these factors affected the stability of the class.

The researcher found this an interesting comment for two reasons. First, infant teachers in the schools seen so far, and in the researcher's own experience, had not admitted to not liking a class. Second, her reference to boys alerted the researcher to the idea that boys might be a problem in terms of behaviour. These ideas were 'filed away' for future reference.

The similarity between the classes at Fairfield and the comments of teachers when asked if they could teach in a different way if they chose, as well as impressions of the headteacher gained from the initial interview, again led the researcher to raise this topic in a longer interview with the Head.

Two examples of short conversations taken from Moorland indicate the emergence of new categories in the research.

The first took place during one of the early periods of observation in Class 2, Mrs. Knowles' class. Mrs. Knowles was going round putting up displays when the researcher came in, so the researcher went to look at these. The children were coming in by now. The researcher observed that

they did not seem to take much notice of the displays, and said this to Mrs. Knowles. She agreed with the observation, and added:

"They don't really enquire about anything, whether it's displays or the work they have to do. It's been like that as long as I've been here."

The researcher asked why she thought the children were like that. Mrs. Knowles said that their parents did not take much interest in them. She stated that "Some parents never take their children anywhere, nor read to them".

The researcher suggested that perhaps parents had problems, such as 'large families'. Mrs. Knowles agreed that they did, but still added that she thought that "parents can't be bothered".

Although the children were all present by now, and this conversation was held in their presence, it was conducted quietly. It was not unusual for Mrs. Knowles to make 'off the cuff' remarks to the researcher when the children were present, and references to their 'background', because the relationship was quite friendly. It was instances like this that led the researcher to focus on this aspect of teachers' 'perspectives' at Moorland, for comments of a similar nature were heard elsewhere in the school.

The other example from Moorland is taken from Class 3, Mrs. Martin's, the deputy head. It had been observed previously that when children came into this classroom, they either went to activities which had been set out by the teacher, or asked if they could get something out themselves. The researcher asked Mrs. Martin, "Do the children always start off by choosing what they'll do?" Mrs. Martin replied, "Well, first thing in the morning is 'Choosing Time', so yes, they can choose what to do". She explained her reasons for this pattern.

It was stated in relation to 'observation' that certain times of the

day were focused on, and thus included 'Choosing Time'. It was wholly Mrs. Martin's comment which made the researcher look closely at this time period, 'first thing in the morning' in other Moorland classrooms, to see if this 'Choosing Time' occurred elsewhere. It did, in fact, turn out to be one of the main features of classroom routine in the school, and to be different from 'choice' in other schools such as Briarfield and Fairfield. There, it was part of the daily routine throughout. At Moorland it was a special time of the day.

So from short interviews arising from observation two new areas of interest had emerged, which were quite different from anything found in the pilot study schools, or subsequently at Larkway.

At Larkway, not only did the researcher go in with more focused ideas, but also the whole 'atmosphere' of the school was different to Moorland. Teachers appeared more 'open'. In one of the reception classrooms, after the researcher had been there for a short time, the teacher came over and initiated the 'interview' by asking, "Is there anything you want to ask about?" The researcher replied, "Well, could you tell me about how the day is organised - what the daily routine is?" She replied:

"Well, the period first thing in the morning is 'choosing time', after this I call the children on to the mat to discuss what they are going to do next - what activities, that is."

She then went on to talk about "grouping" as the basis for what the children did. She also said that she operated an "integrated day", and this statement was not in response to a question by the researcher. This was recorded in the observation notes.

"The teacher introduced the term 'integrated day', not myself. I consciously refrained from introducing terms like this because I wanted the teacher to tell me how she defined her approach."

Having heard the teacher say that she worked this pattern, the researcher was then free to ask her how she defined an 'integrated day'. The teacher

responded by asking the researcher how she defined it, saying, "What do you think this means?" This was a clear demonstration of the two-way exchange that is a 'discussion'. When the researcher had given a definition, the teacher explained what she meant by the term. Her definition could then be compared to those of others at Larkway, including the Head, and to those from the other schools, thus adding to the researcher's knowledge of this category 'integrated day'.

The various examples given above of 'short conversations' arising from observations, and taking place on the spot or soon afterwards, indicate that this situation can produce ideas, which can be developed into categories, for later investigation, as well as some 'complete explanations'. Both of these can be used to compare across and within schools, if the researcher is trying to do this, as to some extent the present researcher was.

The 'longer' interviews are discussed next. These are 'conversations' with a guide, where a topic needed to be expanded.

The 'agenda' or guide for the 'longer' interviews was based either on previous observations, and 'verbal cues' provided by the teachers and children in these, or from shorter interviews or in some cases on documents. These interviews were regarded as attempts to explore in more depth a topic or 'category' that had emerged, such as views of the 'integrated day', pupil 'choice', 'home background' and the role and influence of the head, amongst others. They were also seen, as the shorter interviews, as sources perhaps of new information, as well as clarifying the 'old'.

In order to reduce the stress on teachers the researcher tried to give advance notice of the wish to talk to them. It was not always possible to do this, especially in the Pilot Study schools where visits were shorter.

In the case of Moorland and Larkway advance notice was also sometimes

given of the 'agenda' itself. This was part of 'impression management' and was intended to create the impression that the researcher 'knew what she was doing', as well as to reduce uncertainty for the teacher and so perhaps induce them to co-operate. Also, as part of trying to help 'friendly relations', the researcher tried to begin these interviews with some 'ordinary' conversation, rather than leap straight into the questions.

These 'longer interviews' took two forms. Either they were 'longer' in terms of time on one or more occasions, or they were 'longer' in the sense that they were an extended series of 'shorter' conversations on the same theme either with one teacher, or with more than one for purposes of comparison.

Some of the longer interviews were with head teachers. These arose in some cases out of observations, and comments by teachers, and also from documents.

In noticing classroom differences in and between schools in the Pilot Study attention had focused on the degree to which teachers were free to organise their classrooms as they saw fit. The influence of the head had been mentioned at Rushside, for example. There teachers compared the style of the old head to that of the new. So an interview was arranged with the head, and the specific agenda was how she compared her own style with that of the previous head, and her view of her role and responsibilities. The 'role' and importance of the head had become a major topic, from a lesser interest.

The question of the head's influence was explored further at Fairfield, not because the teachers talked much about it, but because classrooms appeared similar. The researcher, because of what had been said at Rushside, commented to a teacher here that:

"Teachers in the school don't seem to talk much about the head and her 'influence' doesn't seem very apparent."

(Observation Notes, Fairfield)

The teacher replied that the head left classroom decisions to the teachers, except that she had decided on the reading scheme to be used without consultation. The teacher was asked, "Does this mean that you could work in a different way than you do now?" The teacher replied that the head:

"wouldn't approve of a formal day ... but then we wouldn't be here if we didn't want to teach an integrated day."

As a consequence of this 'conversation' an interview was arranged with the head. The agenda was how much teachers were free in the classroom, and again, the head's view about her aims and approaches. The researcher began by saying:

"The impression received from wandering round various classrooms and talking to teachers is that they appear free to decide how to run their classrooms. Would you agree with this observation?"

The head replied that she did leave teachers to do as they wished:

"... if they are working in a way I agree with. I admit I wouldn't be pleased if a teacher worked with a formal timetabled day."

She talked of how she had changed Fairfield over time from "a very formal set up under the old head". The researcher then summarised what she had understood from the head:

"So it could be said that the apparent freedom of the teachers to organise classrooms in the way they want is the result of a definite policy on your part - because you believe in freedom to develop not only for pupils but for teachers?"

The head agreed with the summary, and went on to talk about her own ideas about methods, her 'approach' and views on the organisation of activities and influences on her thinking. She stated that "I have been firmly committed to the integrated day throughout my teaching career". She also stated how she defined this, after a question by the researcher:

"I take this to mean children progressing according to their stage of development, and no fixed times for number, working and art and craft."

This was a new addition to the developing category of 'integrated day'.

As noted, some interviews were a consequence of documentary evidence. One instance of this occurred at Moorland at the end of September in the first term. The document in this case was not directly seen by the researcher, but was the reason for an interview with the head. The initial cause was a comment by Mrs. Neaves that she could not see the researcher at the end of the afternoon on this day, as the researcher had asked, because she had to attend a special staff meeting. The researcher 'naturally' asked what this was about. Mrs. Neaves said that it was to discuss the results of a questionnaire about parental involvement given to a sample of Moorland parents the previous June. One of the members of the university team responsible was coming to the meeting to discuss these results with the staff. This was the first the researcher had heard of this questionnaire, and the first intimation that it might be linked to the Moorland survey report previously given by the research supervisor. This was noted briefly when discussing access and the early response of teachers at Moorland to the researcher.

The researcher asked Mrs. Neaves what she thought about the questionnaire. Mrs. Neaves said, "it didn't mean anything. For one thing, we don't know exactly what questions were asked". She said that she thought that parents gave answers they thought the researchers wanted to hear, and said that one parent who had read the report had declared it to be 'completely untrue'. Mrs. Neaves added that in her view, parents wanted teachers to look after their children all the time.

All this sounded of enough importance to ask the head for an interview to discuss the results with her. This interview was held the following day. The researcher asked what the questionnaire had been about, and was told that it concerned parental involvement in the school, and the provision of out-of-school activities. Mrs. Warner, asked about interviews, first repeated Mrs. Neaves' view on the parents' responses. She said that "Parents put down on the form what they wanted the researcher to hear". She

also said that:

"The researchers aren't aware of the problems staff have in trying to further parental involvement. It has always been a problem here."

The researcher asked what Mrs. Warner thought of 'out-of-school' provision. Mrs. Warner said that, apart from the problem of "starting and supervising" such activities, in her view:

"only a few parents would support these, and they would be the same ones all the time."

She said that this had happened in the past when concerts has been arranged. She herself, she said, felt that "a personal approach" was required, meeting parents as they came into the school in the morning.

This sequence was an indication of Mrs. Neaves' view of parents, one to be followed up, and also of the head's view of 'parental involvement', again something to be explored later.

Another instance of the role of documents in the initiation of interviews also comes from Moorland. In this case, it was a direct influence. On one occasion in the Spring term the head at Moorland asked the researcher if she would be interested in seeing a report which she had prepared for a Managers' meeting, as it concerned her role as head of Moorland. The researcher found that the report also gave details about the head's view of the 'background' of Moorland children. As a consequence of reading this report, which contained a good deal of 'information', two interviews were arranged with the head. One was on her views of her role, and one on her views of the children. In the first interview the researcher started by saying:

"You wrote that 'certain responsibilities are implied in the job'. Can you explain what you meant - what these are?"

For the second interview two questions were:

"You stated in the report that children at Moorland had problems. Can you tell me more about what you see as the main problems?"

And also,

"You said in the report that children on the whole at Moorland 'lacked support'. What did you mean, and in what ways do they lack support?"

These interviews added substantially to the picture of the role and influence of head teachers, and a new idea of the importance of the 'situation' for this. They also added to the concept of 'home background' and 'problem area' derived from 'conversations' with other teachers in the school.

At Larkway, a document was also used to start longer interviews. Here, a questionnaire was given to each teacher prior to any classroom observations. This asked questions about teaching approaches and classroom organisation. The use of questionnaires in general is discussed in the next part of this section. It is mentioned here because the answers teachers at Larkway gave were used as the basis for some of the questions in the interviews which followed. For example, one of the reception teachers mentioned in the questionnaire "code of behaviour". The researcher reminded her of this and said, "I'm not sure what you meant, can you explain it in *more detail*?"

In the case of another reception teacher, she had stated on the form that there was a "common approach" in the school. She was asked in the interview what she meant by this.

The information gathered in this way both added to categories developed, and provided a basis for focused observations.

It was stated earlier that some 'longer interviews' were an extended series of 'shorter' ones on the same topic or topics. One example of this was the 'interview' with the Moorland nursery teacher.

On an early visit to the nursery, when it had been decided to study it in some detail, the teacher had spoken to the researcher about 'children's problems' in 'this area'. In a subsequent 'interview' on the second visit

she was asked by the researcher for clarification.

"When we talked last you mentioned that the children had problems. Can you tell me something about the children in this area?"

This led on to asking how she saw her task at Moorland nursery. She replied that she considered that the purpose of the nursery was to provide "social training" and develop "social skills", and that staff had to "start from the beginning" with these.

During the next interview it was thus asked:

"You talked about social training a lot when I saw you before. Could you explain this a little more, and tell me why you think it's so important?"

She proceeded to do so.

Thus, a series of interviews allowed development of ideas, both for the teacher and the researcher in the context of a quite easy relationship. Although partly dictated by questions of time, these interviews were useful also as checking devices, both on observations and statements in previous interviews, and the researcher's interpretation of events.

This type of 'longer interview' or conversation was used with other teachers at Moorland, though more successfully with some than others.

This 'extension' of short interviews was also used in comparing accounts. The 'series' here, though, comprised 'different' 'short interviews' with different teachers, which, taken together, formed one or more 'longer interviews' on the same topic. An example of this was different accounts of 'the integrated day'. Another was when the researcher decided to focus on the differences between the nursery and the classrooms at Moorland which contained 'reception' children.

In one 'short' interview, the nursery teacher had described the nursery routine as, "informal, there is free choice most of the day, not teacher directed activities"

She stated that in her view, by contrast, the 'reception' class was "a more formal situation, much more teacher directed".

Following this, the two 'reception' teachers at Moorland were interviewed. Both were asked questions about what, if any, were the differences they thought existed between the nursery and reception classes, in terms of aims and approaches. Both mentioned "helping children to settle down" as one of the aims. One teacher, asked what she meant by this, defined it as "socially, getting on with other children". They were asked whether children who had attended the nursery differed in any way from those who had not. One said that children who had not been in the nursery:

"take longer to settle down... They find it more difficult to fit in with other children."

(Class 1 teacher, Moorland)

The other teacher gave an example of a child who had attended the nursery, who could write, and had started reading. She said that if he had not been in the nursery he would have "needed a lot more preparation and more time to play". (Class 2 teacher, Moorland).

This series of interviews was a useful cross-check, both of different accounts and the researcher's observation. It allowed the category of 'differences between reception and nursery classes' to be built up.

In this part of the section the terms in which interviews were seen during the research have been set out. Following this the ways in which other researchers have classified interviews have been noted.

In giving examples of the different kinds of interview conducted by the researcher, the intention has been to show the links between observations and interviews, as 'checking procedures', and also to indicate the manner in which 'categories' were developed or refined in the process.

It has also been pointed out that interviews of any kind are a form of personal interaction. They are thus influenced not only by the purposes of

the participants, which may or may not coincide, but by their subjective interpretation of the 'other' and their intentions. So whether or not 'friendly relations' were established has a bearing on the information to be obtained.

The next part of the section considers the use of documents. As indicated in this part, they are also linked to observations and interviews, as methods of gaining data.

3. Documents

Unlike 'observing and interviewing', which are acts, documents are things, and acted upon. Thus their use cannot be foreseen in the same way that acts can be, at least partially.

Documents of various kinds were, however, used in the research. They were both a means of checking information gained from observations and interviews, and also a source for new information leading to new observations and for interviews. Thus, in practice, they were part of the linked process of data collection. Since the idea for the history chapters arose out of observations, these too can be seen as part of this.

There was not a great deal of information in the textbooks on methodology about the use of documents, but the researcher was alerted to the idea that they could be useful by certain anthropological studies. Boissevain, for example, in his account of research in a Maltese village, stated that besides participant observation, he also used documents such as "newspapers" and "yearbooks" to gather information. "... in order to piece together the background". (Boissevain, 1970, p. 76)

King also stated that in his study of infant schools he used a variety of documents, such as "letters written to parents, and guidance notes written for teachers" (King, 1978, p. 11), as well as "census data". (p. 85).

Also, it was known through the researcher's own teaching experience that such things as school pupil records, and teachers' record books were kept, and that these might be useful in some as yet unspecified way.

As stated previously, various methodological texts were referred to in the early stages of the field work. These included those of Lofland (1971) and Schatzman and Strauss (1973), and Bogdan and Taylor (1975), and Johnson (1975) and Patton (1980). The first four of these however, did not stress the use of documents, a point noted by Burgess in 1984. Patton mentioned two, as will be discussed later, and McCall and Simmons briefly referred to documents. (McCall and Simmons, 1969).

It was noticeable that from 1983 onwards several texts in the Sociology of Education did devote some attention to documents and their use in ethnography. Hammersley had stated, in a text not then available to the researcher, that such use was "an important part of ethnographic research". (Hammersley, 1979, p. 144). This seems an accurate statement.

When using documents as sources of information and ideas it is, however, essential to consider them in terms of the purpose for which they were written, and by and for whom they were. Therefore, as the research proceeded and documents came to hand and were used, they were considered in these terms.

Through experience, the researcher was aware that certain school documents have to be compiled by teachers, either at the request of the head or the Local Education Authority. It was not known what the latter required of the Head. It was also known that some documents were produced by the Head. All of these were seen as instigated by 'legal authority', and were therefore thought of as 'official'.

It was also known that most of these documents were not necessarily available to everyone, but were to some extent 'confidential', because they were meant for a limited audience.

When beginning the research then, it was known that some documents existed in schools, but the researcher was aware that consent might be needed to see them, thus that they might have to be asked for.

From Bogdan and Taylor the researcher picked up the idea of 'solicited' and 'unsolicited' documents. These authors, however, only discussed "personal documents", and their use of these terms seemed odd. They said that "solicited documents" were those which were "produced at the request of the researcher" and gave as an example the 'telling of a story' by a person "to a researcher in a series of open-ended interviews". "Unsolicited documents" were those which were "created by the subject either for his or her own use ... or at the request of some other party". They gave as examples a "personal diary" or "student composition. (Bogdan and Taylor, 1975, p. 96).

This usage seemed to confuse two meanings of 'produced'. This could refer in one sense to being written or 'made' at all, and in another sense to being brought out, or given, to someone. Now, in the first sense, a produced document might or might not be written with the researcher in mind. In the second sense, any 'produced' document (in the first sense) might or might not be 'solicited' by the researcher.

The researcher took 'solicited' and 'unsolicited' to mean whether 'produced' (again in the first sense) documents were asked for by the researcher or given without a special request. The idea of an 'interview' as a document seemed at the time of reading Bogdan and Taylor to be extending the meaning of document too far. Later though, it was realised that a statement by an interviewee or an observation recorded, when written down by the researcher, was a kind of document, one which had to be evaluated both in terms of the 'truth' or otherwise of the 'story' told to the researcher, and of whether the 'observations' and 'interviews' were recorded accurately. The first point, involving the checking of accounts,

has been noted in that the previous parts, and the second part, is discussed later in the section.

It was not considered feasible to ask infant teachers to keep a diary, and the children were too young in any case, so there was no problem of how to consider these.

Thus, at the beginning of the research, it was thought that any document which might be seen would have to be considered in certain ways. These were whether they were 'officially' prepared, who had access to them, that is, for whom were they prepared and to what degree were they confidential, and how they were obtained, that is, whether they were 'solicited' or 'unsolicited' as the researcher defined these terms. These considerations were seen as a necessary guide to evaluating them as sources of information.

The researcher was not overly concerned with 'classifying' documents in any rigorous way either at the start of or during the research. At the beginning it was not known exactly which documents were present in schools nor which might be available. During the research it was realised that most documents seen could be placed in a number of categories. When writing up data from documents, the broad considerations noted above were applied. Making other categories seemed unnecessary.

Since finishing the field work a number of texts have appeared which discuss the use of documents, and classify them in the process. Some of these classifications seem rather artificial, where not simply confusing or undefined. For example, Hammersley and Atkinson referred to a range of documents, which they said could be placed "along a dimension from the most 'informal' to the 'formal' or the 'official'." (Hammersley and Atkinson, 1983, p. 129).

However, they did not define what they meant by such terms, although they gave examples of each category.

Woods also referred to 'official' documents. These were said to include:

"registers, timetables, minutes of meetings, planning papers lesson plans and notes, confidential notes, documents on pupils, school handbooks, newspapers and journals, school records, ... textbooks, exercise books ... workcards, blackboard work and photographs."

(Woods, 1983, p. 90)

This seemed a rather comprehensive list to include under the term 'official'.

'Official' documents in terms of schools could comprise those produced by the Government or LEAs, or those 'legally required' of a school, or those produced by head teachers for teachers, or by teachers at the request of a head. Many of the items classified by Woods could fall within these categories, except for "newspapers, journals, textbooks, blackboard work and photographs". He did not explain why he considered these as 'official'. Also, he did not explain what he meant by 'confidential notes', nor how he distinguished these from 'lesson notes', for example, which a teacher might consider 'confidential'. In fact, it is not clear what Wood's organising principle was, since he did not define 'official'.

Woods also referred to "personal" documents, such as "... diaries, creative writing exercises, rough books, grafitti, personal letters and notes". Again, however, the term is not defined. The term 'personal' could be used to refer to documents which are not necessarily made public, in which case private might be a better word, because they are the property of a particular person who can choose whether or not to give access to it. 'Rough books' and 'creative writing exercises' are not 'private', because the first is issued by the school, and the latter are usually set by and seen by the teacher. So again, it was not clear why 'personal' is included in this range of items.

Burgess made three distinctions in relation to documents. These were, first, between "primary and secondary" sources, secondly, between "public"

and "private" documents, and finally between "solicited" and "unsolicited" documents. "Primary" sources were stated to be those which were obtained:

"... first hand and have a direct relationship with the people, situations and events being studied" [for example] "court records, minutes, letters, memoranda, diaries, autobiographies and reports."

(Burgess, 1984a, p. 124)

Secondary sources, on the other hand, were said to be documents which "have already been published" [such as] "transcripts or summaries of primary source materials". (p. 124).

Although the general distinction between 'primary' and 'secondary' was a useful one, and one of which the researcher was aware, particularly in relation to historical documents, it was not clear why a 'transcript' of a 'first hand account' or a published account in general, was necessarily a 'secondary source'. For example, a head writing down an account of her day is producing a 'first hand' account, and such a document is still then a 'primary' source for a researcher. Also 'court records' are a form of transcript, and so could equally have been considered under 'secondary' sources.

In his second distinction, between "public" and "private" documents, Burgess gave no definition of these terms but only examples. The former were said to indicate "newspaper reports and police records" while the latter included "... letters, diaries and autobiographies". (p. 124). "Public" could refer to documents available to everyone, while "private" could be used to refer to documents belonging to one person, in which case it could be synonymous with Wood's "personal". Burgess noted, however that "private" documents could become "public" if they were published. (p. 124).

While "newspaper reports" seemed obviously "public", in the case of "police records" it seemed that these might be restricted in access, and so more 'confidential', so it was not clear why these items were both classified as "public".

In relation to "solicited" and "unsolicited" documents, Burgess made the same distinction, although with reference to documents in general, that Bogdan and Taylor did with "personal documents", and thus similarly confused the two meanings of "produced". (p. 124).

The above examples show some of the problems in attempts to classify documents, in that categories are not clear cut.

As noted, the researcher chose to consider documents in fairly simple terms, concentrating on who 'produced' what for whom and for what reasons.

During the school based field work a variety of documents were looked at in the various schools. These included: handbooks for parents, guidelines for teachers, an outline of 'Aims and Objectives', details of notes to Managers. All these were written by the head teachers. Other documents included school records. The latter included forms completed by parents when their children entered the particular school. They contained details about parental occupation, names of the parents and problems at home, i.e. whether parents were separated. School records also included individual children's records kept by each teacher about progress in various areas of the curriculum and comments concerning social, emotional and physical development. In each classroom a variety of documents were available. These included information about reading schemes, reading books, work cards and children's exercise books.

Other documents were collected outside school from various sources including the local archives and the library. These included old log books of other infant schools in the area, and newspaper articles dating back to the 1930s. These articles contained comments about the catchment area of Moorland school and the people who had lived there, so they were not general for all schools.

Of course, in a much wider sense, 'documents' refer to the wide

range of secondary sources used during the whole research period, including methodological introductions to field work and ethnography, other research on schools, anthropological accounts of various kinds as well as literature on the history of infant education and educational ideas. It was clearly impossible to discuss all these documents. In this section, therefore, documents refer either to those obtained directly from the schools or to documents such as old school log books, and old newspaper articles, which are historical documents of one kind, although school based in one sense because they refer to Moorland or other schools in the area. The documentary sources for the history chapter are also included, whether primary or secondary. The school based documents are discussed first.

The 'school' documents listed were considered in the ways outlined, whether they were official or unofficial, how far access was open or restricted and whether they were 'solicited' or 'unsolicited'.

Official documents included school log books, admission slips, and 'educational records' and statements of 'Aims and Objectives'. They were official in that they were legally required by the local education authority. On the opening page of the log book at Moorland it was laid down that this must be kept. It was stated that:

"A record should be kept at each school under the supervision of the headmaster or headmistress or superintendent teacher."

(Moorland Log Book)

It also specified what type of 'matters' should be recorded. For example, it was stated that any report concerning the school from the Ministry to the local authority, managers or governors should be recorded, and also notes made about it, "Events specially worthy of record for future reference". (Moorland Log Book).

The researcher was first alerted to the existence of such records

during a visit to one of the pilot study schools, Stone Street. This was the oldest school visited. During the visit to this school the head teacher showed the researcher the first log book kept by the school in the Nineteenth century. He stated that all schools had to keep one.

All schools are now legally required to provide a statement of 'Aims and Objectives'. However, it was only looked at at Moorland. This document began by listing the school's 'Aims and Objectives' and then set out the "teaching programmes" in the various parts of the curriculum. It covered literacy, mathematics, and moral, religious and social education, and "non-academic" areas such as art and craft, PE and music.

Other official records included admission slips. These had to be completed by the parents of new entrants. They contained details about parental occupation.

It was also required by the local education authority that schools should keep what were called the 'Educational Record'. This record required assessments about academic progress and personal and social development. The record card had to be filled in at the end of the reception stage and again at the end of the final year in the infant school. At Moorland each teacher also kept her own records similar in nature to the Infant Record. Each teacher was also required by the head to keep a 'reading' and 'number' record and special forms were given to the teachers for this. Thus these two were official documents in that they were required by someone in authority, in this instance the head.

Other documents were also authorised by head teachers. For example at Rushside and Larkway the head produced a pamphlet for the parents of children new to the school. It was given to the parents of children starting in the reception classes. At Larkway it contained details about the age when children should start infant school, general details about "organisation", who the teachers were, their responsibilities and a brief

outline of areas of the curriculum covered and a list of school rules.

Another document authorised by the head teacher was 'Guidance Notes for Teachers'. The format was not always the same in each school. At Rushside it consisted of an outline of various areas of the curriculum including schemes of work. This covered various aspects, such as "reading", "writing practice", "English" and "number". The head teacher at Moorland was in the process of developing guidelines on the curriculum. She had developed a programme for reading and this included her views on learning to read, and suggestions for teaching different aspects of reading for pre-readers up to advanced readers.

At Larkway the guidance notes contained information about the head teacher's "philosophy of education", her ideas about learning and about her views on the aims of the school. Separate schemes for each class were not specified, nor were suggestions for teaching various aspects of the curriculum given in great detail. Unlike the guidance notes for teachers at Rushside or Moorland, the notes at Larkway also gave an outline of the daily routine.

Other forms of documents were 'unofficial' in that they were not legally required or authorised by anyone for a particular purpose. Thus at Moorland the head teacher prepared notes for managers' meetings. These notes were produced for her own benefit, as an aid to discussion and were not required by the managers themselves. At Moorland the head teacher also compiled a list of occupations of parents of top infant pupils. She was not required to do so.

Some of the documents used in the research were non-confidential in that they were open to any member of the general public to look at. These included old newspaper articles, and a survey of the catchment area of Moorland. The former could be obtained from any public library. Copies of the latter could be obtained from a community project office. Old

school log books were available in the archives. However, access to such documents was not entirely open. People could not simply walk in and ask to see them immediately, in the same way they could with the newspaper articles. A viewing time had to be arranged in advance.

Other documents such as 'Booklets for parents' were available to all the parents of children at a particular school, and were thus partly non-confidential, in that they were available to all those parents. The guidance notes for teachers were also semi-confidential in that they were made available to all the teachers in a particular school.

Within classrooms certain 'documents' such as work cards were 'visible' and 'accessible' to both children and teachers and so could be termed semi-confidential. Children's own 'exercise' books were in a sense confidential and non-confidential in that teachers always had access to them. On the other hand, they did belong to each child in a sense as they contained work that that child had done. Also, when children put their books into their 'tidy boxes' such documents were then confidential in that other children were not allowed to look in those boxes or interfere with them.

Certain documents such as teachers' assessments about individual children were confidential, and not freely and openly available. Access was restricted to those who had the legal authority to look at them, such as the head teacher for example, or the adviser, or educational psychologist.

Earlier a distinction was made between those documents specifically requested by the researcher, and those given to her 'unsolicited'. Many of the documents fell into the latter category. They included the 'Notes for parents' at Rushside and Larkway. These were given to the researcher during the first interview at the above schools. The head teacher at Moorland also asked the researcher if she was interested in certain docu-

ments including some notes on her role as head which were written for a managers' meeting, and a list of parents' occupations, which she had compiled for her own use. The head at Moorland also provided 'unsolicited' an outline of the 'Aims and Objectives' of Moorland.

Some documents, however, were specifically requested including the 'Notes for teachers' at Rushside, the 'reading programmes' at Moorland and the guidance notes for teachers at Larkway. These were all obtained from the head teachers at each of the schools. Individual teachers in all the schools allowed access to materials which the children used including work cards and children's work books. At Moorland access was also permitted on request to teachers' written assessments of individual children.

This part of the section so far has outlined the types of documents used in the research and indicated how such documents were considered. Just as they can be classified in various ways, so they can also be used for a variety of purposes. However, as stated, at the time of doing the field work little information was available on how documents might be used in ethnographic research, although as noted, King did refer to documents in his study of infant schools. He did provide some information about their use. He referred to notes to parents and guidance notes for teachers which he said were used to obtain information about the explicit expression of the ideologies of head teachers. King also mentioned other documents such as individual pupils' records which contained teachers' written comments about children. His discussion of these documents alerted the researcher to two other ways in which documents could be used. First, the use of them could follow on from the use of other methods. Second, documents could be used as a means of corroboration. King stated that after having observed in each school the head teacher gave permission to examine each child's records. (p. 58). He also stated that such documents were another source, apart from "public utterances" and "private verbal

accounts" of teachers' typifications. (King, 1978, p. 58).

At the time of starting the research the only methodological text which seemed to deal with the use of documents was that of Patton (1980). He briefly outlined two ways in which documents could be used. It was argued that they could be "... a basic source of information about programme activities and processes" and secondly that they could provide "... ideas about important questions to pursue through more direct observations and interviewing". (Patton, 1980, p. 152).

McCall and Simmons argued that documents could be used "to establish facts about events which the researcher was unable to observe directly". (McCall and Simmons, 1967, p. 63).

Thus at the start of the research while some ideas existed that some documents *might be useful to look at, this awareness was very general.*

How the documents were used in this research is described next.

Documents were used to complement and supplement the picture being built up by the use of other methods such as observation, and interviewing and also, in the case of newspaper articles for example, to build up a picture about events in the past.

Documents were not referred to much in the pilot study schools, apart from a pamphlet provided by the head for the parents at Rushside, and the guidance notes for teachers at the same school. Of course, children showed the researcher their work books, and work cards were also examined. These latter documents were produced by and for the children respectively. Documents in the pilot study schools were not followed up to the extent they were at Moorland. At Larkway a shortage of time also prevented the use of many documents, and in one instance where a certain document was requested it was found that this was not kept. At Moorland the head teacher and staff kept records about pupils which contained details about home life.

At Larkway such documents were not kept because in the head's view they could be misinterpreted. What records did exist were about academic progress and as one teacher said, did not refer to behaviour.

Documents that were referred to were used in several ways which are now outlined. Sometimes the same document might be used in more than one way.

First some documents were a source of ideas for observation and interviews. One example of this were teachers' written assessments about individual children at Moorland. It was found that in the case of assessments made by one of the reception teachers that reference was made on several occasions to the notion of 'fitting into school'. The teacher wrote that certain children who had recently started school had difficulty settling into its routine. Thus, it was noted for example that one child had difficulty "... fitting into a more structured day", and in the case of another, "Susan has had difficulty conforming and fitting in to the school situation". (Records : Moorland).

It was noted that both children had attended Moorland nursery. The researcher then proceeded to observe the routine in both the nursery and reception classes and later to ask teachers concerned if they saw any differences between the nursery routine and the 'routine' in the reception class. The term structure had come up in discussion with the head and in conversation with teachers and was raised again.

As noted in writing about interviews during the second term of the field work, the head teacher at Moorland asked the researcher if she wished to see a paper given to a managers' meeting. Accordingly it was decided to conduct an interview with the head using the report as the 'agenda'.

In some of the schools the children were observed using work cards

particularly Briarfield, Fairfield and Larkway. Children showed the researcher cards they were using and the teachers allowed the researcher to look at the content of the work card schemes. This examination was followed by short discussions with the teachers about how such schemes were organised, and how they were graded, how they came into being. One teacher at Briarfield stated that "children progress through them at their own rate". (Teacher : Briarfield).

A teacher at Fairfield made a similar comment. Teachers also explained how children started to use the cards when they came into a class, and one teacher at Fairfield explained the factors which affected when children could start the graded scheme. These included having achieved:

"... a reasonable standard of writing, a sufficient vocabulary [and] ... able to read all the first set of reading books!"

(Teacher : Fairfield)

Through an examination of 'work cards' and subsequent follow up interviews with teachers in these schools the researcher was able to build up a picture about the content of the activities but also to find out about aspects of teachers' views about learning and aspects of organisation such as family grouping. The use made of work cards by teachers in different schools was compared. At Moorland it was noted that work cards were not used to the same extent as in Briarfield or Fairfield, although an examination of the work cards that were used showed that the content was similar. On talking to teachers and observing the use of work cards it was found that teachers directed children to particular cards. This observation of 'how the cards were used' was followed up by discussions with the Moorland teachers about the use of cards. Through an examination of work cards and subsequent discussions with teachers, the researcher was able to build up a picture about curriculum activities and the degree of pupil choice.

At Larkway the examination of the work card scheme led to a discussion

with one of the reception teachers about how the scheme was planned and who by.

There were other occasions when observations and interviews preceded the use of documents. At Rushside for example during an interview with the deputy she stated that there was a 'syllabus' for number work but that this was only a guideline. She had added that under the old head who had recently retired that there had been an outline laid down. This 'conversation' caused the researcher to ask the head if it was possible to look at this document. The aim was to see if things had changed. Reading the document it seemed to conflict with what the deputy had said. Therefore the researcher went back to the head to enquire further and received the information that it was still the notes for teachers written by the old head, and she in fact was in the process of producing a new one. So a statement by one person led to checking against a document, and this in turn to checking by means of another interview.

A second example of where an interview was followed by the use of a document was at Moorland. During an interview with the head teacher she described the nature of the catchment area and also described the way in which classes were organised in the school. She stated that the form of organisation which was being used at the time was not the same as had been in operation previously. After the interview the researcher asked Mrs. Warner if the school had a log book. She replied that it did and that it went back as far as the fifties, the time when the school was opened. The researcher asked if it was possible to look at it as it was thought that the log book might contain details of previous forms of organisation. The log book did show these changes and when they took place but also recorded a previous head's views about the nature of the catchment area and that it had been seen as a problem for a long time. This document thus provided information about past events and ideas to which the researcher did not have direct access.

During the field work at Moorland the researcher asked permission to see the written assessments made about individual children by teachers. This was done like King after a period of observation and listening to teachers talking about the children at Moorland in the staffroom, and when they were talking directly to the researcher about the children in their class. The written statements about children were compared with teachers' verbal statements. They were another source of teachers' views about individual children at Moorland. King stated that the records kept by the teachers he observed "were to some degree formalised versions of their typifications of individual pupils". (King, 1978). This was also true at Moorland. In the written assessments comments were made about problems at home for example, "unstable home life", "... has suffered a lot of upset ... father left home ..."

The written statements contained none of the condemnation that verbal statements contained. Comments about behaviour and home background were much more muted. These written assessments were thus another source of teachers' views of children and their home background.

It was noted earlier that King examined 'guidance notes for teachers'. He used these to obtain information about the official expression of the ideologies of head teachers in the schools he visited. During an initial interview with the head teacher at Larkway she talked briefly about how she saw her methods. The researcher asked if the head kept 'guidance notes for teachers'. Mrs. North stated that she did and gave permission to the researcher to look at these notes. As stated earlier these notes contained details about what Miss North termed her philosophy of teaching and views about the nature of learning.

The guidance notes were used as a jumping off point for discussions with the teachers in the school about how far they were in agreement with these notes and how much they shared the views expressed. Like the head

for example, they stressed the importance of children developing independence. The guidance notes also contained information on the "pattern of the day". The researcher had previously observed in each classroom and gained a general idea about the routine and the guideline notes were used as a check on the researcher's own observations and conversations with teachers. For example, from observation it was thought that 'discussion time' was a regular occurrence first thing every day and again at the beginning of the afternoon. The 'guidance notes' for teachers confirmed this pattern, but indicated that it had a specific name. It was referred to as "family time". (Guidance notes for teachers : Larkway).

It was stated earlier that each school had to produce a list of 'Aims and Objectives'. It was only looked at in Moorland infant school. This was the first school in which the researcher became aware of the existence of this document. This 'official' statement about aims was compared with the way the head spoke about her approach, and with teachers' private statements.

There were some instances when the use of one document led to looking at other ones. During the summer term at Moorland the head teacher asked the researcher if she was interested in a list of parental occupations and home problems of a group of top infants. *The researcher said that she would be interested.* At the time the researcher had compiled some information on teachers' views about the nature of children's home background at Moorland and their views about 'patterns' but there was no information on parental occupation. The researcher then asked if such information was available for all the children at Moorland. Mrs. Warner said that the 'admission slips' provided this information and gave the researcher permission to examine them. Much later, in fact the following year, the same type of information was gathered from one of the teachers at Larkway. The list of occupations in the two schools were later compared.

During the first term at Moorland the researcher was given a local survey of the catchment area. The report considered that the area had acquired a bad reputation over the years and referred to old newspaper articles. It was decided to follow these references up. They revealed that views about Moorland catchment area in 1980 had not altered a great deal since the early 1930s. This information together with the 'log book' and what people, including the head, teachers and a social worker and later a policeman said about it were used to 'build up a picture' about the area. This information was acquired over a long period of time.

Other ideas about the ways in which documents can be used in interpretive research have been expressed by other writers whose work was not available at the time of the field work.

Hammersley and Atkinson suggested that documents can:

"... serve as a source of 'sensitising' concepts (Blumer, 1954) [and] ... can be used to suggest potential lines of enquiry."

(Hammersley and Atkinson, 1983, p. 130)

Their comments related specifically to "biographies and autobiographical accounts". (p. 130). However, they would also seem to apply equally well to other forms of documents. As stated earlier written assessments of pupils by teachers at Moorland led the researcher to focus on the nature of classroom routine in a different way, and the notes written for a managers' meeting 'sensitised' the researcher to how the head viewed her own position as head teacher at Moorland.

Woods argued that documents could be useful in several ways. He stated that, for example:

"Useful support to observations and interviews is given through the judicious use of written or printed materials."

(Woods, 1986, p. 90)

It is not clear however what is meant by support. Evidence obtained from

documents could be used as a means of checking evidence obtained from observations or interviews. In other words documents can be used to confirm or corroborate evidence obtained from other sources. This was the case in the 'Guidance notes for teachers' in both Fairfield and Larkway as outlined.

Woods also argued that documents could provide "... useful ways into more detailed qualitative work". He stated that certain documents " ... may have little meaning divorced from the intentions of the compilers". (p. 92) so that it is necessary to follow them up with other methods such as interviewing.

Woods also suggested that the use of documents sometimes follows the use of the other methods. (p. 94). Thus, as stated earlier the researcher referred to the school log book to find out about aspects of organisation which the present head at Moorland had referred to in an interview.

Like Patton, Woods considered that documents provided information about processes. "Sometimes documents form an essential, perhaps the most important part of school processes". (Woods, 1986, p. 95). He suggests that school handbooks for example could provide views about a school's aims, and ideas about "the school ethos". (pp. 95-6). In this research guidance notes for teachers at Larkway and An Outline of Aims and Objectives at Moorland were used to build up a picture about the head's views about aims and objectives. and 'philosophy' and then followed up by talking to teachers to find out how far they 'shared' the head's view.

Thus so far the types of documents used and the way they were used has been discussed. The last aspect to be discussed in relation to documents are some of the problems associated with their use. For example the problem of bias. The researcher has to consider for whom documents are prepared, and by whom and in the light of this knowledge to assess what reliance can be placed upon them. As with other kinds of data, there are problems.

Hammersley argued that in documents what is produced is a:

"... rather more perfected and finely tuned version of someone's view compared to what occurs in interviews where responses are more spontaneous."

(Hammersley and Atkinson, 1979, p. 147)

He further considered that:"if the aim is to elicit ideals or considered judgements" then the production of a document may be preferable to an interview because in the former situation the respondent "... has more time to consider all aspects and implications of the question or phenomenon being discussed". (p. 147).

Hammersley appears to be referring to documents which the researcher asks the researched to write for the purposes of the research. The 'guidance notes' for teachers written by head teachers for teachers could be seen in the same light. Mrs. Warner, the head at Moorland, said that she was in the process of producing guidelines and an informant at Moorland said that she worked on these at home. Mrs. North too, indicated that guidance notes were 'considered' documents, informed by reading literature on child-centred education and attending an 'induction' course to prepare junior teachers for teaching in an infant school. While 'considered' views may be important, it should also be pointed out that documents may be written for a particular audience and that the writer may be concerned with the image he or she is presenting. Thus, with the notes to a managers' meeting written by the head of Moorland, these were written for 'presentation' to a particular group, the school managers. Thus the head may have been concerned to present herself and the school in a favourable light. Unfortunately the relationship between head and managers was not explored.

Patton states that documents may be:

"incomplete, inaccurate and selective in that only certain aspects ... that is positive aspects are documented."

(Patton, 1980, p. 158)

Woods, likewise stated that documents may not be complete. (Woods, 1986).

Thus at Moorland, admission slips did not contain details about all Moorland's parents' occupations. Some parents had refused to complete this part of the form. The written assessments which were written by the teachers at Moorland were watered down versions of what some of them thought about the children and there were only a few references to 'home background'. The comments were more muted than those expressed in the staffroom by the teachers themselves, or made to the researcher. Similarly the 'Aims and Objectives' outline mentioned the notion of children's interests and the need for oral work.

It mentioned social skills. However, it did not state that the children were not interested in school, that they had no 'language' or that they were like wild animals. These statements were made to the researcher by some teachers at Moorland.

It has been shown that documents as a source of data should not be accepted at face value. Like other sources of data it has its advantages and disadvantages. This point is made by both Patton (1980) and Hammersley and Atkinson (1986). Patton suggested that by using a variety of methods the researcher can "build on the strengths and weaknesses of each type" (Patton, 1980, p. 158).

He suggested that using a variety of methods increases the validity of the data. Hammersley and Atkinson also pointed out that all types of data should be treated critically and that no one set "... can be treated as unquestioningly valid representation of reality". (p. 138). They pointed out that while there may be problems with the use of the documents, the ethnographer is in a good position to overcome problems of validity because he or she may be able to investigate the context in which such documents are produced. (Hammersley and Atkinson, 1983, p. 139). In addition the researcher needs to supplement the use of documents with other forms of data collection. In other words no one method on its own is

sufficient to build up a picture about the 'reality' under study.

Thus far the reason for using documents at all has been discussed. It has also been stated which documents were used and for what purpose. A brief look at some of the problems involved in document use has been noted.

The documents outlined so far were produced for the schools' own use, and as Woods stated were part of a school process. However, other forms of document can be requested by the researcher which are designed by him or her. These include, for example, questionnaires. Woods argued that questionnaires may not be approved of by 'ethnographers', and that some may dismiss them as belonging to a different "style of research with basic assumptions opposed to ethnography". (Woods, 1986, p. 114). He stated however, that some ethnographers including Lacey (1976), Ball (1981) and Davies (1984) considered that questionnaires could be useful. (p. 113). He himself used one to find out about parents' views of subject choice. (Woods, 1979). Of course other writers before Woods argued that there should be some flexibility regarding methods, including Trow (1970), Wax (1971) and Schatzman and Strauss (1973). Trow for example argued that the researcher should attack problems "... with the widest array of conceptual and methodological tools that we possess and they demand". (Trow, 1970, p. 149).

During the course of research at Moorland a questionnaire was designed for use by the teachers. Later, as will be explained, it was redesigned for use at Larkway Infant School. The questionnaire at Moorland was in fact designed at the request of the head teacher. Towards the end of the research at Moorland the head asked to talk to the researcher about the research. She expressed a concern that the teachers did not understand what the researcher was doing. She considered that it was unfair

to expect teachers to discuss matters off the top of their heads and suggested that a questionnaire might be useful. It was thought that this would enable the staff to give "considered opinions" and that they would prefer a questionnaire. The researcher agreed to construct a questionnaire for use by the teachers at Moorland. This incident is important in that it indicates the importance of the head teacher as 'gatekeeper'. Hammersley and Atkinson suggested that the 'gatekeeper' may attempt to "guide the research in directions they prefer, or away from potentially sensitive areas". (Hammersley and Atkinson, 19 , pp. 75-76). Clearly Mrs. Warner was attempting to 'guide the research', and had a view of what counted as 'proper research'.

The first stage, in the design of the questionnaire, was to decide on the subject, and the questions to be asked. It was decided to have four sections. The first section was designed to get a picture of teachers' views of the catchment area of the school and relationships between the school and parents. The subject of the second section was the training and teaching experience of the teachers. The third and fourth sections were designed to look at the role of the teacher in the infant school, and the aims of infant education respectively. The main source of the ideas for the questions were various studies which had been concerned with the aim of primary teachers including Ashton et al (1975), Taylor et al (1975) and to some extent Barker Lunn (1970) and Bassey (1978). During the main part of the fieldwork at Moorland the head and the teachers had referred to the catchment area, but some teachers had been more forward in this respect than others. The main reason for including a section on the environment of the school was to ensure that some information was provided by all the teachers. The section on teachers' training and experience was included because there were gaps in the researcher's knowledge of this area. The main part of the questionnaire was concerned with the aims of infant teachers since this was a major concern of the research.

The above sources, together with Oppenheim (1966) and Cohen (1976) provided information on the actual way in which questionnaires are designed, and factors to take into account when designing them. For example Ashton et al included a section on biographical details in the questionnaire they designed and this was used as a basis for Section Two of the questionnaire. In relation to the sections on the 'role of the teacher' and 'aims' of infant education, the researcher looked at how various writers classified aims, particularly Ashton et al (1975). A list of questions was adopted from these sources.

It was decided to have a mixture of 'closed' and 'open ended' questions in each section. The first questions in the first two sections were intended to be fairly factual and where the respondents were offered a choice of alternative replies. These were followed by open ended questions where the respondents were free to give their own views on the issues.

Oppenheim argued that some "survey writers" preferred to start with "... a few factual multiple choice questions about the respondent's own habits, background or experience", and to follow these with questions concerning attitudes, whilst other researchers prefer to start with open-ended questions, and ask about the respondents' behaviour and experience at the end. (Oppenheim, 1966). In the first part of Sections Three and Four, respondents were presented with a series of statements relating to the role of the teacher, and aims of infant education. The respondents were asked to choose between a number of response categories, which indicated various strengths of agreement and disagreement. The response categories were numbered Five to One, the former indicating strong agreement and the latter strong disagreement. This form of rating is known as the Likert Scale. (Moser and Kalton, 1971).

Ashton et al (1975) stated that teachers involved in early discussions

about aims suggested a progressive, traditional dimension, and that the terms were meaningful to them. They accordingly identified certain elements and produced a set of paragraphs along the 'progressive' traditional dimension. Ideas for the statements in Section Three and Four were obtained from these paragraphs, and the statements were produced bearing these distinctions in mind. As noted previously in the Review of the Literature, terms like progressive are ambiguous. Therefore, it was not an easy task to produce a short list of statements which were precise. Oppenheim pointed to the need for clarity. He stated that "Greater precision concerning the purpose of the questions will sometimes make it easier to avoid ambiguity in question wording". (Oppenheim, 1966, p. 52).

The final draft of the questionnaire was not wholly satisfactory. Oppenheim stated that the researcher should allow plenty of time for planning and exploratory pilot work (p. 24). Only a few weeks were available and there was no opportunity for pilot work.

After having designed the questionnaire the next stage planned was to explain its nature to the staff and administer it. However, the head of Moorland stated that she wished to administer it. The researcher was not happy with this. The head teacher returned the completed questionnaires a few weeks later. She stated that she had 'discussed' the questionnaire with the staff during a staff meeting and that they had looked at each question in turn and decided how to answer it. It was not clear from the completed questionnaires how great the collaboration had been but in any case any collaboration did seriously reduce the validity of the questionnaire as a whole, apart from any faults in its design.

It was planned to follow up the questionnaire with a group discussion with Moorland teachers in order to clarify the issues with which the

research had been concerned and bring out submerged tensions and also to discuss the questionnaire itself. The head would not permit this, however.

It was stated earlier that the head of Moorland considered that if the teachers were given a questionnaire they could give more considered answers. The teachers' responses did not appear to reflect this view. On the whole those teachers who had responded well during interviews did so in the questionnaire. In the case of one teacher with whom the researcher had not managed to establish rapport the responses were very poor. This clearly indicates that the establishment of rapport is as important to those administering questionnaires as it is to those doing participant observation.

Mrs. Warner's comments about considered answers raises another point. Considered statements, in response to a formal questionnaire may bring in 'theoretical' educationalist views rather than everyday practical attitudes. Considered formal statements could be used to present the most favourable face to the researcher. There is a place for the questionnaire as Woods (1986) pointed out, but as part of a range of methods used by the ethnographer.

The head and teachers did raise some issues which the head sent to the researcher which seemed important and made relevant comments. With regard to questions 21-31 (see Appendix 1) it was stated that many of the statements were vague and "were difficult to agree or disagree with", and questions 45 and 46 were "too big a question to answer briefly". (Head : Moorland).

Such criticisms are accepted as justified. As stated in the Review any statements about progressive ideas are complex, vague and difficult to define.

At Larkway it was decided to try a shortened version of the questionnaire. (See Appendix 2).

The sections on 'The School and its Environment' and training and teaching experience were taken out. Two teachers, according to the head at Moorland, had refused to answer some of the questions in those sections. They considered, first that in doing so they could easily be identified and second that such questions were an invasion of their privacy. For these reasons they were deleted from the revised version.

The revised questionnaire was divided into two sections. Section A consisted of statements concerning the purpose of the infant school which teachers were asked to rate on a five point scale ranging from 'entirely agree' to 'entirely disagree'. The second half of the questionnaire contained a series of 'open-ended' questions in which the respondent was "... invited to put forward your own views" on aims, approach in the classroom, whether it was considered that there was a 'common approach' in the school, and finally whether teachers considered that there were any influences or constraints on what they did in the classroom.

A letter was written to the head of Larkway asking if she and the teachers would be willing to complete the questionnaire, and a copy was enclosed. As stated in the earlier section on access, the questionnaire was sent along with a request for an extended period of observation at the school. Woods argued that questionnaires can be used "as a starter to the use of more qualitative methods" (Woods, 1986, p. 115).

The questionnaire was introduced at the beginning of the research at Larkway, in order to indicate to the teachers in the school some of the issues with which the research was concerned, and it was hoped, provide a basis for discussion with individual teachers. The researcher was available if the teachers wanted any of the questions clarified.

The response rate at Larkway was very good. All seven teachers including the head answered all the questions. In the case of question thirteen: "Do you consider that there is a common approach in your school?" all except one teacher answered with a one word answer.

The researcher followed up this question during discussions with the teachers and the head. The latter explained how this approach had come about in her school, as is shown in Chapter Four.

There were a number of reasons why the response to the questionnaire at Larkway was better than had been at Moorland. First, as stated, the questionnaire was explained to the teachers and the researcher was around the school if the teachers wished to discuss any problems. Each questionnaire was handed back to the researcher personally. Secondly, the change in format might have been a factor. A criticism of the original questionnaire made by the head at Moorland and teachers was that some of the statements in Sections Three and Four contained more than one idea which made them difficult to rate. Both Oppenheim (1966) and Moser and Kalton (1971) emphasise the importance of the way questions are worded in questionnaires. When redesigning the questionnaire for use at Larkway an attempt was made to keep to one idea per statement. Another factor could have been that the questionnaire was given to the teachers personally.

The teachers at Larkway were asked if they had any criticisms to make about the questionnaire. They commented on the lack of space to answer the questions in more detail, and lack of time, and that the questions were too involved to answer briefly. One teacher wrote, for example, that:

"I could have answered the questions in much greater detail given more space and having more time. The questions are too involved to answer briefly."

(Teacher : Larkway)

The other teachers made similar comments. For example, another teacher wrote that the questions were "extremely broad [and] difficult to pin down exactly how one feels". (Teacher : Larkway).

In the case of the statements in Section A of the questionnaire one teacher noted that these were:

"difficult to answer by one word answers and consequently are not entirely accurate in expressing my opinion."

As stated, a similar comment was made by teachers at Moorland with

regard to the statements in Sections Three and Four of the original questionnaire.

During the course of the description of the use of a questionnaire at Moorland and Larkway a number of issues were raised concerning the importance of carefully planning, the development of rapport, and about the influence of the head teacher as 'gatekeeper' in the research. There are other issues. The comments by the teachers at Larkway just noted, indicate some of these issues.

The first issue is whether certain topics lend themselves to a 'closed' question format. Teachers both at Moorland and Larkway indicated that these questions were difficult to answer in one word, and that certain questions were ambiguous. Bassey considered that in designing a questionnaire the researcher should avoid ambiguity, and provide questions suitable for brief answers so that these can be easily collated. (Bassey, 1978). Such ambiguity may be difficult to avoid, where the topic for the questionnaire is complex and involved as was the case in this questionnaire. The teachers themselves stated that the issues were complex and were involved too.

Questions which respondents have to rate on a scale may not reflect what they think, and again relates back to the complexity issue and also to the importance of context. In response to one statement: 'Children learn best when involved in individual work', one of the teachers at Moorland replied that she could not rate the above statement because:

"Children will learn when work is presented that is suitable for their everyday needs. Sometimes children learn, individually, sometimes as a group, sometimes as a class."

(Teacher : Moorland Questionnaire)

The teacher had avoided rating the statement because it did not reflect what she thought. Oppenheim considered that a disadvantage of

closed questions is that they introduce bias forcing the respondent to choose between a list of alternatives. He argued that respondents may become annoyed because they cannot express their own ideas. (Oppenheim, 1966). This was one reason for having open-ended questions. However, in both the questionnaires given to teachers at Moorland and Larkway closed questions were put first and then followed by open ended ones. Oppenheim considers that the former should be placed first because if they are preceded by closed questions this may influence the response given in the open-ended ones. This did appear to be the case with respect to one of the respondent's replies to an open-ended question which followed a closed set of questions concerning 'Aims of infant education'. The question was: 'How would you describe your aims in relation to your school or classroom?' The teacher replied: "One attempts to organise the school/classroom in such a way that these aims can be worked for". (Teacher : Moorland Questionnaire).

In contrast to the information obtained through interviews with the heads particularly, such a reply in no way reflected how they saw her aims.

During this discussion on the use of the questionnaires no mention was made of comparison. It was not possible to compare the results of the questionnaire across the two schools because of the change in format, although some questions remained the same. It was planned to send the original questionnaire to all the pilot schools. A letter was sent to the head of each school asking if they would be willing to complete the questionnaire. The head of Ashley agreed to complete it but filled in one form for all the staff. Two other head teachers stated that their staff were too busy to complete the form. The head of Fairfield did agree to completing the questionnaire but due to the arrival of the summer holidays and illness of the researcher, the questionnaire was not sent out.

In conclusion to this discussion of the use of the questionnaire in this research, it can be seen as useful because it does as Woods pointed out,

give the respondents the opportunity to reflect about opinions, and can provide, therefore, a starting point for more individual 'qualitative discussions' with teachers. (Woods, 1986). If the questionnaire is followed up then any problems with questions can be discussed. Sivakumar used a questionnaire in her study of an Indian women's village. She followed it up with interviews to clear up any doubts regarding items in the questionnaire. (Sivakumar, 1979, p. 225). As indicated such discussion did occur at Larkway but not at Moorland.

The next part of the section looks at the historical sources for the two historical chapters.

So far various documents have been discussed, but not those used for the historical chapters. In the introduction to this chapter it was stated that the documentary sources for these would be discussed and also that the reasons why an historical dimension was considered would be given. The latter aspect is discussed first.

It was noted in Section Four of the Review of the Literature that there may be many related reasons for doing a particular form of research and it can be difficult to disentangle them. This was true to some extent of the historical chapters. As also noted in the Review the original impetus for considering the inclusion of an historical dimension arose from reading Sharp and Green and King, which, given a prior interest in the history of education, raised questions as to whether they had sufficiently considered the history of infant education. The empirical work stimulated further exploration.

As noted earlier, the researcher had had some prior interest in the history of education, having previously undertaken a study on the concept of teacher autonomy which required looking at this. In the course of doing that research the development of 'progressive', 'child-centred' ideas had been briefly referred to. It was against this prior historical background,

which provided some perspective from which the accounts could be assessed, that the researcher read the work of Sharp and Green and King, and because of it questions were raised during this reading. In particular these were concerned with their comments on the historical development of infant education. Sharp and Green, as noted, wrote of the interplay of different traditions within "British Education". This seemed a wide definition of infant education. This seemed an interesting idea to follow up. The researcher wondered if they had specifically looked at infant education as a special field or if this had been taken for granted. If different traditions exist in 'British education' it seemed that this might also be true of 'infant education' particularly since King seemed to consider that a separate 'infant tradition' existed as did Silberman (1970). The researcher wondered if infant schools had been so separate from the rest of the State system that an 'infant tradition' could be said to exist. Thus, there were doubts about Sharp and Green and King's account of the development of infant education. A further question then raised in reading was the view expressed by both Sharp and Green and King, as noted in the Review, that phenomenology was ahistorical. Vulliamy stated that one criticism of some "phenomenologically based participation studies of classroom interaction" was that they were ahistorical. He, however, argued that an historical approach could be built in "which attempts to locate current attitudes as part of the on-going historical process". (Vulliamy, 1978, p. 119). Vulliamy did this in his own account of the "subject perspective of music teachers". He argued that by "looking at the original motivations of those who sought to introduce school music in the late nineteenth century" it could be seen how music teaching came to contain the ideas it did. (p. 119) This researcher was concerned with looking at the "original motivations" of those who sought to develop infant schools. This researcher considered that using a phenomenological approach historical issues could be examined by asking what definitions or typifications were held by various groups and individuals about the purpose of education in, for example, the Nineteenth

Century, with respect to infant schools in particular.

A third idea was the view that perhaps both Sharp and Green and King had overestimated the influence of Plowden and had consequently 'taken for granted' the status of the 'child-centred' 'progressive' ideology within infant education. It was considered that if a 'phenomenological' approach had been used, as Sharp and Green stated had been originally in the work, then this would involve questioning 'taken for granted' ideas. As M. F. D. Young (1971) stated in relation to the 'New Sociology' it should be concerned with examining such ideas and assumptions.

Thus initially, interest in the historical dimension was in the form of a series of 'I wonder ifs' rather than being a clearly formulated intention. At first the idea of having an historical dimension was just an interesting possibility.

Apart from this initial interest, the empirical work itself gave rise to themes which indicated that some reference to history would be necessary to explore them.

References were made at Moorland by the head and others to the history of the area. There were some indications that these past views of the area to a considerable extent remained part of their present consciousness. Also, as noted a local report referred to the history of the area as well. An examination of this document indicated that ideas about the area had not altered very much. This was discussed in the previous part of the section.

During the fieldwork at Moorland teachers expressed views about the nature of the children and their parents and the need for social training. It struck the researcher that there was a strong similarity between these views and those expressed by the founders of the elementary system in the Nineteenth Century. It was wondered whether such a concern had also been expressed by the founders of infant schools.

During the empirical work at Moorland differences were observed between the nursery and the infant school. It seemed from observation that the former contained more of the features generally attributed to progressive, child-centred practice than the latter. This raised questions in the researcher's mind as to how nursery schools had developed historically and whether such development had been similar or different to that of infant schools. The researcher had already, as noted, some knowledge of the historical development of the elementary system but not specifically of the infant school and nothing of the nursery school. There was an element of serendipity. The researcher came across a study of the transfer from nursery to infant school. This seemed to some extent to confirm some of the researcher's observations about nursery school.

By the end of the fieldwork it was considered that there were enough ideas to support to inclusion of some historical material.

Finally, a note on sources for the two historical chapters. These, Chapters Nine and Ten, have used for the most part, although not exclusively, secondary source materials which were readily available rather than original sources. It was considered that even using such sources it was possible to show the range of ideas which affected the development of infant schooling. The history section also only occupied a small section of the thesis and there was not time to look at many original sources. Also, whilst this researcher did have some previous knowledge of the history of education she was not trained in the use of historical documents and their interpretation.

The use of secondary sources can present problems. One is that without background knowledge of the historical period, including the philosophical and political debates, and without the possibility of knowing the political and sociological views of the writers themselves, it is hard to evaluate the degree of bias entering into the selection and interpretation of material in the histories presented. However, even if primary sources

are used this difficulty remains. All experience is filtered through reflection and thus interpreted so that there is a possibility of bias entering into the perceptions of those reflecting and giving accounts. Even if original sources are used these are not free from distortion. Platt argued that unlike the "field worker" the "documentary researcher" cannot "get behind the original accounts". (Platt, 1981a, p. 49). As noted previously, some documents were referred to in the schools visited, such as a head's 'notes' to a managers' meeting. In this instance it was possible to check statements made and discuss them with the head. It is not possible to do this with documents written by people in the last century.

The possibility of bias is increased when using secondary sources. In compiling material for these, the original historian will have been interpreting interpretations, a 'second order construct' in Schutzian terms. Writers in the past writing about events in which they did not directly participate may have interpreted them in terms of a particular perspective just as today they write from a present day frame of reference. In the case of secondary sources, a researcher who then uses these is operating with a 'third order interpretation' and so compounding the problem of bias. There is a possible danger in using 'history of education' accounts of reproducing the same bias as in the history books. This is perhaps reduced by reading a range of material,

In relation to such sources as histories of education another problem is that they have tended, until comparatively recently, to concentrate on only one area, that of the 'official views', those of the administrators and founders of education rather than on the practices of education and the views of those receiving it. Silver stated this view, commenting that accounts of Victorian 'popular' education, such as descriptions of the monitorial system, were based mostly on the views of its founders rather than how this operated in practice. (Silver, 1977). McCann also argued

that practice had been largely ignored in the past stating that:

"... educational historians have assumed that what schools set out to do was achieved without taking into account the problematic nature of the working class pupils and his parents".

(McCann, 1977, p. XI)

Whitbread, writing specifically of previous histories of infant education argued that these had concentrated more on "the ideas of the great educators" rather than on "the development of the infant school". (Whitbread, 1971, p. XI). Whitbread herself attempted to show that "Content and method in nursery and infant schools have been subject to conflicting traditions" rather than one. (p. XI). What Whitbread seemed to be implying was that accounts of infant education had been selective leaving out other traditions.

A concentration on 'official' views is not in itself wrong. It very much depends on how these views are selected and interpreted. It can be shown by reference to official sources such as HMI reports, that references to the practice of schools were available, because HMIs were able to see at first hand what was taking place in schools. It seems that a reference to official sources is seen as automatically meaning presenting a uniform view. In this study some official sources were used including log books and some 'original' sources were used such as personal accounts of educationalists and administrators such as Wilderspin (1840), Robert Owen (1857) and Kay-Shuttleworth (1862). Also secondary sources were used which themselves referred to primary sources such as HMIs. It was found that there was far from being uniformity in the views expressed.

McCann's view, as indicated in his comment above, was that the processes of education needed to be considered rather than the aims alone, particularly the response of pupils and parents. He considered that primary sources were available for this, including "... autobiographies ... diaries of those who attended elementary ... schools". (McCann, 1977, p. XI).

McCann also referred to school record sources such as "school log books, attendance records and minute books". (McCann, 1977, p. XII).

Humphries showed that the use of oral history could be a source of 'working class' people's views about the latter schools. (Humphries, S., 1981). The problem about those recollections is that they generally seem to refer to people's later experience of school and not their experiences of the infant part of education.

One further problem in trying to write an account of the development of infant education is that history of education texts tends to deal with the development of elementary schools in general and not that of infant schools in particular. There were some specific references to infant schools used in this research. These included the accounts of Rusk (1935), Raymont, (1937), Selleck (1972) and Whitbread (1972). However, other material was more scattered such as that found in Silver (1965), Stewart and McCann (1967) and Blackstone (1971). Silver found that citing a shortage of information was a poor excuse. (Silver, 1977). This, however, may apply more to a specialist. A non-historian operating with limited time and access to archive material has to do the best with what is more readily available. Even using the readily available and largely secondary source material there was still something to be mined in this.

Whitbread cited the Froebel Institute as a source of information about infant schools. Clearly there do appear to be primary sources available on other aspects apart from 'official viewpoints'. In this study an attempt was made to look at primary source material of this kind including, as stated, log books as well as those previously mentioned which referred to administrators. However, many of these primary sources are not necessarily available in libraries and as time was limited not all avenues were explored although some were in a small way. The previously mentioned lack of training in history was also felt to be a disadvantage when seeking to use archive material.

This part of the section has concentrated on the use of historical documents in this research and attempted to show why an historical dimension was considered necessary. It has also been shown that as with any other form of document historical ones cannot be taken at face value.

The next part of this section deals with the methods of data recording used in this research.

4. Recording Data

All forms of data collection provide copious amounts of information of various kinds, even in the early stages. Obviously this information cannot all be kept in the head for all of the time, however good a memory the researcher possesses, so some means of recording data must be utilised in order to produce a research report. As Lofland stated:

"... the fundamental concrete task of the observer is the taking of field notes. Whether or not he performs this task is perhaps the most important determinant of later bringing off a qualitative analysis. Field notes provide the observer's raison d'etre"

(Lofland, 1971, p. 102)

It is not only a question of remembering data factually, however. As Schatman and Strauss stated, a researcher:

"requires recording tactics that will provide him with an on-going development dialogue between his notes as discover and social analyst."

(Schatzman and Strauss, 19 , p. 94)

That is to say that the records kept during fieldwork have to consist not only of factual descriptions of observations, interviews and documents, including verbatim comments and statements, but also the researcher's comments to and about him or herself. These will be either about the ideas which occur (or not), the methods being used and their effectiveness (or not) and the personal feelings about being a researcher. Burgess divided records into "substantive", "methodological" and "analytic" field notes. (Burgess, 1984, p. 167). The whole process is not unproblematic. It involves ques-

tions of what to record and how, when and where to record it, as Hammersley and Atkinson also noted. (Hammersley and Atkinson, 1983, p. 146). But even knowing that these questions have to be considered is not a help in actual recording. Boissevain, writing of his research said that in his "graduate work" he had "received little systematic instruction", (Boissevain, 1970, p. 79), on how to process his data, but heard how someone else had kept notes, and followed their advice. There is now considerable advice in methodological tasks, but the problems still have to be tackled by the researcher in the individual setting, especially in terms of what and how much.

Both Lofland and Spradley distinguished between those notes made during observation, and later notes written up. Lofland outlined the process as first directed "consciousness" and the making of mental notes, secondly jottings and third the full field notes. (Lofland, 1971, pp. 102-3). Spradley spoke of "condensed" and "expanded" accounts which should include the verbatim and concrete principles, that is note the actual words used and use concrete language not generalisations to describe situations. (Spradley, 1980, p. 68).

Schatzman and Strauss outlined a different system. They presented a model whereby a researcher's field notes are organised:

"in relatively distinct 'packages' of materials according to whether they constitute 'observational notes', 'Theoretical notes' or 'Methodological notes'."

(Schatzman and Strauss, 1972, p. 99)

However, the actual example he gave of his own field notes so ordered seems quite tidy, not as if it was made on the spot. The same criticism can be levelled at the example given by Spradley, who said of his recordings of a grand jury that he:

"worked unobtrusively ... making notes during meetings where possible, and recording my observations in more developed form after a day long session."

(p. 74)

He said that some field notes were made during a first meeting, but did not say that they were expanded. He quoted that:

"I parked near the county courthouse Streams of people flowed into the lobby and scrambled into the waiting doors of elevators. 'Going up Sir?' a young man called to me"

(Spradley, 1980, p. 74)

This did not read as if he was making hurried notes on the spot, so it was not a particularly useful example for the first stage of recording the 'condensed' or 'jotted' account. Nor did Lofland give actual examples.

Hammersley and Atkinson, who noted like Spradley the importance of concrete observations, gave an example of what they meant by this in contrasting two accounts of the same incident, which makes this difference clear. (p. 153)

Hammersley and Atkinson also pointed out that field notes involve selection in some way. (p. 150). There is no way in which everything can be recorded at once. Overcoming this problem involves the same move from general to focused recording already noted in relation to observations and interviewing. In the classroom, notes were made of particular segments in turn, returning to these over time. Longer 'conversations' were recorded verbatim, because these were slower paced and in any event were focused.

In recording data the researcher followed Lofland, and to some extent Schatzman and Strauss as a general guide.

The principal means used to record data were handwritten notes rather than using a tape recorder. The researcher was not in the pilot study schools long enough to enable a tape recorder to be used. Woods argued that 'note taking' in the early stages of the research may get in the way of developing rapport and trust with the researched. (Woods, 1986, p. 28). However, the comment also seems applicable to the use of tape recorders in the early stages. At Moorland it was not really feasible to

use a tape recorder, in spite of being there longer, although it was used on one occasion with some top infants. There was little space to actually use one. The teacher's table was usually covered with various items such as children's work books, class register for example. The children's tables were usually covered with materials the children were using and work books. On one occasion a tape recorder was used in a classroom, that of Mrs. Neaves. It was placed on a small cupboard. There are four problems. First, there was a whine on the tape. Second, with children moving about as they did, what tended to be recorded was a general hum, interspersed with a few barely audible snatches of conversation which were very difficult to transcribe accurately. Third, the teacher did not appear to like the presence of the tape recorder. She kept looking at it everytime she passed the cupboard. Fourthly, it seemed to distract the children too since a tape recorder was not a piece of equipment which was a usual feature of the classrooms.

A tape recorder was used on one occasion outside the classroom at Moorland with a small group of children, two boys and two girls. This was on a day when the researcher was not too happy with the research, and temporarily partly abandoned it for a teaching role for some light relief. The researcher had in mind the use of pictures to tell a story, as in Bernstein's experiment, and asked one of the teachers, Mrs. Neaves, if she would allow some of the children to come and do this activity, which she did. It was found that the children were able to explain the story from the pictures, though one girl was the most fluent, especially if they were asked to explain as if the researcher wasn't there and couldn't see the pictures.

The rest of the tape was a 'conversation' between the researcher and one girl, in which she described a 'day at school'. This tape was transcribed. The picture presented by the child confirmed observations made in the classroom and similar to the statements about classroom activities.

This was quite a long interview, and tended to throw a different light on the frequent comment by teachers that children at Moorland could not concentrate. It was the first time the researcher had ever talked to a child on tape, or anyone else for that matter.

A tape recorder was not used at Larkway either. The research experience at Moorland had been such as to make the researcher wary of upsetting the teachers at Larkway by using one. However, if the period of observation at Larkway had been extended then use of a tape recorder might well have been considered as relationships with staff were developing well.

Tape recording of events has one particular advantage. It does record exactly what is said, providing the machine is working properly. As Woods stated, a tape recording captures what is said:

"not just snatches of it, or approximations to it,
but all of it, and as it was said, not as
remembered."

(Woods, 1986, p. 42)

Secondly, it is argued that using a tape recorder does not threaten or impede the flow, nor distort the conversation. (Woods, 1986, p.81). The value of a tape recording is that it can be a useful check on possible claims that an informant did not say whatever a researcher said that she did, as well as on the researcher's own questions. There was one occasion at Moorland when a dispute arose over the accuracy of certain statements recorded in some observations. As part of the process of checking whether accounts are accurate representations of what was seen and heard the researcher presented teachers with outlines of the observation notes. Teachers were asked to make comments and add any other information if they wished. The majority of the teachers made few if any comments and said that they considered the observations and conversations to be accurate representations. However, in the case of one teacher at Moorland the situation was different. She maintained that some of what had been recorded as

observed and heard in her classroom was inaccurate. The researcher had asked the teacher if she minded 'visitors' coming into the classroom, and noted down her reply.

"Mrs. Dale replied that at first she hadn't liked it, specifically when parents had come in to help during the previous summer term. She said that she had been apprehensive at first when she reprimanded children because she was afraid of what parents might think, that they might criticise her."

(Notes)

Mrs. Dale herself talked about parents as visitors. She wrote in response to reading that particular observation that:

"I did not say I didn't like the parents in. Why don't you mention that I found them a great help when the class was such a large size?"

(Mrs. Dale : Moorland)

However, Mrs. Dale did not mention this *at the time of the initial conversation*. Mrs. Dale went on to take issue with the fact that the researcher had not "... asked specific questions about parental involvement". The head at Moorland took up the issue. She had also read these 'confidential notes'. She wrote:

"I had asked Mrs. Dale and given her the option of not having parents in - other teachers had not wanted them and were not forced to have parents in. I think Mrs. Dale would have told me if she had not wanted them. 'You had to get used to it' - visitors all the time."

(Head : Moorland)

The last comment "you had to get used to visitors ..." was made by Mrs. Dale in relation to visitors in general and not in relation to parents at all.

Mrs. Dale also questioned the accuracy of another observation made in her classroom.

"During the morning I thought I heard Mrs. Dale ask a child which reading book he wanted. She told him that he was too young for it."

There was no opportunity after the event to question Mrs. Dale about it as a student was in the classroom at the time. When Mrs. Dale herself read

the notes she adamantly maintained that what was recorded had not been accurate.

"I never let children choose their own readers ... if you mean a story book from the book corner, then again there is not one children's book which I would say to a child 'You're too young to look at that book'. I cannot believe that I said this."

(Mrs. Dale : Moorland)

The researcher considered that the teacher had made the comment to the child. The head at Moorland did not permit any further communication with the teacher to sort the matter out. There can be a problem with recall and accuracy with taking notes instead of using a tape recorder, and one cannot prove what that one wrote was what was heard exactly. As stated earlier one of the main advantages of a tape recorder is that it records exactly what is said and heard and so avoids the problem of whether or not statements are true. However, there are disadvantages with tape recording. It still needs to be contextualised, which usually requires the addition of written notes and also a tape recorder only records what is said, not the written or visual aspect of classroom life. Nevertheless with hindsight, the researcher wishes that she had attempted to do more tape recording, particularly with the children. At the time it seemed more important to help the teachers to be reasonably content with the researcher's presence in their classrooms. In the particular situation there was some lack of confidence and a nervousness with one teacher. It was felt rightly or wrongly that in this school the use of a tape recorder in the classroom presented problems, and that the teachers at least could not be interviewed in this way. The children had seemed to enjoy it.

Therefore the keeping of written notes was the choice for recording data.

These written notes comprised, first, observations recorded on the spot, either in the classroom or elsewhere in the school. These were kept in small notebooks which could be kept in the pocket and were fairly unobtrusive to use.

Second, there were records of longer interviews kept in a rather large loose-leaf notebook. Third, there was a diary of personal feelings about the research. Lastly, there were expanded field notes which included the 'jottings' written up together with notes made during interviews. These expanded notes included not only more detailed observation notes and interview reports and any feedback from these, but also the researcher's own comments and queries. The expanded notes were filed according to school. Within each 'school' file the notes were further divided into classroom observations and *interviews with the teachers*, interviews with the head, staffroom conversations, and documents.

In some situations notes were made on the spot while in others notes were written down as soon after the event as was possible.

Notes were written 'on the spot' whilst observing in the classrooms and during longer interviews with teachers and head teachers. People were asked if they objected to the researcher making notes. No one raised any objections. Teachers in the classrooms said that after a few days they had got used to the researcher taking notes. In longer interview situations it was explained that the researcher wished to take notes on the spot in order to record as faithfully as possible what was said.

In the classroom writing was a usual activity so it was considered that taking notes in this situation would not be too obvious. However, there were situations even in the classroom when it was not possible to make notes on the spot, for example when helping the teachers and working with the children. There was not time to make notes in such situations. Woods made a similar point. He stated that taking notes in some situations might not be:

"... convenient [and] ... Especially if a participant, there may be no time and it may actually interfere with the interaction."

(Woods, 1986, p. 44)

There were other situations where it was considered that taking notes on the spot would be difficult, for example during assemblies and when listening to talk in the staffroom. Diamond stated that when the researcher was hearing "items of gossip, personal revelations and ... personal interactions" then these could not be noted down on the spot. (Diamond, 1970, p. 140). In such situations notes were made as soon as possible afterwards, either in the staffroom if one-one was there, or the lavatory.

Woods argued that taking notes in front of people in some situations might put them off and "... make them feel they are being spied upon or evaluated in some way". (Woods, 1986, p. 44).

This was one reason why notes were not made in the staffroom during break times.

Another reason for not taking notes in situations like assembly was that unlike in the classroom writing was not a normal activity. Note taking in such a situation would have been too obvious.

In the case of documents notes were either made on the spot or in some cases it was possible to take them home and make notes there.

Notes made during the day were usually written up the same day. In the case of the pilot studies this was done during the late afternoon and early evening because of other work commitments mentioned previously. At Moorland notes were written up in the evening. By the time the research began at Larkway it was considered that writing up notes took up time that could be better spent reading through the material and noting key 'ideas' which appeared to be emerging.

The early period of observation, as noted, was a 'mapping' operation. The first notes in each school, apart from initial interviews with the heads were a record of the layout of each school and the classrooms within it.

Plans were drawn of the school and classrooms showing the physical arrangement. Also a list of the materials and equipment in each classroom was made. These lists were to prove useful later. As shown in Chapter Seven a comparison between materials in the nursery and reception classes at Moorland indicated differences between the types of provision in the two settings. Attention was directed to these features at first rather than classroom events, or conversations.

During the early stages of observation attention was directed to trying to get an overall picture of the daily routine and to capture the flow of events. Early notes consisted of *jottings and abbreviated notes*. As noted earlier one of the problems in the early stages of note taking was that of over-compression and lack of detail, leaving out the actual words said by people and not making observations concrete. At Stone Street for example, a whole afternoon's observations were compressed into half a page of notes, as follows:

<u>Class:</u> Reception	<u>School:</u> Stone Street
<u>Date:</u> 14.3.80	<u>Time:</u> Afternoon
<u>Place where observed:</u> Classroom	

1.10 pm Children come in and sit down. Tchr says they have "fixed places". She calls register. Next all cls tld tht they hve to finish "Mother's Day" cards.

1.15 pm Tchr gvs out flwr shps/cards to 2 tables wrtng message inside crds - Another tble just startd making 'MD' cds - Childrn stick yellow flwr shapes on to crds - All cls engaged in some activity.

2.00 pm Chldrn go to hall - listen to radio prog with tp info.

2.45 pm Plytme.

3.00 pm Chldrn listen to story.

3.15 pm 'Hometime'.

In this extract no details of what either the 'teacher' or the 'children' said nor how they did the activities they were engaged in. Hammersley and Atkinson argued that when an observer compresses and summarises information not only is "... interesting detail lost [but also] vital information". (p. 133). They stated that the actual words used by people are of "analytical importance". (Hammersley and Atkinson, 1983, p. 153).

The problem of over compression did not occur only during the early period of observation. There were occasions later when it recurred for various reasons. King noted that the more he observed, then:

"... the more familiar and predictable events became, so much so that towards the end of the research what was at first interesting was sometimes rather boring, and I made notes shorter."

(King, 1984, p. 125)

In this research too as situations became 'familiar' there was a tendency to compress as noted in 'Observation'. Also on occasions the researcher felt tired which also affected the quality of the notes made.

Hammersley and Atkinson noted another feature of the early stage of recording, that "... the scope of the notes is likely to be fairly general". (Hammersley and Atkinson, 1983, p. 150). An early example of recording from Moorland indicates this. These notes were made in Mrs. Dale's classroom in late September.

"8.50 am Children enter clsrn in ones and twos. Some puzzles on one table - some chldrn do these - 3 others sand tray - few Wendy hse - some on mat with construction toys.

9.10 am Mrs. D tells chldrn to stp what they are dng - sit on mat.

Lined up for assembly. After assembly chldrn told to change for PE (shorts/plimsolls). When ready lined up. Tchr tlks to them about apparatus - v. clear - told 'must not go on the appts' until she tells them. Says this again. Children -- hall. Tchr says again not to use app. See some children tch app - 1k at Mrs. D Children told to find a space then run, wlk, jump up and down - then mk themselves smll - told choose piece of app [1st time have used it]. Tchr says to chld that prev wk - only lkd at app. This wk can go on it. Stress tkng turns. After 10-15 mins chld tld to line up by door and wlk back to clsrm and change on mat."

(Observation Notes : Moorland
Sept. 18th, 1980)

These particular notes continued for the rest of the morning's activities.

The notes represented an attempt to catch the flow of the morning's activities without concentrating on details, except for any particular emphasis by the teacher which stood out. It was felt important at this stage to establish the basic ordinary pattern of the day.

Some of the notes, even in the early stages, did contain though, not only jottings and abbreviated notes but sometimes snatches of conversation. An extract from notes written during the first half an hour of observations in a classroom at Briarfield illustrate this.

"Chldrn cum in frm plygrnd - hang up coats - sit down on lrge mat in front of a table where tchr is sat - Child asks tchr questn.

Child: Are we going to do our work now?

Tchr: Yes, its story and maths.

[O.C. ditt orgnstns to othr cls in the schl - 1 age

group only + day org diff - tlk to tchr about this.

The researcher asked the teacher about the organisation when the children were engaged in activities. Again no attempt was made to record all that was said, thus:

"Asked teacher -- morning - children play games
do art and craft
afternoon - "formal work"

Observer: Why not work in morning?

Tchr: When children first come to schl "... don't want to do formal work especially the boys."

(Observation Notes : Briarfield)

As stated no attempt was made to record every word spoken but what 'stood out'. The fact that in other classrooms in Briarfield a range of activities were observed to run concurrently made the comment by the child cited above stand out as important to record.

Later, as noted in 'Observation', attention became more focused either on particular periods of the day, such as the start, or on particular activities such as 'choosing' at Moorland, or on situations such as assembly. Hammersley and Atkinson argued that as such focusing occurred the nature of the recording altered to include more verbatim speech. (Hammersley and Atkinson, 1983). In this research the notes did become more detailed concentrating particularly on what was said. Thus for example on one occasion in the nursery at Moorland the focus was on the nursery teacher and the way she talked to the children and what she said was written down verbatim. The notes were made during the afternoon, thus:

O.C. 1) Use of endearments Nursery Teacher -- Child: Sit down
poppet.

Nursery Teacher -- Children: Don't
throw sand boys. You're being v.

- 2) Teacher does not normally raise voice silly. [Mrs. Raynor raises voice - boys look at and stop]
- 1) Nursery Teacher - child Goodbye honey.

(Notes : Moorland Nursery)

The above extract of the notes made during the period notes indicate too that the researcher was also adding comments as well as recording the actual words said.

In another example taken from Class 3 at Moorland the teacher was doing oral work with the class. Oral work was observed in detail because it appeared from previous observations to be a regular event and also the teacher had stressed its importance.

"... whole cls on mat in front of tchr - Easel with chart on it - letters of alphabet.

Tchr: Now look at the easel everyone. [I Spy]

 I spy with my little eye smthg beg with 'h' [said slowly].

Child 1 Hand. [Another child is making faces*]

Tchr: [to child*] ... Stop showing off. I shall put you in the corner if you don't stop. [She asks another child with hand up]

Child 2 Helephant.

Tchr: No, that begins with 'e'.

Child 3: Hair.

Tchr: No, not hair. I will give you a clue, it's red ..."

(Observation Notes : Class 3 Moorland)

It was relatively easy to record what was actually said during this type of exchange where the teacher was talking to the class. This was also true when the focus was on situations in which the teacher was talking to a group or individual children.

In this research there was not always a clear distinction between 'early' and 'later' note taking. As indicated even the early notes included fragments of conversation. In the later stages there was some variation in the nature of what was recorded. Even the notes at this stage contained some general description, sometimes interspersed with snatches of conversation or the detail of a particular incident. One set of notes for one period of observation could contain all these items together with the more detailed recording of interaction already noted. Hammersley and Atkinson stated that unless the focus of a piece of research was very narrow, then some detail would be sacrificed for increased scope and so there would be variations in detail. (Hammersley and Atkinson, 1983). This seemed true in the case of this research. However, there did seem a good reason for sometimes recording general details even at a later stage, for example in order to see if the 'routine' in a particular classroom had altered or not.

It was stated earlier that handwritten notes were also made during the longer interviews with heads and teachers. These notes made 'on the spot' were made in a similar way to those made during observations. They contained details of who was being interviewed, when and where the interview took place. For example, when the nursery teacher at Moorland was interviewed these details were recorded:

Nursery Teacher

Moorland Nursery

Date: 14.10.81

Place: Sitting in the nursery

For longer interviews there was normally an 'agenda' which was written at the top of the page. In the case of the above interview with the nursery teacher this was as follows:

- 1) How the nursery teacher sees her task/aims in the nursery.
- 2) Are they different to those of the reception teachers in her view.- How does she see the nature of the parents at Moorland?

Each theme for discussion was also allocated a separate page.

The actual notes consisted of jottings, key phrases and where possible the actual words used by the interviewees themselves. An extract from an interview with the nursery teacher at Moorland illustrates this. Here she was asked to describe the parents at Moorland.

"Mrs. Raynor sees the area as being more 'deprived' than others she has taught in - sees many of the parents as being what she calls "emotionally starved" but "not necessarily materially". Believes that "the women in particular "have a difficult life - seem to bear most of the worries on their shoulders" - consequently many young mothers 'look older than their years' ..."

(Interview Notes : Mrs. Raynor)

While it was not possible to write down all the teacher said the researcher did try to concentrate on the key phrases when she was talking. In the words of Sudnow the written notes were as near "as on the spot handwritten recording will permit". (Sudnow, 1967, p. 7).

There may be a tendency to try and summarise as Hammersley and Atkinson (1983) point out and it is possible to forget items of conversations. (Hammersley, 1985). In this research there was to a certain extent a problem in recording exactly what was said but the problem was overcome in certain ways. First, key phrases were noted down rather than an attempt to write down 'everything'. Secondly, as noted in the section concerning interviews, a series of interviews was conducted over a fairly long period, at Moorland at least. At Larkway, too, it was possible to go over details of interviews with teachers. This meant that the same topic or an item which did not seem clear could be raised again rather than relying on one

instance. Thirdly, in the case of Moorland interviews, notes were shown to the teachers so that they could check on the accuracy of the written statements. As stated earlier there was only one occasion when a teacher questioned the truth of such statements.,

After each interview the written notes were read through and any 'impressions' about the interview noted down. Thus after an interview with the deputy at Rushside it was noted that:

"I felt that this interview went really well today. The deputy said that she enjoyed talking to me - said that it was nice to have the chance to talk about what teachers do - nice to have someone who listens"

(Impressions of Interview :
Deputy Rushside)

In contrast after an interview with Mrs. Dale at Moorland it was noted that:

"I found it very hard to get any conversation going - felt very inhibited - cannot really pin down why but feel that Mrs. Dale is suspicious of me and the research."

(Mrs. Dale : Moorland)

Woods stated that the written notes should include details of "impressions of the interviewee's disposition and attitude towards the research". (Woods, 1986, p. 81). However, in practice it was not always easy to assess an interviewee's attitude to the researcher especially when as in the case of Mrs. Dale, *no direct comments were made*. At Rushside one teacher made it clear to the head that she did not 'want me in her classroom'. In such an instance the teacher's attitude was clear.

It was stated earlier that notes written on the spot were written up later, usually in the evening. It was seen as important to do this in order to minimise loss of memory. (See Lofland, 1971). As Woods pointed out too, if one waits any longer then the following day's experiences will crowd out earlier memories, "... promoting confusion and possible loss of important data". (Woods, 1986, p. 45). However, it was also true that even days after the observation or 'interviews' an incident might suddenly be

recalled on reading through the notes again.

The written up notes consisted of an expanded account of what people were doing, when and where, and as noted, where possible the actual words that were used. In other words they were similar to what Lofland termed a "running description", (Lofland, 1971, p. 105), or what Burgess referred to as a "substantive account". (Burgess, 1981, p. 76). These expanded substantive notes form the body of the thesis so only one example is used here just to show how the jottings and abbreviated notes were developed.

"Observations Class 4 . Monday Morning

Classrm 8 8.45 am I went into C4 asked Mrs. Neaves if I could

8.50 am come in for morning. Says all right. Childn

come into clsrm in 1s and 2s - some with

parnts. Most sit down - draw pctrs. Tchr

asks three chldn to lk at some wds on wd chrt

Mat: ... After about 10 mins Mrs. Neaves calls

9.00 am everyone on mat - tlks abt a chls brthdy - lk

at his crds he has brought. Then says - "Talk-

ing times over it's listening tm now" Tells

thm going to listen to radio prog 'Poetry

Corner' - various poems about Autumn - Autumn

colours. When finished tchr asks questions.

Tchr: Did you like the poems?

Childn Yes. [Not many ans.)

Tchr: I'll ask again and you can answr prop."

(Asks again).

Chldn (All) yes ...

(Abbreviated Notes)

The written up notes were written as follows:

Class 4

Time: Monday morning

Date: 29.9.80

Place: In the classroom

I went into Class 4 today. I asked Mrs. Neaves if I could come in to her classroom for the morning. She replied that this was all right. (O.C. This was not the first visit). Mrs. Neaves was laying paper and crayons on the tables. The children began to come into the classroom in ones and twos, some with parents and some alone. Most of the children sat down and began to draw pictures. Mrs. Neaves called out three children and asked them to look at some words on a word chart. (She holds up several cards and asks children to say the words written on them).

... After about twenty minutes the teacher calls out all the children to "come and sit on the mat". She talks about one of the children's birthdays. Then she says:

O.C. Rule Establishment
new class. See also
Mrs. Knowles.

"Talking time's finished. It's listening time now". (Says that it's time for a radio programme 'Poetry Corner'. The children listen to a poem about Autumn. Various autumn colours are mentioned. After it's finished the teacher reads through the poem, from the teacher's manual and asks the children some questions about it, and if they have enjoyed it.

Teacher: Did you like the poem?

Children: (few reply) Yes. (Said by one or two children).

O.C. What counts Teacher: I'll ask you again and you can answer
as a properly ... (looks cross)
proper Children: (All answer this time) Yes.
answer?

(Class 4 Moorland : Written up
Observation Notes)

The written up notes included thus abbreviated notes written up in full, together with anything else which was recalled whilst 'writing up' the notes. They also included key ideas which seemed to strike the observer. These were underlined and 'observer comments' (O.C.) added. These key ideas were in fact analytic notes which are discussed in the section on analysis.

Thus the full notes contained more than a descriptive account. They also could include some analytical and methodological notes, together with personal feelings about the research. These last were recorded in a diary.

As noted, after reading through interview notes, immediate impressions about the way the interview had gone and the interviewee's reactions were written down. These notes were incorporated into the 'written up' notes.

During the process of writing up observations and 'conversations' further methodological comments were added. Some concerned the nature of the field role, and the way observations were carried out as the following example shows. These were written after the first few visits to classrooms at Moorland.

"In the classrooms my initial method was non-participant observation. This was done deliberately in order to give the teachers a chance to accept my presence and myself to get an overview of the classrooms. ... However, I did not discourage the children from asking questions or asking for help. It seems to be a good way of finding out about the nature of the children's activities and as a means of reducing any possible strain on the teachers during what I see as a crucial stage of the research."

(Field Notes : Moorland 1980)

The second example also concerns the nature of the field role, and again comes from the Moorland notes.

"Today I asked Mrs. Martin to explain the daily routine. She replied, "Well you know what it's like. You've seen it". She also added, "You've been a teacher so you know".

O.C. Adopting naive position (see Strauss, 1969) does not necessarily work. The teacher knew that I had been a teacher - difficult therefore to adopt naive position. I asked the same kind of question in the pilot schools and the teachers responded well. I had not observed all day so genuinely did not know what was going on."

(Field notes : Class 3 Moorland)

Sometimes the comments included reflections on how questions were asked as in the following example from Larkway:

<u>Reception Teacher:</u>	I work an integrated day.	<u>O.C.</u>	The teacher introduced the
<u>Researcher:</u>	What do you mean when		term integrated
	you say an integrated day?		day. I wanted
<u>Teacher:</u>	What do you think it means?		the teacher to
<u>Researcher:</u>	Well, different activities.	<u>O.C.</u>	say how she saw
<u>Teacher:</u>	Yes, it does mean that but		her approach.
	I really think of the		It is sometimes
	integrated day in terms of ..		necessary to give
			teachers <i>some-</i>
			thing to encourage
			them to talk. At
			Moorland I avoided
			giving personal
			opinions, but I
			think in retro-
			spect this put
			some teachers off.

(Field notes : Larkway, 1982)

During the main period of field work, as noted, methodological comments were incorporated into the field notes. Having completed the field work it was possible to reflect back on the earlier reading in 'Interpretive Methods' in the light of experience. The ideas became more real. At the same time the reading in turn enabled reflection upon the fieldwork experience itself. So methodological notes were made not only during the fieldwork but also after it when reading and re-reading

methodological texts. Such notes were kept in a separate file and contained notes made about the various methodological texts and the researcher's comments on those related to experience during the fieldwork itself.

Thus for example some notes were made on an article by Dean et al. The article was concerned with the establishment of field relations. They noted how at first the presence of the researcher (outsider) might have an inhibiting effect on behaviour. Their actual comment was recorded plus an idea of the researcher.

"At first the presence of an outsider may seriously inhibit behaviour. But as he becomes fully accepted, others will behave quite spontaneously in his presence."

(Dean et al, 1969)

O.C. However, it would be an oversimplification to attribute the researched inhibited behaviour to only the presence of the observer. There may be other reasons. For example in one school visited previous circumstances - the nature of the previous head who was considered authoritarian by the staff caused some antipathy towards any 'strangers' - perceived as a threat to their privacy but not simply because of 'being a researcher'. The two things interrelate."

(Notes on Methodological Texts)

The above notes thus indicate that reflection on the fieldwork is a continuous process.

This part of the section has attempted to show the main means of recording data used in this research and the advantages and disadvantages of the form that was used, in comparison with other means such as the use of a tape recorder. It has indicated the nature of the notes made. However, as stated, one aspect of recording data was not discussed, that is the writing of 'analytic notes'. As stated these analytic notes or key ideas are referred to when discussing the process of analysis in the next section.

The final part of this section looks at the issue of validity and triangulation.

5. Validity and Triangulation

This final part of the section discusses the problems of efforts to produce a valid account. The actual concept of validity has been discussed in Section Two of this chapter. Furthermore, when describing 'gaining access' and 'developing field relations', and when recounting the actual methods of data collection and recording in this section, various factors which can affect validity have in fact been considered, though not stated directly in terms of this concept. Therefore in this part of the section, the features which may have affected validity in this 'interpretive' account are restated briefly, together with attempts by the researcher to overcome some of the problems.

As far as this research is concerned, the audience for whom validity is considered important are the academic community and also as far as possible the teachers concerned. However, a sociological interpretive account is not always likely to be fully acceptable to the latter, because the researcher's gloss is almost inevitably placed upon the actual data. Hence the basic problem in seeking to produce a valid account is two-fold. It is whether the methods used have produced 'truthful' data, and also whether the inferences drawn from the data by the researcher are justifiable.

As noted when discussing the concept of validity earlier in this chapter, there are two general aspects to this, external validity and internal validity. The former relates to the degree to which results from one piece of research can be generalised to other situations, while the latter covers most of the issues which will be discussed in this part of the section. Basically it concerns the issue of how far and in what ways the researcher affected the collection of the data. This includes both the presentation of the researcher and the research, and the methods used and the researcher's competence in using these, anything in fact which may have 'contaminated' the data.

External validity involves, at one level the issue of representativeness, as well as the actual findings.

This research although not overly concerned with the issue, did try to cover, even in a small way, a wide range of schools, as noted in Section Four of this chapter, and in the next chapter. Though no 'matched' sample of teachers was made, the teachers seen covered a range in terms of age and experience, which did not appear outstanding or unusual, in the researcher's eyes, for similar schools.

Another aspect of external validity involves the degree to which the research relates to other similar work. As indicated there was an attempt to place this particular research on infant schools in the context of other research on primary schools. This research for example in looking at the type of 'approaches' used by infant teachers, and aspects of organisation in the infant classroom in a sense builds on to other research which looked at similar aspects, such as that of Sharp and Green (1975) and King (1978). It also does not contradict other research on 'primary' schools such as the 'Oracle' studies, which as shown, have indicated that teachers' practice is not necessarily as 'progressive' as the rhetoric suggested. This research also accords with the view of Richards (1979) that there is no one belief system in operation in primary schools. This research indicates that this is also true of 'infant schools'. On another finding, this research indicated that some teachers in an E.P.A. school defined the children they taught in certain ways which appeared similar to those used by teachers in the E.P.A. school observed by King. However, from one school it would be unwise to generalise about 'most' E.P.A. schools. In this particular respect this research might also have a bearing on other work, mainly at secondary level which has dealt with under-achievement.

The first part of the sub-section examines the ways in which presentation of the research, and 'presentation of self' may have affected validity and the ways in which the problems were overcome. The second part looks at how far previous research done in the main school could have had an effect. The third part of the sub-section looks at the effect

of personality on data collection and so validity. The fourth part looks at the effect of the researcher's role. The final part of the sub-section concerned with validity looks briefly at the methods of data collection as a possible influence and the means of trying to overcome some of the problems through the use of triangulation.

The first part of the sub-section looks at the ways in which presentation of the research and the ways in which the researcher presented herself may have affected validity.

Presentation, as noted involves two aspects, presentation of the research and presentation of self. Both aspects involve an attempt to influence the respondents' perceptions of the researcher in a favourable direction. As stated these two concerns are not limited to initial access but continue throughout the research.

As noted the presentation of self and the research is a difficult process since it involves the perceptions of the researched as well as those of the researcher.

First, in terms of this research, it is not possible to assess how far the way in which the research was presented was a problem.

An attempt was made to present the research in a way that was acceptable to the teachers in the schools. In the pilot schools this presented no difficulties. The explanation to the heads and teachers about the purpose of the research was apparently found acceptable. Teachers did not appear to find answering questions a problem and chatted easily to the researcher. Of course the time element was an important factor in the pilot schools. The researcher was not in them long enough for any personal antagonism to develop. Therefore nothing in the pilot schools prepared the researcher for the 'problems' which were to be encountered at Moorland.

At Moorland, as stated, due to the extended nature of the field-work,

a more detailed explanation of the proposed research was required by the head teacher. The researcher considered that this initial explanation was adequate according to advice in research texts, being 'honest' but not too specific. It was considered at the time that providing too much information might affect the behaviour of the respondents in that they then might have tried to give the researcher what she wanted rather than their own opinions. It became apparent from an informant's comments at a fairly early stage of the research and from comments soon afterwards by the head that the 'initial explanation' may not have been understood, and that the initial explanation of the purpose of the research given to the head was not in fact passed on to the teachers at all. However, it was difficult to appreciate this problem particularly when only one of the Moorland teachers actually asked the researcher personally about what the research was about. In the case of the informant the researcher was never certain whether the comment was genuinely derived from the teachers, or whether she was merely echoing the views of the head with whom she was friendly and met socially.

The researcher having been alerted to a possible problem did subsequently offer a written explanation to each teacher with offers of further explanation if required and if the teachers wished to ask any questions about it. This offer was never taken up, therefore the researcher assumed that the problem had been resolved. Only near the end of the field-work at Moorland did the head reiterate that:

"the teachers have never understood what you were doing.
They thought you wanted to know about the integrated
day."

(Head teacher : Moorland)

Only then did the researcher realise that the problem if it had existed, had not really been tackled. In retrospect the head and the teachers should have been asked more directly if they now understood. Also ethnographic research and its nature should have been discussed. It became

evident over the issue of the questionnaire that the head had a totally different view of research from that of the researcher, and the teachers were more used to research which had more 'practical relevance', such as 'language in the classroom'. It still remains, however, difficult for the researcher to 'know' if the head genuinely expressed the view of all the staff at Moorland in the absence of direct comments from the teachers themselves.

At Larkway, as noted, a more detailed explanation of the research was given to the staff. By this time the researcher had 'learnt from experience' and was more aware of the difficulties in this area. It is not clear, however, whether any less detailed explanation at Larkway would have resulted in less co-operation from the teachers though it did seem important in gaining initial access. There was quite simply a wholly different atmosphere in the school compared to Moorland.

The second aspect of presentation, which has already been referred to, is the presentation of self. As stated the researcher told the teachers in all the schools that she had been a teacher. This was done in order to 'establish rapport' with the teachers. However, as also noted, this did prove to be a disadvantage to some extent, as evidenced by the fact that one teacher, Mrs. Martin, considered it odd to have to explain events in the classroom to someone who had 'been a teacher'. As stated, however, this problem was overcome by explaining to the teacher that the researcher had never taught in an E.P.A. school.

Presenting oneself as a teacher may thus affect how much information teachers in a school provide. It is not just whether experience as a teacher colours the perceptions of the researcher, whilst observing (although this is an important issue) but the way in which people view the researcher as well. Such perception may be favourable or unfavourable, depending on the previous circumstances. Presenting oneself as a teacher might be an

advantage in some situations but a hindrance in others. There are no hard and fast rules, and this applies to other aspects of presentation of the self and the research.

This ends the discussion of 'presentation of the research' and 'presentation of self' on validity.

The second part of the sub-section looks briefly at another factor which could have affected validity, the effect of previous research having been carried out at Moorland.

Another factor which could have affected the validity of the data obtained from Moorland, specifically, was the fact that some research had already been carried out in the area, concerning parental involvement with the school. This research as previously observed, had been carried out by a department with which the researcher was connected. The research was not viewed favourably by the Moorland staff who considered that it had been "a waste of time". The fact that the researcher was connected to this department may have influenced the way in which the teachers saw the present researcher and the research and therefore may have affected their responses. It was noted previously that at the time the full implications of the relationship between the department and the school were not realised, and also as stated, there was a more immediate concern to 'get started' since difficulties had already been experienced in finding a school in which to do the research. This meant that at the time this particular issue was suppressed from consideration. A degree of suspicion by the researched was seen as 'natural' at the start of the research in any case, so it is difficult to assess whether there was any extra. The researcher was simply not experienced enough to be able to recognise this.

The third part of the sub-section looks at another factor which seemed important in relation to validity, that of the influence of personality on data collection. The issue was only briefly returned to in the

previous part of the section when it was stated that the amount of help given to teachers was affected by the degree of 'friendly relations' achieved with them. The issue of personality was not considered prior to the field-work but subsequently became important during it particularly at Moorland. Doing field-work teaches the fieldworker much about him/herself. Sometimes the researcher learns things which are difficult to face up to.

Data collection, whatever the method, is influenced by the ability and personality of the researcher. However, in an interpretive study the personality of the researcher who is much more directly involved with the subjects of the study over a greater length of time is likely to have a greater effect. McCall agreed that personal characteristics can have a negative effect on relationships and may lead the researcher to avoid certain people. (McCall and Simmons, 1969). Thus there can be a problem of bias which may affect data obtained from both observations and interviews. The consequences can be quite severe for data collection. Due to personal characteristics the researcher may fail to ask certain questions and misinterpret the information received. The degree to which the researcher 'gets on with' or 'likes' the people he or she is studying may also affect the weight that is given to their responses and perhaps colour the researcher's perceptions of the data. Of course, it is also the case that the respondents themselves will either like or dislike the researcher and will consequently perhaps either give or withhold information.

In this research it was found relatively easy to establish friendly relations with most teachers, both in the pilot schools and at Larkway. However, with one or two teachers at Moorland, one of whom was the head, this was definitely a problem. Since Moorland was the main research school it does seem important to look at the issue. If the school had been larger, this may have mattered less. At Moorland because the staff size was small, six teachers, including the head and the nursery teacher, the fact of not getting on with two of the staff does seem a significant issue.

It, was easier to relate to those teachers who seemed more approachable, who seemed interested in the research, and who either approached the researcher unsolicited, or as in the case of the deputy at Rushside stated that it was 'nice' to have someone to chat to and listen to what teachers have to say. In general, a researcher is likely to find it easier to approach some people rather than others and establish relationships with those who seem to appreciate their efforts. (See Pye, 1978).

The researcher found it very difficult to develop other than a superficial relationship with Mrs. Dale, one of the teachers at Moorland. It was hard to talk to *this teacher and the researcher felt ill at ease in the classroom*. Also it appeared to the researcher that she was disliked by Mrs. Dale though nothing was said which might suggest this and she said when asked that she did not mind the researcher in her classroom. As noted in the previous section this teacher was the only one to be 'upset' by the researcher's records of classroom observation. As stated she went to the head and the researcher was prevented by the head from discussing the matter with Mrs. Dale herself. In retrospect it might have been better to have confronted the teacher and tried to sort the matter out and brought the conflict into the open. To a certain extent, however the researcher's own personality characteristics were a factor in failing to pursue this issue as the researcher was afraid of confrontations of this kind. With greater experience a researcher may find it easier to cope better with these types of situations. It does depend, however, on the degree to which a researcher has a general experience of social situations. At the time of doing the field-work this researcher lacked this particular type of 'social experience'.

The situation in Mrs. Dale's classroom was made more difficult by the presence of another student doing some language research. This student appeared to find no problem in communicating with Mrs. Dale and in fact saw her 'socially' outside school hours. It was both disconcerting and annoying

to find someone else being able to establish a relationship like this.

It is at least possible that observations in Mrs. Dale's classroom were at least coloured by these feelings. However, the researcher made great efforts not to allow feelings to show and did not give up trying to establish a relationship with this teacher, or doing observations.

In the case of Mrs. Warner, the head teacher, the degree to which the personality problem may have affected events was not clear. In retrospect, however, there may have been a clash of personalities. Outwardly the head teacher seemed friendly enough and helpful. She provided a great deal of information, often unsolicited, as noted previously. The researcher was able to talk to her about sensitive areas such as, for example, whether children at Moorland of a particular age would have made similar progress to children of a similar age in another school that she had taught in.

There were, however, differences in personality. The head teacher at Moorland appeared confident, outgoing and forceful, whilst the researcher was more reserved, and unless provoked, was rather quiet and lacking in confidence. At the time such differences did not appear important. However, in the light of certain events which occurred near the end of the fieldwork at Moorland they do have significance. As noted previously, the head considered that the teachers might prefer a questionnaire to informal interviews. The researcher did not entirely believe that a questionnaire was appropriate in the circumstances. Also given that there would be little time to plan it, she did not believe that a 'good' questionnaire could be presented. The researcher, however, gave way on the issue. A more forceful personality might have argued about it and have suggested meeting with all the teachers at this point. This may have affected the attitudes of the teachers to the research and the way the questionnaire was administered, if one had then been considered by them to be useful.

As stated, it took a long time to establish a relationship with another

teacher at Moorland, Mrs. Martin. She appeared to be a very quiet person and it was sometimes difficult to get a conversation going with her. However, when asked if she objected to the researcher's presence in her classroom, Mrs. Martin said that she did not mind at all. She said that she had been nervous at first but that after a few days she had become used to the researcher's presence and sometimes even forgot that she was there. It was noted earlier that a student in Mrs. Dale's classroom was much friendlier with her than the researcher was. The same student, however, said that she found it difficult to make any progress with Mrs. Martin and in fact did not particularly like her. A student on teaching practice in the school also said that while she enjoyed being in Mrs. Dale's classroom, she 'hated' it in Mrs. Martin's classroom because she thought that this teacher was quiet and did not talk to her much. The researcher while finding it hard to get Mrs. Martin to talk about some of the issues, nevertheless did like this teacher, and given the researcher's own personality, not shy but not wholly outgoing either, there was perhaps greater compatibility with this teacher. The researcher did persevere in Mrs. Martin's classroom. The main tactic adopted was to wait for the teacher herself to bring up issues, which in time she did. Of course, this was very time consuming, but the researcher considered that with this teacher this was the best way of getting her to 'open up'.

The point of the above discussion has been to show that different personalities may obtain more or less information from some people than others. This was demonstrated for the researcher who was fortunate in having someone to compare herself with. The student in Mrs. Dale's classroom clearly had a better relationship with her than the researcher and so was able to gain access to more information than the researcher in certain areas, particularly with aspects of life outside school for example. Yet as shown above it was a matter of swings and roundabouts because the same student did not establish this type of relationship with Mrs. Martin whereas the researcher

did. There is no way in which one person is ever likely to like or be liked by all those with whom there is interaction, but being aware of the possible bias does make the researcher more careful. The researcher tried to keep her likes and dislikes to herself and tried to appear cheerful in the school. However, there is the possibility that subliminal clues might be transmitted to the researched as to the real feelings of the researcher. This of course can operate in reverse as well and may alter the behaviour of the researcher towards the researched.

There is of course, the possibility that the nature of the research done by the researcher and the student also affected the relationship. 'Language' in the infant school is seen as a valid concern by infant teachers, whereas this may not necessarily be so in the case of 'sociological' research.

The next part of the sub-section examines the role of the researcher in relation to validity. The 'role of the researcher' was referred to earlier, in Section Five and so is only briefly discussed here. It was noted that the role adopted may affect validity in that what role is adopted will affect where the researcher can go, with whom he interacts, what the researcher can talk about, and the degree of access to information. In this sub-section these issues are examined with relation to the role adopted in this research, which as noted, was that of 'involved observer' and notes problems there these were experienced.

Woods argued that being a full participant enabled "access to all groups and activities". (Woods, P., 1986, pp. 33-7). However, the role of 'observer' also seems to permit this too. As noted the researcher was able to wander freely round the schools observed, although head teachers did take the researcher to particular classrooms in the first instance. This freedom of movement enabled the researcher to talk to a range of people. Thus at Moorland the researcher spoke to the head teacher, teachers,

children and other visitors to the school such as health visitors, a social worker, an educational psychologist and an H.M.I. This enabled the researcher to look at particular issues from a range of standpoints. Thus, for example at Moorland, when examining perspectives on 'the way children and parents are at Moorland' the researcher not only collected teachers' views but also those of the health worker, social worker and educational psychologist, who came to the school. Thus a more 'rounded', less biased picture, was developed of this aspect.

In the role of 'involved observer' no difficulty was found in gaining access to a range of activities. The researcher was permitted to attend staff meetings at Fairfield, Moorland and Larkway, and assemblies in the schools visited, and was also invited by the head teacher and teachers to have dinner with the children and attend a parent/teacher workshop, as well as visit the teachers' classrooms. As a teacher a researcher would not necessarily have had access to other teachers' classrooms in a way that was possible in this research, unless teaching part-time. Also, other staff might object to another 'colleague' seeing 'what goes on' in their classroom. Having such access also meant that a more rounded picture was built up of 'life' in the infant schools visited.

There are problems, as noted previously, with certain roles. One of the dangers of 'non-participant' role is that the researcher remains an 'outsider'. Schwartz and Jacobs considered that where the role adopted is a "passive" one and where the researcher does not interact a great deal with the researched then the researcher is likely to remain on the outside. They considered that if this role was maintained then "it becomes difficult to evaluate this effect on the situation". (Schwartz and Jacobs, 1969, p. 97).

It is not altogether clear what Schwartz and Jacobs meant by this comment. What they appear to be saying is that if a researcher maintains

an observer role in which he/she observes but does not interact with the researched then he/she will be unable to assess what effect they are having on the researched. As noted, earlier only one of the teachers apart from the head at Moorland openly criticised any aspect of the research. The observer was not really aware of what teachers did think about the research until the end of the research and even then only through statements made by the head.

The role of observer can be misinterpreted and misunderstood, and create suspicion. As noted earlier the head teacher at Moorland expressed some concern about the nature of the research and said that the staff saw the researcher as a spy and like an H.M.I. There were occasions where the researcher sat at the back of the classroom and watched and sometimes 'recorded' what was going on and it is possible that this could be interpreted as 'spying', although every attempt was made to inform the teachers of what had been observed. Also in the staffroom there were several occasions when the researcher did not talk much. This could have been misinterpreted. However, as noted, not all teachers talked much in the staffroom. Some were very quiet. So the researcher was not really behaving so very differently from some of the teachers.

There is, however, the possibility that the observer was seen as a 'spy'. If this was true, and it was not clear at the time if this was the case or not then this may have influenced the Moorland teachers' attitudes towards the researcher and the research and what they were prepared to talk about. However, no such problems were encountered at Larkway where a similar role was adopted so it is difficult to say whether it was the role adopted which was an important factor, or some other, such as the 'way' in which the research was presented initially, for example.

McCall and Simmons argued that the role adopted can affect what a researcher can talk about. As a teacher, for example, there may be things

which are discussed as part of the daily life of teachers such as views about the head, or even about colleagues which they might not discuss with strangers. In this research there were sometimes incidents observed which the observer considered could not be discussed. For example on one or two occasions the head at Moorland was observed 'standing in' for a teacher while the latter took another group of children for an activity. During these periods the children behaved in a completely different way to the way they did when their own teacher was there. The teachers at Moorland rarely made comments about the head in front of the researcher. It may have been possible if the researcher had been 'teaching' in the school to have brought up the issue even if only with one or two teachers. It should be pointed out that a 'teacher' as 'researcher' might not necessarily have been in the position to observe such incidents in the first place, unless the teaching was on a part-time basis.

Woods argued that adopting a participatory role enabled the researcher to "penetrate the experience of others" in a group. (Woods, 1986, pp. 33-7). However, being a non-participant observer does not necessarily prevent this. For example a teacher at Fairfield said that it was nice to have someone who listened to what teachers had to say and spoke at length about different issues. At Larkway, one teacher spoke about her family and what it was like to bring up a family and teach as well. It could have been that these teachers felt less 'threatened' by a 'stranger' and someone not directly involved with them and so 'opened up'. It has been the personal experience of the researcher that relative strangers have often talked about personal aspects of their lives.

The relationship between the role adopted and validity is not a straightforward one.

The sub-section thus far has considered the ways in which the researcher can affect validity. However, other factors, namely the methods of data

collection and sampling procedures can affect validity as was noted in the previous section. This final part of the sub-section is concerned to briefly restate the issues and show what efforts were made by the researcher to produce a valid account. This involves the discussion of 'triangulation', a term which was briefly referred to in the previous section.

The issue of validity in relation to methods of data collection concerns how far statements made about the results of observations, interviews and use of documents actually represent what was said and done and people's 'real' practices and views. In other words it concerns the effectiveness of the methods used, and whether these are 'appropriate' for getting at certain types of information. Validity in relation to data collection also concerns whether there is sufficient evidence to justify the conclusions drawn. The context in which the methods were used is important, as noted in the previous section. This includes the manner in which the methods were used, who was observed and interviewed, the time when observations and interviews for example took place, and which places were observed. As noted, these factors are associated with sampling and the question of representativeness.

In order to overcome the problem of validity cross checking was carried out. The name given to this process is called triangulation. Hammersley argued that triangulation was one of the main means of assessing the validity of ethnographic data. (Hammersley, 1979).

The term triangulation refers to a form of measurement used by navigators and surveyors who "use ... several location markers in their endeavours to pinpoint a single point or objective". (Cohen and Manion, 1987, p. 254), and in sociological terms it refers to the collection of accounts from different points of view (p. 235). Eliot's interpretation of triangulation was more precise. He stated that in the context of teaching:

"Triangulation involves the gathering of accounts from three quite different points of view, namely those of the teacher, his students and a participant observer."

(Elliot, J., 1976)

This was one of the first references to triangulation that the researcher came across during the early stages of the research. Another reference which was used was Denzin. He referred to "multiple triangulation". (Denzin, 1970, p. 42). This included data triangulation, methodological triangulation, investigator triangulation and theory triangulation. Data triangulation was further yet divided into time, space and person triangulation. The first, time triangulation is:

"... where the researcher attempts to consider the influence of time using cross-sectional and longitudinal research designs."

(Burgess, 1984a, 145)

Space triangulation involves researchers engaging in "some kind of comparative study" (p. 145) and person triangulation which can be at three

"levels of analysis: (i) the individual level, (ii) the interactive level among groups, (iii) the collective level."

(p. 145)

Data triangulation thus seems to be concerned with sampling. Methodological triangulation was divided into two aspects 'within method triangulation' and between method triangulation. The former referred to the "same method [being] used on different occasions" and the latter to where "different methods are used in relation to the same object of study". (Burgess, 1984a, p. 145). Investigator triangulation involved the use of more than one observer to look at a setting and theoretical triangulation involved the use of different theories in relation to a situation.

In this research some of the above forms of triangulation were used. First, an attempt was made to collect data from three points of view in the way suggested by Elliot (1976). Thus, at Briarfield, as noted in the

previous section, the researcher observed children in one class doing a range of activities. Some children came up to the researcher and said 'unsolicited' what activities they had done. The researcher then asked them if they could 'choose' what to do. Later during the same day the researcher asked their teacher if children could choose what they could do. Her reply confirmed the researcher's own observations and those of the children spoken to.

Secondly, data triangulation was used in that the researcher collected data over a long period of time, a school year, and also looked at different periods within a school day. Data was collected from the same group at different times in order to see if ideas changed or remained the same. Looking at what teachers and children did over a period of time also enabled the researcher to see how many times actions appeared in the respondents' behaviour. As noted, in the previous section, however, it was only possible to observe in the pilot schools for part of a day, and each school was only visited for a short period. This meant that only limited information was obtained on some aspects. However, as noted, an attempt was made to overcome the problem of time by asking teachers about the period that had not been observed.

As noted, an attempt was made to 'observe' and talk to as wide a range of people as possible. In the pilot schools this was limited by time available. At Moorland, the researcher talked to teachers, the head, children, a social worker, an educational psychologist, and an H.M.I., and at Larkway to the head, teachers and children. However, parents were not interviewed due to lack of time. On the whole the researcher tried not to limit interaction to particular groups, that is just the teachers and so produce a biased account. At the same time, the researcher wished that more time had been given to the children's views, although the research does attempt to show what they did, and has included examples of conversa-

tions with children where possible. Reliance was not placed upon one person's account of an event or issue. Thus with regard to the 'nature of Moorland children', and the area, the researcher obtained accounts from the head, teachers, social worker, and a previous head.

As well as getting accounts from as wide a range of people as possible, the researcher observed in several schools, although in some it was only for a short period, and within schools. With regard to the latter, the researcher visited classrooms, sat in the staffroom during break time, went to the hall, and strolled around the corridors and playgrounds. Within each of these settings a range of activities was 'sampled'. Thus, for example in the hall the researcher attended assemblies, watched, and sometimes participated in P.E. and had dinner with the children. In the staffroom the researcher, participated in break-time conversations, and sat in on staff meetings. As noted however, fewer activities were sampled in the pilot schools. Thus the picture of activities and views of head and teachers is only a partial one. The picture of activities and views at Moorland, and to some extent Larkway is more 'representative' for the reasons already outlined.

Methodological triangulation was used. Cohen and Manion argued that exclusive reliance on one method "may bias or distort the researcher's picture of the particular slice of reality investigated" (Cohen and Manion, 1987, p. 254). In this research, as noted in the previous section, a variety of methods were used to gain information about the settings under consideration including observations, interviews, and documents, (including a questionnaire). As noted in the previous section different methods were used in order to look at the same data from different directions and also to gain different types of information. As noted observation was the first source of data from which questions were generated but also was used as a means of comparing statements made about practice with the actual practice. Interviews were then used, as noted, as a means

of checking the researcher's observations and account of 'the situation' and also to 'get at members' accounts of this situation.

Interviews were also used to develop or extend understanding by eliciting new information. A series of interviews was also used to cross check different accounts. Thus, for example, as noted, the researcher talked to the nursery teacher at Moorland about whether there were differences in terms of aim and approach between the nursery and the reception classes in the infant school. A similar interview was also conducted with the reception teachers. This enabled a category to be built up.

As noted, documents were also used, as a means of checking the information gained from observations and interviews as a new source of information, and to complement the picture being built up by the use of other methods. Thus, for example, during an interview the head at Moorland discussed the children and nature of the area at Moorland. She was asked if the school log book could be examined. This contained further information about how previous heads had seen the area.

Documents were also, as noted, a source of ideas for observation and interviews. Thus 'fitting into school' was a phrase which the researcher came across in the teachers' records concerned children. This led to observation of the 'routine' in the nursery and reception classes at Moorland, and interviews with the teachers on routine in both settings.

At Larkway the 'guidance notes for teachers' were referred to to gain access to teaching approaches in the school but this was followed up by observation in the classrooms, discussion with the staff and head to see whether the guidance notes 'represented' both the teachers' and head's views on the issue - with the head to find out when it was written, whether her views had changed.

As noted, a questionnaire was used both at Moorland and Larkway. However, in relation to validity this was the aspect of data collection where

there were most problems. It was considered that there were several. First, concerning the lack of time in which to adequately prepare the questionnaire; second the fact that the researcher did not supervise its administration at Moorland, as was the case at Larkway. Third, it was possible that there was some corroboration over responses to the questionnaire. The head and teachers at Moorland discussed the questions, according to the head. Thus there is the problem of whether the answers given 'represented' the teachers' own views.

As noted the actual ordering of the questions could have influenced the way in which the teachers answered them and so affect validity. Another important factor was that, as noted, the format of the questionnaire was altered before being given to the staff at Larkway (although some questions remained the same). Thus it was difficult to make comparisons, as like was not being compared with like.

It was stated earlier that if the questionnaire had been the only method used to get data on teachers' educational perspectives, then it would have provided very limited information. However, this may have more to do with the design of the particular questionnaire and its administration rather than being a feature of questionnaires per se, a point noted earlier in Section Two of this chapter. The questionnaire at Larkway, as noted, provided a useful jumping off point. Its success was partially due to adequate follow up after completion.

As noted historical documents were used. The question of the validity of these was discussed in previous parts of the section. As noted it was difficult to evaluate the degree of bias which had gone into the selection and interpretation of material presented in secondary sources, and in the case of primary source materials written a long time ago it is difficult to get 'behind' the accounts to find out the intentions of those who wrote them. This contrasts with the documents the researcher had access to in

schools where it was possible to ask why the document had been written. The researcher tried to look at a range of sources, both primary and secondary, in order to get many different views in the same issue, for example, views on the purpose of infant education. It was also a means of checking the accuracy of statements made by different authors.

Thus so far it has been stated that a range of methods was used in order to overcome the problem of 'representiveness' and 'bias'.

'Within method' triangulation was also used. This was interpreted by the researcher as meaning a range of strategies. Again, these were discussed in the previous part of the section so are only briefly summarised here. Within interviewing, as noted, a range of different 'strategies' were used. First short conversations were held with teachers based on observation in the classroom and often conducted during observation. Longer interviews were held to discuss particular issues. However, as noted a series of interviews were held over a period of time rather than one to enable the researcher to discuss a particular issue more than once and ~~try~~ to reduce the effects of reactivity. Further, a range of different types of questions were asked in order to look at an issue from different angles, and to clarify issues. It was stated earlier that the researcher did not wholly rely on 'solicited' answers, that is answers given in response to a particular question by the researcher. Some discussion was 'unsolicited' in that the respondents brought up particular issues during conversation. Thus, at Larkway the teacher talked about the version of the integrated day that she operated, rather than the researcher specifically asking her about it.

The previous part of the section also discussed data recording, and some of the factors which may affect validity, such as whether written observation notes actually represent what was said and done by the researched. It was argued that this could have been a problem in this

research since a tape recorder was not used. In order to overcome some of the problems, it was noted that the researcher concentrated on 'recording' what went on during short periods in order to be able to write down 'exactly' what was said and done. Where this was not possible 'key phrases' were noted down rather than trying to write down everything. A further check on the 'accuracy' of the recording was to present the 'written up observation notes' to each teacher so that they could read through them and comment upon them. As noted in the majority of cases there were no problems. Teachers on the whole accepted the written account as an accurate representation of events, and what had been said. On only one occasion, as noted, was the accuracy and truth of what the researcher had written brought into question.

To some extent theoretical triangulation was used. As noted at the end of Section Two of this chapter, the researcher agreed with the principle of eclecticism with regard to both theories and methods and the idea of using what will work in a given situation. Section Two attempted to show what theoretical aspects were found useful in this research.

This brings to an end the discussion of validity. An attempt has been made to show that a range of factors affect validity which cannot be wholly separated from one another. One can never conclusively determine how valid a piece of research is because there are so many variables involved which interact in many ways. This is true, however, for all kinds of research, not only ethnographic. The problem too is that the criteria for assessing validity are rather vague. All that can be said is that this researcher was aware of some of the problems of validity and tried as far as possible to consider the various factors which would affect this and adjust the research performance accordingly.

CONCLUSION

Overall this section has been concerned with the methods of data collection used in this research, although as pointed out data collection and analysis are seen as joint activities. Therefore, the section also tried to show what 'categories' emerged and how these were developed.

The sub-section attempted, not only to show what methods were used in this research, but the manner and also the content in which they were used. Although, each method was discussed separately, in the interests of clarity, it was considered that the methods were used continually and not in a particular order.

Other writers' 'interpretations' of different methods were given, followed by the interpretation of the researcher. This was done in order to show what the researcher had 'made' of the literature on methods.

An attempt was also made to show some of the problems associated with the different methods used and the ways in which the researcher tried to overcome them.

In the discussion of validity the researcher tried to show that the 'methods' of data collection cannot be considered in isolation from the 'person' actually using them. It was noted that it is very difficult to separate the various issues.

In conclusion then, this section has been a reflection on the process of data collection.

The next section of this chapter looks at the analysis of data in this research. As noted the issue has to a great extent been dealt with in this section. Therefore it looks first at some general definitions, the model of analysis developed by Glaser and Strauss (as this was partly used in this research) and finally the practical procedures of analysis which included; underlining and the writing of memos, for example. It also discusses briefly 'the writing up' stage.

SECTION SEVEN : THE ANALYSIS OF DATA

INTRODUCTION

This section is concerned with the analysis of data. This has to some extent been dealt with in the previous section because as stated data collection and analysis are not wholly separate. An attempt was made to show how categories emerged and were developed and the issues that were 'focused' on. This section is more concerned with the practical procedures of analysis.

The first part of the section looks at some general definitions of analysis and comments upon them and also notes some general problems concerning analysis.

The second part of the section discusses the model of analysis developed by Glaser and Strauss. It was noted in the previous section that in the early stages of the research Schatzman and Strauss (1973) was used to gain some idea of what observation was about. However, some of the ideas about stages of analysis which were discussed by Glaser and Strauss were used in so far as they were understood at the time. The second part of the section also notes some of the problems associated with Glaser and Strauss's model of analysis. This sub-section has been included to try to show what the researcher 'understood' Glaser and Strauss to mean, particularly in the early stages of the research.

The final part of Section Seven looks at the 'procedures' used in this research to analyse the data both during the field and after. These then are the concerns of this section.

1. Some General Definitions of Analysis

This part of the section looks at some general definitions of analysis and some general problems associated with it.

Ball referred to analysis as a "... process of interpretation". (1983, p. 96). Patton on the other hand appeared to separate analysis and

interpretation for he stated that:

"Analysis is the process of bringing order to the data organising what there is in to patterns, categories and descriptive units. Interpretation involved attaching meaning and significance to the analysis explaining descriptive patterns and looking for relationships and linkages among descriptive dimensions."

(Patton, 1980, p. 268)

This distinction seems problematic. It would seem to this researcher that an important part of 'analysis' may be just this fact of looking for 'relationships' and 'linkages'.

Spradley, for example, defined analysis as being just this for he stated that analysis was:

"... the systematic examination of something to determine its parts, relationships among parts, and their relationship to the whole [and] ... a search for patterns." [in the date].

(Spradley, 1980, p. 85)

Schatzman and Strauss referred to analysis as being "the working of thought processes" and argued that such thinking was self-conscious. Like Spradley they considered that the process of analysis was systematic. (Schatzman and Strauss, 1973, p. 108).

However, analysis may not be necessarily either be self-conscious or systematic. It is because there may be some unconscious processes at work that makes writing about analysis so difficult. It is, and was for this researcher very difficult to be explicit about how the analysis was done despite the existence of memos and notes written with the observations and other data. For example it was not always clear at the time, or now, why certain things 'come to the attention of' the researcher during observation. As indicated in the previous section analysis may be to some extent automatic, part of the way the brain operates.

There are both conscious and unconscious elements in the act of observing. In the act of observing the observer places what he or she sees

into categories. To some extent this 'classifying' is automatic. However, there is also a self-conscious element which consists of going over this 'automatic classifying'. (See also Lofland, 1971, p. 6).

It has just been argued that analysis begins during observations, that it is part of the act of observing, a point made by King. (King, 1984, p. 129). He was referring to conscious action. Writing about analysis, making the thought processes visible to others, as well as oneself, is also part of the reflective 'conscious' process of analysis. As noted previously this reflexivity is an important aspect of ethnographic research.

As stated Spradley viewed analysis as being systematic. The use of this term gives the impression of analysis as being orderly and straightforward. In practice the process is a more muddled one as noted even by 'experienced' ethnographers. (See Ball, 1984; Woods, 1986; Mardle, 1987). The muddle may to some extent reflect the fact that there are no 'models of fieldwork practice', including analysis which "translate straightforwardly into a structured routine of data collection". (Ball, 1983, p. 71). This comment was written in 1983. It was particularly true in 1980 when this research began.

Both Turner and Gleeson and Mardle made similar comments. Turner, for example, argued that there is "no pre-ordained plan". (Turner, 1983, p. 76). Gleeson and Mardle too, noted that there is no tried and tested formula. (Gleeson and Mardle, 1987). Nevertheless, some people have put forward models of analysis and presented them in terms of stages.

The idea of stages has two aspects, first, the time when analysis takes place and second the actual procedures used which are sometimes presented as a progression through stages. In relation to the first aspect some writers, as noted in the previous section see analysis as going on throughout fieldwork and after it is finished. For example, Schatzman and Strauss and Lofland considered that data collection and analysis were not separate

stages, a point with which this researcher agrees. As noted in the previous section Schatzman and Strauss considered that analysis should begin early in the field work. (Schatzman and Strauss, 1973). Lofland also made this point and also argued that the 'final stage of analysis' could then be used to bring " final order into previously developed ideas". (Lofland, 1971, p. 118). In practice analysis may not necessarily proceed in this way as has already been noted in the previous section. In so far as there are stages in the analysis, both in the field and later those are not necessarily distinct from one another nor gone through chronologically. Such stages are in fact artificial 'markers'. In practice such 'stages' not only tend to merge into one another, as Woods pointed out, but also may proceed simultaneously. (Woods, 1986, p. 121). Thus while in some areas of the research categories can have been developed and in depth focusing begun, in others the researcher can still be at the stage of seeing what categories, if any, are emerging. It is also only too likely to notice further possible categories after having left the field, or to see that other material does in fact relate to an existing category. That is, some procedures gone through in the field also occur afterwards. Under the pressure of working in the field it is all too easy to fail to notice what one has or hasn't.

It has been noted so far that analysis of data starts while in the field and continues afterwards. However, in a more general sense analysis can be seen as a process of reflection on the research as a whole including the approach used, the methods of data collection, recording, and the question of validity, or the accuracy of the interpretation placed upon the data. Thus in one way this whole chapter has been a form of analysis.

However, analysis also more specifically refers to the interpretation of certain items of the data as being important categories, and refers to the development or otherwise of such categories and the way in which this was done.

Presenting an account of how the analysis was done is essentially putting a systematic gloss on what is a complex process. Nevertheless as Turner has argued, that, although it might not always be possible to trace our thoughts or be fully aware of their line of development, it was still useful if the researcher at least tried to do so. (Turner, 1983).

The next part of the section outlines the approach to analysis which Glaser and Struss advocated, and what ideas the researcher used from them.

2. The 'Model' of Analysis used in this Research

This part of the section discusses the model of analysis used by Glaser and Strauss. This is done because aspects of it were used by the researcher in this research. Glaser and Strauss compared their 'approach' to others in order to show how their own differed or was similar in some respects. One such approach was that used by Becker and Geer (1968). As aspects of the latter were also used their approach is also discussed briefly.

During some stages in the field work the work of Spradley was referred to. (Spradley 1980). This showed how to go about doing field work step by step, including analysis. This is discussed in the next sub-section which focuses on the way analysis was done in this research, on the practical procedures as noted earlier.

This sub-section starts by briefly looking at the other approaches to analysis mentioned by Glaser and Strauss before discussing that of Glaser and Strauss. It attempts to show some of the problems associated with their 'approach' and what the researcher 'understood' analysis to mean, and found difficult to understand.

Glaser and Strauss in their discussion of their own approach to analysis distinguished it from three other approaches.

The first approach identified by Glaser and Strauss was one in which:

"If the analyst wishes to convert qualitative data into crudely quantifiable form so that he can provisionally test a hypothesis he codes the data first then analyses it."

(Glaser and Strauss, 1967, p. 101)

Glaser and Strauss argued that the emphasis was on verification and testing of theory and finding proof for given propositions. Becker and Geer were cited as users of this approach. As will be explained later some of the procedures used by Becker and Geer were used in this research.

The second approach described by Glaser and Struss was more general, and one in which the analyst is "constantly redesigning and re-integrating his theoretical notions as to review his material", and where explicit coding was not done. (p. 102).

Glaser and Strauss also identified a third approach, that of analytic induction which in their view combined aspects of the first two approaches and was:

"concerned with generating and proving an integrated limited, precise, universally applicable theory of causes accounting for a specific behaviour (e.g. drug addiction)."

It was also considered to be concerned with testing:

"a limited number of hypotheses with all available data consisting of numbers of clearly defined and carefully selected cases of phenomena."

(Glaser and Strauss, 1967, p. 104)

Glaser and Strauss considered that their approach combined certain aspects of the above approaches but was different in other respects. They considered for example that their approach was similar to Becker and Geer in terms of the "explicit coding procedures" (p. 102) but different in that it was concerned with the generation, and discovery of theory rather than testing of a few hypothesis, and verification. It also seemed to the researcher that there were other similarities between Becker and Geer and Glaser and Strauss. Firstly, Becker and Geer seemed to be advocating that data collection and analysis were not separate

activities, like Glaser and Strauss, as noted in the previous section.

They stated that:

"important part of the analysis [are] being made
while the researcher is still gathering his data."

(Becker and Geer, 1958, p. 653)

Becker and Geer also stated that early analysis in the field directs future data collection. (Becker and Geer, 1958). This is something that Glaser and Strauss also advocated.

As noted above Glaser and Strauss also argued that in contrast to the approach of Becker and Geer their approach was more concerned with the generation of theory. This gives the impression that Becker and Geer were only concerned with testing hypotheses and started with a clear idea of what they wished to research. However, reading Becker and Geer gave a different impression. They stated that during the first stage of the research:

"the observer looks for problems and concepts which
give greater understanding of the organisation he is
studying."

(p. 653)

and that at this stage "the researcher is using his data to speculate about the hypothesis". (p. 654). However, there is also evidence to support Glaser and Strauss's view as well because Geer, elsewhere, argued that in one study Becker and Geer did start with an idea of what they wished to look at and did seem concerned with the testing of an hypothesis. (Geer, 1969).

Glaser and Strauss considered that Becker and Geer were more concerned with verification and testing of hypothesis, as was analytical induction in their view. However, it was not clear first what was meant by these terms, and secondly, whether Glaser and Strauss did reject verification and testing. Later reading of other writers' accounts of Glaser and Strauss suggested that the researcher was not alone in this respect. Rose for example argued that:

"... Glaser and Strauss state their case in such a way that testing ... appears to be excluded."

(Rose, G., 1982, p. 127)

He himself considered that it was difficult to analyse data:

"without at any stage having to revise some of one's ideas or discard tentative hypothesis."

(p. 127)

Hammersley and Atkinson considered that Glaser and Strauss did recognise some form of testing as necessary. (Hammersley and Atkinson, 1983, p. 22). It was not clear in fact whether Hammersley and Atkinson meant testing or verification. Certainly Glaser and Strauss did not appear to wholly reject verification for they stated that:

"Of course verifying as much as possible with as accurate evidence as possible is a requisite while one discovers and generates ... theory."

(Glaser and Strauss, 1967, p. 21)

If verification is considered to mean checking whether a given phenomenon exists by further observation then there seems no reason why it should not be part of 'theory development', whatever that may mean.

Glaser and Strauss stressed that they were concerned with theory development. Again it was not clear what was meant by the term theory. This lack of understanding particularly during the early stages of the research made it difficult to assess Glaser and Strauss. It seemed to the researcher too that Glaser and Strauss assumed that researchers were necessarily concerned with theory development or testing. These two points were made by Brown. He argued that Glaser and Strauss were unclear in their explanation of theory and he himself distinguished between two types, descriptive and explanatory. The former was said to be concerned:

"with classification ... mapping out a problem area - a kind of taxonomy. Things are described which go together - in doing so sense is made of the world."

(Brown, 1976, p. 2)

Exploratory theory on the other hand was said to be concerned with causes. "It asks whether something is responsible for something else." (Brown, 1976, p. 2).

Rose distinguished between theory development and testing but also considered that some studies may not necessarily be concerned with either but rather instead the investigation of:

"a perceived social problem to challenge a generally accepted assertion, or simply to describe a phenomenon of interest to the sociologists."

(Rose, G., 1982, p. 11)

Burgess too argued that studies may not necessarily be concerned with 'theory' but more with "ethnographic description which may involve elements of theory that are involved in the writing." (Burgess, 1984, p. 181).

Glaser and Strauss argued that their approach was similar to the second style outlined earlier in terms of "the style of development" but that their own 'coding' and 'analysis' was more systematic. They argued that "re-designing the analysis is a well known normal tendency in qualitative research (no matter what the approach to analysis is". (p. 101). If this was true then it is not clear why this approach constitutes a separate way of doing analysis.

Glaser and Strauss emphasised the 'systematic' nature of their analysis. In this research it was not always possible to be 'systematic' during the field work due to lack of time, sheer tiredness after a day's field work and the stressful nature of the latter. Becker and Geer seemed to make an important point that a researcher may not be "as systematic as he would like until he reaches the final stages of analysis". (Becker and Geer, 1958, p. 653). They argued that there were practical reasons for leaving most of the 'systematic' analysis until after completion of the field work. It was argued that:

"the final comprehensive analysis may not be possible until the field-work is completed."

(p. 653)

This seems a realistic view of the process of analysis and also a further indication that Becker and Geer did not see data collection and analysis as necessarily separate but rather that the extent to which the latter could be done during field work was limited by practical factors.

Rose argued that the "usefulness of Glaser and Strauss' work" lay more "in ... their valuable suggestions for research strategies and procedures". (Rose, G., 1982, p. 130) During the early stages of the field work the researcher was only able to partially understand some of the above aspects of Glaser and Strauss just discussed. What ideas were understood mainly related to the actual procedures of analysis, the four stages of what Glaser and Strauss termed the constant comparative method.

Glaser and Strauss identified four stages in analysis. The first stage consisted of 'coding', or sorting incidents observed into categories. Some of the categories will be constructed by the researcher and others come from the language of the participants. Glaser and Strauss stated that such 'coding' was done by making a note in the margins of the field-notes, or on cards. They also recommended that at some point the researcher should stop 'coding', reflect on the data and record 'memos' on these reflections.

According to Glaser and Strauss the second stage consisted of the integration of categories and their properties. Coding continued during this stage but changed from comparison of one incident with another to comparison of incidents with the properties of the category. During this stage the researcher begins to look at relationships between categories.

The third stage consists of 'delimiting the theory and reducing the number of categories to be coded'. The researcher focuses on a smaller number of categories and the coding becomes more selective and focused. Coding continues until categories are 'saturated', that is when the incidents observed no longer add anything to the existing category.

The final stage consists of 'writing up'. During this stage the researcher collates and brings together all the 'memos' written during the field work. Such memos, according to Glaser and Strauss, provide the main themes for later publication. (Glaser and Strauss, 1967). This seemed to the researcher as being a vast oversimplification of the writing up process, one which for the researcher was a horrendous one.

The stages outlined were followed to some extent in this research. The basic ideas taken from Glaser and Strauss were first that data collection and analysis were not separate activities, secondly that 'categories' and their properties should 'emerge' from the data, and thirdly that these emerging categories should then guide further data collection. The previous section attempted to demonstrate how categories did emerge and were developed. It seemed difficult to separate the two aspects, data collection and analysis. In this research, however, a substantial amount of the analysis was carried out after the completion of the field work. In many respects, as will be shown later, the process of analysis after the field-work was similar to that in the field in that 'coding' was carried out. The reasons for doing more detailed analysis after the completion of the field-work were noted earlier.

This sub section has attempted to discuss the model of analysis used by Glaser and Strauss, because, as noted, some aspects were used in this research, and also at other approaches discussed by Glaser and Strauss. It has also tried to show some of the problems associated with Glaser and Strauss' model and what the researcher 'understood' by the model. The final sub section focuses on the 'practical' procedures used both during the field work and after it, and the process of writing up rather than showing how categories 'emerged' and developed, as it is considered that these latter aspects were dealt with in the previous section.

3. The Practical Procedures of Analysis and 'Writing Up'

This final sub-section is concerned with the practical procedures of analysis used in this research, both during the field work and after it, together with the process of writing up. As noted, it is not concerned with how 'categories' emerged and developed because this was dealt with when looking at methods of data collection.

The first part of the sub-section looks at the practical procedures of analysis used during the field-work.

The second part of the sub-section looks at the procedures used after the completion of the field-work, and at the 'writing up' process.

Brief examples of the 'practical procedures' are given, from about a hundred pages of analysis in its various forms, in order to demonstrate what procedures were used. Inevitably, therefore, there is some degree of selection.

The sub-section starts by looking at the practical procedures used during the field work.

The initial stage of analysis, as noted, consisted of 'coding' or sorting the data, trying to find out 'what was going on'. However, 'coding' continued throughout the field-work but became more focused to some extent, more selective. As noted, during the later stage of analysis the researcher is trying to develop and build up the categories which were found earlier.

In practical terms the analysis in the field involved certain procedures including: 'making a mental note', underlining what stood out or seemed difficult to understand, notes in the margin of the observation interview notes, observer comments incorporated into the observation notes, the use of 'Spradley's Domain analysis', and the writing of 'memos' of various kinds. Each of these is discussed in turn. The analysis during the field-work, as noted, was not as systematic as Glaser and Strauss

recommended (Glaser and Strauss, 1967). As noted there were constraints on the amount of analysis which could be carried out in the field, namely lack of time, and the 'stress' of doing the research. The latter sometimes inhibited clear thinking. The categories that were developed were on the whole 'descriptive'. More detailed analysis took place after the field-work when there was more time to 'reflect' on the data.

Analysis in the field sometimes was little more than making a brief mental note during the actual observations themselves. Thus, for example, during observation of one of the classes at Briarfield, a reception class the researcher noted that it appeared to be organised differently to other classes in the school. At Moorland, the researcher visited the nursery after having seen classrooms in the infant school. The researcher mentally noted that the atmosphere seemed different and that activities seemed different. This information was mentally 'filed away' to be referred to later when writing up the observation notes.

Observation and interview notes were written up as soon as possible, as noted in the previous section. During this process the researcher read through the notes and underlined what seemed to stand out, or appear puzzling, for example:

1. Briarfield Class A
'Children come into the classroom and sit on the mat.
One asks:
Child: Are we doing our work now?
Teacher: Yes.
Child: Good, I like writing.
Teacher: It's story and mathematics.

In the pilot schools the analysis mostly consisted of underlining, although some notes were written in the margins of the observation notes themselves. These will be referred to later. No attempt was made to consolidate information in the form of 'memos'. If the researcher saw something

interesting or puzzling then this was, as noted, underlined and then the issue discussed with the teachers if there was time. As noted in an earlier section the researcher worked during the evening which reduced the time available for detailed analysis in the field. Underlining was a quick way of drawing the researcher's attention to a category.

Underlining was also used at Moorland, for example:

2. Moorland Class 1

'Some children look at puzzles. The teacher tells them to sit on the mat ... she tells them that after assembly they are going on a visit. After the visit the teacher talks about it. While she is doing this one child, Mary, tells another:

RULES: Children's

"She's not listening Mrs. Dale."

recognition of rules

Teacher: "She's not listening. When it comes to drawing and writing you won't know what to do. You won't have anything to say."

See also 30.9.80 for other examples of children's knowledge of rules.

(Observation Notes)

Underlining was not limited to the early stages of the field work. It was used throughout the field work to indicate that an item might be important. Incidents which appeared to belong to the same category were underlined in the same colour for easy identification of particular categories. When looking at documents the same procedure was used.

SCHOOL LOG BOOK

1970s period

' ... new infant/nursery adviser ... visited school At last somebody in authority who sees and understands the problems of the children ... teachers ... lack of resources.'

In addition to underlining notes were written in the margins of the written up observation notes. The underlining merely indicated that some item was important and the note in the margin showed why it was. In example number three it seemed to show children's awareness of rules concerning being quiet while the teacher was talking. In fact other examples had been found much earlier during the observation in other classrooms. The note in the margin referred to an earlier example. This procedure again meant that the researcher could see how a category was developing and compare incidents to see if they were examples of the same or different category.

A particular section of observation notes could be 'coded' several times, as the following examples indicate. They are taken from one of the pilot schools, Moorland, and Larkway.

4. BRIARFIELD Class A

Division between morning/
afternoon activities.
Play/work distinction?

Mrs. Brown the teacher stated that during the morning ... children could either "play games or do art and craft". The teacher said that in the afternoon they did "formal work".

View concerning readiness
- children starting school
not ready to do certain
activities

The teacher was asked if she did this work in the morning. She replied that "When the children first come to school they do not want to do formal work, especially the boys". When asked why this was so she said that she thought that the boys always wanted to play games and gravitated to them as soon as they came into the classroom"... and "When the boys first come to school they take more time to settle."

Gender differences

Boys want to do
particular activities.

Take more time to
settle. Why?

(Observation Notes:
Briarfield, 1980)

The notes were not set out as tidily in the actual written up notes as above. Many of these categories were expanded after looking at the other

pilot schools, and at Moorland and Larkway, particularly the play/work distinction. However, as noted in a previous chapter, the gender issue was not really developed in detail. The researcher did not see the above teacher in example 4 again so the idea was not explored, that is why boys wanted to do particular activities, and why they were considered to take longer to settle.

The next example comes from Moorland:

	5. <u>Class 1</u>	<u>Afternoon</u>
<u>Direction</u>	... children come into the classroom. <u>Mrs. Dale is seen putting out various activities</u>	
<u>Control of resources</u>	<u>... crayoning, cut out shapes, puzzles, plasticine, lego.</u> After activities have been laid out, <u>Mrs. Dale directs children to certain activities</u> ... two children are chosen to go in the Wendy House, sand, water tray. The rest of the class are told they can " <u>choose plasticine</u> ".	
<u>Control of activities</u>		
<u>- choose to certain extent</u>		
<u>Choice?</u>		

(Observation Notes, 1980)

As noted, teachers at Moorland considered direction at Moorland to be important. After having observed in Mrs. Dale's classroom, the researcher went to the other classrooms in the school to see if such direction was found there. Examples were 'coded' in a similar way. The researcher also looked to see if there were examples of 'greater pupil choice' rather than teacher direction of activities. (See Mrs. Neaves - Chapter Six).

The third example is taken from Larkway.

	6. <u>First visit to Larkway</u>
	During the visit I mentioned to the deputy that I thought the school was <u>quite large</u> . She replied that she did not agree with this view. She <u>considered that the classrooms were too small for thirty</u>
1) <u>Constraints</u>	
<u>Amount of space</u>	
<u>restricts movement</u>	

2) Form of control
non-verbal

See King (1978)

3) Behaviour related
- See Moorland for
other examples (1, 2, 3)

children - not enough space to move
around ... I stayed to dinner today ...
At the end of the meal the head stood up.
She looked at a group of children. They
were talking. She said:

"I'm waiting for you to stop talking.
If you want you can stay in all
dinner-time."

The children stopped talking....

(Larkway : Observation Notes)

The above example shows not only that a section of data could be coded for more than one category but also that comparisons were being made with previous data in order to build up particular categories. Thus the 'descriptive' category rules consisted of 'the nature/type of rules, teachers' views about rules', children's awareness of 'rules', and how 'rules' were made visible to pupils. The researcher continued to 'code' examples of the various sub-categories. At a very basic level analysis, consisted of comparing and contrasting data related to a particular category by simply reading through the observation notes again and again.

Sometimes comments were incorporated into the actual observation interview notes.

7. Nursery Moorland

Children appear to
choose what to do

When the children came into the classroom
they went straight to various activities

O.C. Classroom - not sure if this is
an appropriate term to use to
describe nursery.

... Two children begin to ride
tricycles ... One of them asked
another watching them if he wanted
to ride one of the tricycles.

O.C. This line of action noticeable in
morning sessions. During afternoon

sessions children were observed pushing, shouting, spitting at one another. Some grab toys they want - don't ask one another. Are there differences between the two groups. Does Mrs. Raynor see these differences and if so, to what does she attribute them?

(Observation Notes : Moorland)

These questions were in fact raised in a later interview with Mrs. Raynor. Out of this interview came a new idea, that of the "nursery group". When the interview notes were written up this term was underlined, a reminder to the researcher to discuss the item with this particular teacher.

The second example of an 'observer comment' is taken, again, from Moorland. It is taken from a discussion by Moorland staff on the results of diagnostic tests given to the 'top infants' (6 - 7 yrs) in the school.

8. Moorland Staffroom Discussion -
Results of Diagnostic
Testing

problems - defined earlier
in a conversation in the
classroom ... 'poor
concentration', not
interested in school -
Mrs. Knowles

... Mrs. Neaves said that she knew the children had problems but did not know what to do about them. She said that the educational psychologist had identified the problems ... Mrs. Neaves suggested that parents could be asked to do certain activities at home to help their children. The head shook her head, and said that:

"They wouldn't know what to do."
[did not have] "... sufficient
knowledge of what to do like
teachers do."

O.C. 1. Again Moorland children defined as having problems. This is done in a number of situations - teacher to researcher in classroom, teachers

to teachers in informal chats during dinner hour, and during staff meetings.

2. Mrs. Neaves' view of parents as capable of giving help.
3. Teacher 'not knowing what to do'.
The head's comment is rather ironic in view of the above comment. It raises the issue of 'professionalism' and teachers' claims to be professional. In earlier discussion with the head she described her staff as "fellow professionals".

In fact the above extract shows three practical procedures of analysis underlining, comments in the margin, and observer comments.

The third example is taken from Larkway and shows that sometimes the notes were more 'analytic' and less descriptive.

9. Larkway Class 2

The teacher tells the children what activities are available ... "We're going to make a secret garden ..."
[and] ... "make some bubble prints. David, come and watch, you'll like doing this". Some children push in. The teacher said, "Stop fussing please". She starts to explain what to do:

"If you are going to start a blue one ... bend down and blow. Don't blow too fast..... You must put on your aprons for this job ... Now I know you all have a garden ... Close your eyes and tell me what you can see in your garden ..."

- O.C. This brings to mind Bernstein's elaborated and restricted code. At Moorland teachers were observed talking to children. It

was considered that teachers may use a restricted code. Also there might not be such a discrepancy between teachers' speech when talking to 'working' class children and the speech of working class children.

(Observation Notes : Larkway)

Thus some observations could spark off ideas. In this instance the researcher was 'playing' with an idea which later formed the basis of part of a chapter. However, on the whole the 'playing around with ideas' was done after the field-work was completed.

The above example, showed the researcher's concern for language in the infant school. During observation in the nursery at Moorland, a similar concern was expressed for the 'way in which teachers talk to children'.

10. Nursery Teacher Talking to Child

Use of Endearments

"Don't do that darling."

- O.C. Is this the way the nursery teacher usually talks to the children. Look for further examples.

(Observation Notes : Nursery)

Initial analysis could also include questions raised by the particular piece of observation and show what to focus on next. In this instance the researcher returned to the nursery and Moorland infant school to collect examples of the 'way teachers talked to children', and then analysed these examples, comparing the ones from the nursery and the infant school. A note was then made as follows.

- O.C. The style of language in the two settings: nursery and infant school appears different - no examples of endearment

in infant school but come across more examples of nursery school, e.g.

"poppet"

"honey"

"sweetheart"

Perhaps an indicator of a different type of relationship between teacher and children in the two settings - See also differences in approach and nature of activities in the two settings.

(From observer notes:
Moorland Nursery)

Again the incorporation of these 'comments' into the actual observation notes enabled the researcher to see at a glance the way in which a category was developing, and how it related to previous observations on a similar theme. Hammersley and Atkinson considered that incorporating comments in this way enabled the immediate analysis of an item, and that it could be done relatively quickly. (Hammersley and Atkinson, 1983, pp. 169-70).

During the early stages of field work at Moorland it was difficult to see what categories were emerging. They did not necessarily 'stand out'. It was for this reason that Spradley's 'Domain analysis' was used, in order to help identify categories and discover patterns in the data, to help clarify the issues which had emerged during the first term. Spradley considered domain analysis to be the first stage of analysis. This final stage consisted of the search for 'cultural domains', or categories of meaning, and their sub-categories. These 'cultural domains' were said to include:

"... three basic elements, cover terms, included terms and semantic terms."

(Spradley, 1980, p. 84)

The cover term is the name of the particular cultured domain, for example, 'rules'. Included terms, can be considered as sub-categories, for example types of categories. The semantic relationship refers to the linking together of two categories. It defines a relationship between the two.

Spradley referred to several types of relationship including:

<u>STRICT INCLUSION</u>	X is a kind of Y
<u>ATTRIBUTE:</u>	X is an attribute of Y
<u>SPATIAL:</u>	X is a place in Y
<u>LOCATION FOR ACTION:</u>	X is a place for doing Y
<u>CAUSE/EFFECT:</u>	X is a result of Y

As noted various categories were identified during the first term at Moorland. The observation notes and interview notes were read through again at the end of the first term in order to identify the 'cover terms' or categories and then to identify any sub-categories, if these had emerged and to consider any possible relationship between them. As noted this had been done to some extent. Plain sheets of paper were used to write these down. Each 'domain' or cover term was written on a separate sheet of paper, for example:

1. SEMANTIC RELATIONSHIP (A KIND OF)

<u>INCLUDED TERM</u>		<u>COVER TERM</u>
Games, puzzles, lego	x <u>A KIND OF</u> y)	
lotto, word/number games	_____)	
sand	_____)	
'bricks'	_____)	
'playtime'	_____)	
'playing dogs'	_____)	
)	
)	<u>PLAY</u>

2. SEMANTIC RELATIONSHIP (ATTRIBUTE OF)

<u>INCLUDED TERMS</u>		<u>COVER TERMS</u>
'Aggressive'	x _____ y)	
'Apathetic' ...	_____)	
poor language)	_____)	
development)	_____)	
lack of social)	_____)	
training)	_____)	
inability to)	_____)	
concentrate)	_____)	
)	<u>MOORLAND</u>
)	<u>CHILDREN</u>

3. SEMANTIC RELATIONSHIP (AS A RESULT OF)

<u>INCLUDED TERM</u>		<u>COVER TERM</u>
Aggressive behaviour	x-----y	lack of control at home
inability to concentrate	-----	<u>home background</u>
apathetic	-----	lack of stimulation at home

4. SEMANTIC RELATIONSHIP (REASON FOR DOING)

<u>INCLUDED TERM</u>		<u>COVER TERM</u>
Lack of social training	-----	emphasis on social training at school, particularly nursery
poor language development	-----	'increased' oral work at school

(Domain analysis : Moorland)

It has been argued that Spradley's guide to doing observation is somewhat 'contrived'. (Burgess, 1984a). The researcher considered this to be true to some extent but also found aspects such as the domain analysis useful. It was considered at the time (during the early stage of research at Moorland) that nothing much had emerged. However, the domain analysis indicated, at least to the researcher, that 'something' had been 'picked up', but also showed more clearly the nature of the emerging categories which had emerged, first that children at Moorland were defined in a particular way, as having problems and that this was linked to home background. Further, the 'way children were' was seen as a reason for presenting a particular curriculum.

The domain analysis also highlighted differences in the 'kinds of problems' children at Moorland were perceived to have, for example:

<u>WORK ATTITUDES</u>	<u>SOCIAL</u>	<u>GENERAL BEHAVIOUR</u>
apathetic	lack of social training	"no idea how to behave"
lack of interest		aggressive
can't concentrate		noisy

All these had been identified during chats with teachers in the classrooms and during break times in the staffroom. Access was given to children's school records written by the teachers. As noted in the previous section these were analysed in order to see in what ways teachers wrote about the children and whether these were similar or different to the ways they 'talked' about them.

The analysis consisted simply of reading through the records and noting down on a plain sheet of paper the comments made about individual children. These were then sorted out according to whether they referred to academic progress or social development for example. There was a separate sheet for each child which was divided into sections. The number of comments in each category was added up and the total written at the bottom of each section. These written comments were compared to spoken statements by teachers, to see if there were any similarities or differences. What did emerge from the analysis was that different teachers saw particular children in different ways, as shown in Chapter Five.

The domain analysis was carried out again during the summer term at Moorland in order to see how existing categories were developing and if any new ones had emerged. It was not used at Larkway, first because the research focus was clearer and there was not such a problem in trying to identify the issues, and secondly because the researcher was only in the school for a relatively short period compared to Moorland.

At various points during the field work 'memos' were written, for example at the end of the first term, during the spring term, and at the end of the summer term. These memos were usually summaries of all available information to date on a particular category. The point of the memos was to see what categories had emerged and what sort of information was available on them, as well as pointing to areas which could be explored during later field work, or 'playing around' with an idea. Those

memos were kept with the field-work notes, again for quick and easy reference.

At the end of the first term at Moorland the field-work notes were read through and a 'resume' written of what appeared to be the main themes which had emerged during the field-work during that period, and also looking back over the pilot school materials. On one side of a sheet of paper the 'emerging' categories were written and on the other side questions relating to those categories to be looked at or asked about during the later field-work, for example:

- | | |
|---|---|
| 1. <u>Moorland children defined in a particular way, e.g. aggressive.</u> | a) What is the nature of the categorisation used by teachers at Moorland to define children and parents. |
| 2. <u>Moorland Parents defined in a particular way</u> | b) Which teachers?
c) Why do teachers see children/parents in this way? |
| 3. In definitions of children reference made to <u>home background</u> | a) How is 'home background' defined by teachers?
b) Does home background mean 'working class'? |
| 4. <u>Home does not provide certain skills (Moorland)</u>
(<u>Children lack certain skills on arrival at school</u>) | a) Are children seen as ' <u>deficient</u> ' in some way?
b) What is the nature of these <u>skills</u> ? |

(Memo : Emerging Themes 1980)

These are just a selection of the 'categories' and questions which were written down. At the same time the researcher also read through the data and listed on sheets of paper some of the themes which seemed to have emerged, for example:

SHEET TWO : IDEAS MOORLAND

1. Children's 'failure' in the classroom is attributed to their background by the Moorland teachers.

2. The children's 'home background' is considered to be the cause of their 'poor attention', apathy, aggressiveness, poor social skills. [The children are defined as being aggressive, apathetic, lacking discipline. This did not stand out in the pilot schools.]

3. There appears to be differences in the organisation of activities in different classrooms. The classrooms of the two younger children seem different to those of the two older teachers.

(Ideas sheet Moorland
End of first term)

As stated the pilot school material was read through and the first term's observations at Moorland in order to see if there were similarities.

For example:

'... One of the main conclusions to be drawn from the observation in the four infant schools was that despite the apparent freedom to choose ... behind this freedom were ... limitations in terms of ... areas of knowledge covered, priority of the basics, hierarchy of activities ...

... Initial observation at Moorland also showed similar limitations. Unlike at Briarfield and Fairfield however 'freedom to choose' did not seem to play a major part in teachers' discussions about classroom organisation ... In the present school 'the problems' of the children and their 'background' are much more frequently mentioned than in the other schools previously visited.'

(Initial Ad Hoc Reflections 1980)

During the second term at Moorland the researcher focused on teachers' definitions of Moorland children and how 'home background' was defined as well as the actual nature of activities in the classroom, for example the degree of choice.

At the end of the second term further memos were written. Basically these were summaries of information on the developing categories, but also indicated when new information in a category had been found. For example:

'... new idea ... different teachers emphasise problems of children and home background to different degrees, [i.e. C1 and C3 don't mention 'problems' as much as C2, to some extent C4 and the head] ... also there are other forms of categorisation based on views of child development.'

(Memo : 2nd Term Moorland)

This particular memo then summarised each teacher's views on the issue.

At the end of the summer term a further series of 'memos' were written. The researcher concentrated on teachers' views of 'approaches' and methods used in the classroom during the summer term and reasons for using them. One memo summarised the data that had been obtained on this issue. The information was obtained by reading through all the field notes extracting the necessary material on the particular category and then noting it down on a separate sheet of paper. For example:

Methods Used by Moorland Teachers

'Teachers at Moorland, in their own words use "more structured" methods. They consider these more suitable for children at Moorland and that they "could not cope with free choice" because of their background - do not have the necessary skills to cope with free choice.

Moorland teachers said that they would use different methods in a more 'middle class' school.

Structured approach seen as more likely to help Moorland children ... Paradoxically because Moorland teachers have low expectations of what Moorland children can do this defeats their aims and sustains failure.'

(Memo : Summer Term Moorland)

As well as summarising material an attempt was also made to play around with an idea. On the whole however, the memos were fairly descriptive.

As noted earlier school records were analysed. In doing so the researcher came across the notion of 'school routine'. A teacher stated that

a particular child who had just started school had not got used to the school routine. This led, as noted in the previous section, to a period of observation in the nursery and reception classes at Moorland in order to see if there were differences in the nature of the routine. A further memo was written specifically on this aspect, which summarised information on it.

'There is a different pattern in the reception classes at Moorland to that in the nursery in terms of types of activities available, degree of pupil choice, teacher direction of activities ... Reception teacher described her approach as being - 3Rs in the morning and art and craft in the afternoon.

(Memo 1981)

As a result of writing the memo further questions were formulated, for example:

1. Do you see differences between the infant and the nursery class in terms of approach? .
2. How do you see your 'role' compared with the nursery teacher? (and vice versa).

These are only a very small selection of the questions formulated. The researcher returned to the field and put the above questions to those concerned. Their views were then later summarised in the form of a written memo.

The previous examples are only a selection from the memos written given in order to show their nature.

No memos were written at Larkway as already noted. The field notes were read through and key ideas underlined, as noted. If any questions were raised by a particular piece of observation or interview then these were written at the end of the notes. For example:

'First Interview with Head Larkway

'The head ... considered that her methods were "child-centred" -starting from the child ... The "Inspectorate"

and "Plowden". She said that she thought these had influenced her a great deal - influenced her practice ..."

Some further questions

1. In what ways does she see her approach as 'child-centred' - what about the teachers?
2. How did Plowden influence her practice?

(Interview Notes : Larkway)

As noted the 'Notes for Teachers' was then analysed. The researcher underlined anything which related to views on methods, and then talked to individual teachers.

More detailed analysis of the field notes was left until the end of the field-work.

This part of the section on analysis attempted to describe some of the actual procedures used to analyse the data. The process was rather muddled. This was partly due to the fact that the researcher was analysing different categories at the same time.

The process of analysis in the field was a far from orderly one. There were several occasions on which a memo was written on a particular category and then it was found that a similar memo had been written previously in the same category. (See Lacey, 1970).

The process of analysis is difficult to present, as noted, and it is not possible to show the whole process because of the sheer volume of material involved. (hundreds of pages).

As noted the analysis in the field was very 'descriptive'. Time was a major constraint in the development of a deeper analysis in the field. Glaser and Strauss suggested taking a long period of time off from the field-work in order to analyse data. In practical terms this is very difficult.

However, despite all the difficulties, by the end of the field-work the researcher managed to come up with various categories, and links between them. This analysis provided the basis for more detailed analysis on completion of the field-work. The latter analysis is the subject of the final part of this sub-section.

This final part of the sub-section attempts to show the nature of the analysis done after the completion of field-work. As noted, it is basically concerned with the practical procedures of analysis, that is, the nature of the 'filing', for example. The account is inevitably selective because of the amount of material involved. For example, there were over four hundred 'filed' cards, plus several large sheets of paper containing 'jottings' and drafts of a particular theme.

It was noted earlier in this chapter that doing analysis was hard but that writing about it was an even more difficult task. This researcher certainly found 'writing about' how the analysis was done extremely difficult.

The first part of the final part of the sub-section shows how the researcher attempted to clarify the main issues of the research, and that the attempt was not considered to be very successful for various reasons, which are explained.

The second part looks at the process of filing all the material on to cards, and gives some examples. There were different types of cards and these are discussed.

The third part of the final part of the sub-section briefly discusses the analysis of the questionnaire, which was not done until after the field-work had been completed, because of lack of time.

The fourth part looks at how the structure of the thesis was decided upon in terms of chapter headings and also briefly looks at the 'drafting'

of chapters, in particular the use of flow diagrams to help 'clarify' the main themes.

Finally, the 'process' of 'writing up' is examined, together with some of the problems involved. It attempts to show that even during the later stages of writing up the researcher is still doing analysis. As noted earlier, analysis is a continuous process.

These then are the concerns of this last part of the section on analysis.

Towards the end of the field-work an attempt was made to clarify some of the main issues and think about some of the implications of the research on infant schools, and to consider its relationship with other research. A memo was written on these aspects following a discussion with the research supervisor.

SOME THOUGHTS ... ON THE IMPLICATIONS OF THE P.O.
STUDY OF THE INFANTS SCHOOL

'It seems to the researcher that there are two main issues which the study will inform ... first, that of being a contribution to various studies such as Sharp and Green, King and other studies such as that of Galton and Simon, on the first years of schooling, and an opportunity to offer an explanation of how teachers' common sense knowledge inform ... actual practice.'

(Memo, 1982)

The idea of the research being 'a contribution' to other studies on primary schools was in fact used during the writing of the Introductory chapter. However, this early attempt to 'clarify' the issues was limited. While the researcher was aware of some of the main issues which had 'emerged' from the field-work, the implications of these had not been explored. There was little time during the field-work to really 'reflect' on these issues at all. Only after more detailed analysis of the data after the completion

of the field-work together with extensive reading, particularly on theories and methods, was the researcher able to see more clearly what the main issues were. This attempt at clarification in fact continued throughout the writing up period.

As noted, by the end of the field-work some 'themes' had emerged and these were noted down in various ways as explained in the previous subsection. The first step on completion of the field-work was to try to 'sort out' these themes. This consisted of reading through all the observation notes (observations, interviews, document notes, memos etc.) and writing down on a large sheet of paper what appeared to be the main themes, (or categories) and underlining these in different colours.

1. Example of List of Themes

VIEWS OF PUPILS

1. Some teachers 'label' pupils' behaviour (Moorland).
2. The head holds a particular view of Moorland children.
3. Teachers at Moorland see pupils as being different to those in other areas.
4. Some teachers 'blame' the 'way the children are' on 'home background'.
5. Some teachers (pilot schools) did not seem to place much emphasis on home background.

The last contained about fifty items. After this list had been compiled the sheets of paper were laid out on the floor and then cut up and all material related to one particular theme placed in separate piles. The next stage involved transferring all the sorted material on to cards. The purpose of the 'sorting' at this point was to help clarify what the main themes were and to try to order the material. There were different types of cards.

First, there were cards which contained summaries of all information related to a particular category. These were very similar to the 'memos' mentioned earlier, which were written at the end of a period of observation

and contained a summary of all information collected on a particular issue up to that point. Two examples are provided.

1. TEACHERS' VIEWS OF PUPILS OUTLINE OF KEY THEMES
 - a) Moorland children defined as being a problem - defined in certain ways - emphasis on the 'way these children are'. This emphasis not found to same extent in pilot schools. (See separate card).
 - b) Not all Moorland teachers defined children in the same way, or held with the same aspects of the definition.
 - c) Different types of definition, i.e.
 - i) Children as children
 - ii) Children in this school/area
 - iii) Children in a particular class
 - iv) Individual children
 - d) At Moorland more emphasis on children in this area: - one group - definition - travellers. Whereas at Larkway - individual children, although
 - e) BUT similarities - between Moorland and Larkway. In both cases reception children seen as 'lacking certain skills' and requiring 'schooling'.
 - f) Flexibility in definitions - 'work' 'behaviour'

The second example refers to nursery/infant school differences. As noted earlier, this theme developed during the field-work itself.

DIFFERENCES BETWEEN NURSERY AND INFANT PRACTICE

1. Greater free choice in nursery (Moorland) compared with reception classes. (Moorland).

WHAT: Activities available/emphasis on 'play'- more emphasis on 3Rs skills in reception, and children told what activities to do.

WHEN: More choice over when in nursery.

HOW: Greater emphasis on evaluation in reception
- Increase in 'control' of activities by the teacher. (Moorland Reception).

- Idea of 'direction'/formal. The nursery described as 'informal' situation.

OTHER SCHOOLS: Variety of patterns

WHEN: Time circumscribed/few areas of discretion.

RUSHSIDE - some classes

Different pattern Briarfield/Fairfield/Larkway

HOW: Greater choice over how: Fairfield/Larkway

Other cards were prepared which dealt in greater detail with 'particular' aspects which were only briefly referred to on the 'summary' cards. Thus in relation to 'teachers' views of pupils' there were cards which dealt with individual teachers' 'definitions of pupils', the notion of 'schooling' for example. In relation to nursery/infant school differences there were cards which dealt with 'Control of activities - what, when, how, in the nursery at Moorland and in infant classes in the schools observed. There were also cards which dealt with 'free choice' in the nursery, and infant classrooms observed, and 'The play/work distinction', for example. One example is given. This relates to 'teacher control of activities'.

CATEGORY : TEACHER CONTROL OF ACTIVITIES (Mixed: What, When, Who, How)

... M3 : Mrs. Dale: 1) CONTROL - 'WORK' (3Rs)

(See also 'Definitions of work', 'play/work distinction' and 'play in the nursery')
'Nature of 3Rs cards.

- a) Teacher decides when 'work' is done.
- b) Teacher decides what is done.
- c) Teacher decides who does what
- class/group/individual.
- d) Teacher decides amount of time spent on an activity.
- e) Evaluation of (how) activities done/ notions of 'good/bad' work plus 'ways' in which it is done. (See

'Types of Evaluation card'
for distinction between
evaluation of work/'work related
behaviour' and 'general behaviour')

- f) Rewards for 'good' work, e.g. choosing time. (See 'Types of Choice' cards, 'Differences between teachers Regarding Choice cards - Pilot Schools/Larkway.)
- 2) 'CHOOSING TIME' - This too is controlled - when - duration/and what to some extent.
- 3) Above what was observed to happen - confirmed by M3 - describes teaching approach as "more directed" - also use of team "structured". (See 'Teachers' View' - 'Nature of Teaching Approach'/Aims/Organisation. (Pilot Schools plus M1 - M4 and head - Larkway), and 'Differences Between Teachers : Notions of 'Control' and 'Formal'/Informal cards).

A similar outline was done for every teacher in every school visited. Various examples of each category were 'filed' with these cards. During the process of writing these cards items were 'cross-referenced', either in the form above, or were written in the right hand corner of the page.

While the above mentioned cards were being prepared the researcher also started to compile various cards concerned with the literature related to the themes in the research. During the process of analysis, particularly after the completion of field-work, there was an interplay between the ideas which had emerged from the data and those developed from reading the literature.

Glaser and Strauss considered that a researcher:

"should use any materials bearing on his area that he can discover."

including reading the literature. (Glaser and Strauss, 1967, p. 169). This researcher concurred with this view and considered that looking at the literature was useful for several reasons. Firstly, it was useful in order to clarify ideas on the researcher's own work and help the researcher to

formulate the themes more clearly. Secondly, the literature was useful as a source of comparison, to show how themes developed in the research related to other research in the same area. Thirdly, a reading of the literature was useful in providing a different perspective on an issue, a new way of looking at it. The perspective could be one which the researcher may or may not agree. Finally, in the researcher's view, 'other literature' could provide support for an idea which the researcher had developed.

The 'Sociology of Education Index' was used to locate information on various themes, and various journals and systematically checked to see what information was available on, for example, 'typification'. These journals included the 'American Journal of Sociology', the 'British Journal of Educational Studies', 'British Journal of the Sociology of Education', 'Educational Review', 'Human Organisation', 'Research Intelligence' and 'Sociology', to name but a few. The process was gone through several times over a long period of time, even including the 'writing up' period in order to keep 'up to date' on research in areas such as primary school practice, for example. It was an extremely time consuming activity, not least because librarians are apt to re-order the stock from time to time. Relevant articles were read and what seemed like interesting ideas were noted down in a note book, together with what appeared to the researcher as being the 'key themes', and any questions raised. This information was then transferred to cards and colour coded according to which set of 'categories', the researcher had found, it related to. Thus included under the file are 'Views of Teachers: - Children/Parents [mauve] was information from the literature on 'typification', first in order to see how it had been defined and also whether it related in any way to the researcher's own work on teachers' 'definitions of pupils'. As stated, the 'relevant' information was written on cards. The cards included information such as the author and title of the article or book, key ideas, and where ideas related to the themes in the research, and cross links to other research on related issues,

for example:

LEIGH, P. M. (1977) 'Great Expectations' CAT: Teachers'
Definitions of
Pupils

1. "Expectations are a universal phenomenon ... everyone exists within a framework of expectations"
 2. Expectations concerned with behaviour/academic ability.
[See also MURPHY, 1974 and HARGREAVES (1977)]
- O.C. Moorland children defined as poorly behaved but this is kept separate from views of 'academic ability'
- Charlie M2 - defined as 'a thoroughly bad lot', 'aggressive', 'nasty' BUT 'good at his work' - positive/negative definitions held at same time.
[See M2 Moorland : Mrs. Knowles 'Definitions of Children']

The literature cards were filed under the appropriate categories, in the above instance, under 'Teachers' Definitions of Pupils'.

The researcher was also aware from previous reading, other reading which might be useful. For example, BLACKSTONE, (1980) and QUINTON, (1980), and their work on 'Inner City Areas'. It seemed to the researcher that the Moorland catchment area strongly resembled an 'inner city' area. Thus cards were written on both these. First, Blackstone (1980):

BLACKSTONE (1980) 'THE CHALLENGE OF THE INNER CITY' CAT: DEF. OF
CATCHMENT
AREA

- Some ideas here seemed to have a bearing on own research.
- Inner city described by Blackstone as an area of deprivation/disorder by other commentators for more than a century, Booth 1902-3, Wirth (1938)
Moorland also seen as deprived - Comments also go back a long way [See also Quinton, D., 1980 - stereo-typing of inner city]

and secondly, Quinton (1980):

QUINTON, D., (1980) 'Family Life in The Inner City' CAT: DEFINITIONS
'Myth and Reality' OF CATCH-
MENT AREA/
CHILDREN

- Talks about stereotype of inner city: 'poor families'
'single mothers', unemployed families, poor quality
housing, disadvantaged families, more skilled workers
moved away - Stereotypes become fixed over time.

He states that - children in such circumstances grow up without stimulation and that the results are: poor attainment and disruptive behaviour. Moorland children described as disadvantaged. Also evidence from newspapers of definitions becoming fixed over time. Moorland Teachers also argued that children lacked stimulation at home. [See also 'Consequences of Definitions' card]

As with other cards, cross links were made to other cards.

During the course of reading the researcher often came across 'useful' literature quite by chance. Glaser and Strauss argued that "fortunate circumstances" can play a part in the library search and that the researcher can "stumble upon useful data by chance", while:

"... He is checking through the 'Readers Guide' on one topic ... happily his eye lights on another relevant topic about which he never thought of."

(Glaser and Strauss, 1967, p. 174)

Certainly there was an element of 'serendipity' in this research, when something just seemed to 'come up'. Thus in the process of searching the literature on 'typification', the researcher came across an article by Baratz and Baratz, and considered that while they referred to the 'deficient' mother, Moorland teachers seemed to refer to 'deficient' parents. At this point the children's records written by teachers were read through again in order to see whether this idea was correct. All comments referring to 'mothers' were underlined and also references to 'fathers', 'family' and 'home'.

Also during the process of searching the literature for material related to primary school practice, the researcher came across some re-search on the change over from the pre-school setting to the infant school. (Cleave et al, 1982). Their findings on differences between the pre-school setting and the infant school were found to be very similar to the differences the researcher had found between Moorland nursery and the infant school. Their findings were recorded on a card together with any links with the researcher's own data.

Apart from the cards already mentioned there were also cards which not only contained an outline of the main ideas related to a category but also ideas related to the category which had 'emerged' from reading the literature, for example:

- | <u>DEFINITIONS OF PUPILS</u> | <u>MAIN IDEAS/RELATED LITERATURE</u> |
|------------------------------|--|
| 1. | Teachers talk about nature of children - definitions relate to <u>behaviour/learning/work</u> . (See Leigh, 1977; Murphy, 1974; Hargreaves, 1977). |
| 2. | Evaluations not uniform or static. (See Leiter, 1976).
..... |
| 3. | 1) <u>Deficiency Model</u> - children (and parents) seen as <u>deficient/deprived</u> and therefore in need of ' <u>schooling</u> ' (See ' <u>schooling</u> ' card and ' <u>Notion of Competent Pupil Card</u>) |
| | 2) <u>Notion of Deficient: 'Parent'</u> (See <u>Baratz and Baratz 'Deficient Mother Hypothesis</u>) |

These cards were constantly being modified as the researcher came up with further ideas, either from re-reading the observation notes, the cards themselves, or from reading, and also after 'playing around with an idea'. As noted, this was not always possible during the field-work itself.

The next stage, having sorted the data into cards and done some

reading, was to analyse the questionnaire. This was a fairly straight-forward analysis. The questionnaire was divided into several sections as noted in the section on data collection. First, the researcher went through each questionnaire section by section and listed the responses of teachers to questions within them. Each section heading was then written on a separate card. All teachers' responses to questions within each section were then placed on these cards. The responses were 'coded' using the Likert scale, as noted in the previous section. (See also Appendix 1), for example:

<u>MOORLAND : AIMS OF INFANT EDUCATION</u>						
	<u>NT</u>	<u>M1</u>	<u>M2</u>	<u>M3</u>	<u>M4</u>	<u>Head</u>
Q. 35 The purpose of education is to encourage independence	1	5	3	4	4	5

This was done in order to be able to abstract the information and to be able to compare the responses to individual questions more easily. However, as noted in the previous section, the format of the questionnaire at Larkway was altered. Some questions were the same as the ones in the Moorland questionnaire, but not all. Where questions were the same, comparison between all the teachers at both Moorland and Larkway was possible.

All this information was then filed under 'Teachers' Views' along with the other material on this aspect.

After coding all the empirical data onto cards they were grouped into very broad general categories. These included; 'Background To The Schools', 'Heads' Views', 'Teachers' Views', and 'Classroom Practice'. Having sorted all the data onto cards into 'general themes' the next stage was to think of what the 'chapters' in the thesis might consist of and which one to write first. It had been decided, as noted in the 'overview' that it was important to provide a background for the empirical observations on schools. The reason for doing so, as with the historical dimension, was to act 'as if'

one was a phenomenologist. Schutz stated, as noted, that research should be contextualised. At the time the researcher was not clear how to go about writing this particular chapter so various studies were read in order to clarify the problem. One account which was found particularly useful, as noted, was that of Delamont (1976). She discussed the idea of 'Setting the Scene'. This seemed a useful idea to the researcher. In fact this phrase was adopted as the title of the chapter. Delamont talked about the various forms of setting including temporal and institutional aspects, as noted in Chapter Three. Having read this the researcher referred back to the cards on 'The Background To Schools' and then extracted all the information on 'temporal' aspects, such as the physical environment of the schools, and classrooms and their contents, and institutional aspects such as 'numbers of teachers in each school', 'deployment of staff, numbers of children⁸ and 'class size', for example. The cards under the heading of 'background to the schools' were then refiled under the new title of 'Setting the Scene'. The researcher considered that the chapter on 'Setting the Scene' was a good one to start with because it seemed relatively straightforward.

Having sorted the material out a draft of the above chapter was written working straight from the cards. This took two weeks to complete.

During the process of writing this first draft of 'Setting the Scene', work also began on the historical chapters, as noted in the 'Overview'.

The researcher was aware of references available in the area of Nineteenth Century elementary education. These were checked through and any references to infant school development were followed up. As a result of this reading the researcher came up with certain themes, such as 'The Notion of Social Discipline' and the 'Notion of Social Welfare' and 'Progressivism' in the Nineteenth Century, and these were written out on a

large sheet of paper. At this stage the ideas were not in any particular order, for example:

HISTORICAL THEMES

1. Teachers as agents of social control - missionary role.
2. Teachers as agents of cultural transmission - moral/social values (social and moral discipline).
5. 'Typification' of w.c. pupils/homes - children see as in need of rescue - needing orderly environment.

Vocabulary of accounts shows typifications employed, e.g. undisciplined, power of paying attention lacking ... Which groups held these views?

6. Grace - states that "progressive pedagogy" of many infant schools stands in marked contrast to the controlled uniformities of their predecessors.

O.C. BUT there are continuities as well as changes.

(Memo for History Chapter)

The next stage, as with the other material discussed, was to transfer this information onto cards, for example:

PROGRESSIVISM (Nineteenth Century)

1. - Not a strong influence.
 - Ideas of 'Great Educators' not necessarily influential.
 - There are writers who accept the influence of this tradition ...
 - No single model of progressivism.
5. ... Effects of Ideas
 - ...Progressive reformers did inform various experiments.
 - In 1850s little provision for infants.
 - Appointment of women inspectors - change of attitude towards infant schools

(Memo : Historical Development of
Infant Education : 'Progressivism')

Various other 'memos' were written on cards including: 'Training in the Nineteenth Century', 'The Notion of Social Welfare in the Nineteenth Century' and 'A Radical View of Infant Education', for example. As noted

previously, in the process of writing and filing the cards an attempt was made to summarise the ideas. In this instance the researcher tried to identify the main aims of the chapter, for example:

HISTORICAL DEVELOPMENT OF INFANT EDUCATION

INTRODUCTION

- Aim of chapter to examine taken for granted assumptions - show variety of influences/divergent viewpoints - otherwise distorted view of practice ...
- Try to show relevance of Nineteenth Century developments for infant schools today.
- Show continuity of ideas and also changes

(Memo : History Chapter)

The information filed onto cards eventually formed the basis of the first draft of the history chapters. However, before this stage was reached there was an intervening process of 'playing around with ideas' and seeing how they 'worked out'. For example, the ideas about 'social discipline' in relation to elementary and infant schools was arranged in different ways on different pieces of paper. This pattern was followed with several of the ideas. In the end there were several sheets of paper on different 'themes' and sometimes even sections. The researcher then, spread all these out on the floor, read through them and from this process prepared an 'outline' for a preliminary 'draft' of the whole chapter, and then proceeded to write this 'draft'. This procedure was followed for all the chapters, although there was a slight difference in some cases, as a 'flow diagram' (a technique learned during an Open University Course) was prepared from the sheets of paper. The following example shows a 'flow diagram' for part of a section of an early draft of Chapter Six.

OUTLINE : SHARED ETHOS, SIMILARITIES/DIFFERENCES

- A. INTRODUCTION --- PURPOSE OF SECTION --- 1) 'SHARED ETHOS' AS EXPLANATION OF SOME INFANT SCHOOLS

2) STRUCTURE OF SECTION

- B. MAIN SECTION ---- 1) DEFINITIONS ----- NOTION OF SHARED PERSPECTIVE
 - SOME PROBLEMS
- 2) CONSENSUS OR CONFLICT THOSE WHO CONSIDER SHARED ETHOS EXISTS
NATURE OF ETHOS - THE LITERATURE THOSE WHO HOLD A DIFFERENT VIEW
- 3) CONFLICT/CONSENSUS LINK BACK TO LITERATURE
 - SITUATION IN SCHOOLS OBSERVED
- SHARED ETHOS - ALL SCHOOLS
1. DIFFERENCES BETWEEN MOORLAND NURSERY AND INFANT SCHOOL (MOORLAND RECEPTION)
 2. DIFFERENCES BETWEEN TEACHERS - MOORLAND
 3. DIFFERENCES BETWEEN SCHOOLS
 4. VIEWS OF HEADS/TEACHERS - AREA OF DISAGREEMENT
4. FACTORS WHICH INFLUENCE FORMATION OF ETHOS/ ----- INTEGRATION OF LITERATURE/ OBSERVATIONS ----- OBSERVATIONS
 - CATCHMENT AREA/NATURE OF CHILDREN
 - HEAD TEACHER

(Flow Diagram to Show Development of Idea)

Even when this draft stage was reached, considerable re-ordering and restructuring took place. For example, references to ideology and 'shared

ethos' were taken out of their original place in Chapters Four and Five as they seemed to muddle the flow of ideas presented.

In fact several drafts of each chapter were written before a final one was achieved which seemed to reasonably express what the researcher intended. The basic problem here, as with the thesis as a whole, was juggling several ideas which could be ordered in several different ways, each one making its own kind of sense. The task in this stage of analysis is to bring the mass of information on the cards into some related whole which bears some relation to the often vague ideas about what the researcher really wants to say. The problem of trying to bring together a mass of information and ill-formed ideas was particularly acute in relation to the analysis of the procedures of analysis. This, in fact took well over twelve months to sort out.

At every stage of producing drafts there were scribbled notes written in the margins, amendments, additions and suggestions. The problem of arranging ideas was worst in the methodology because there were so many. The sheets of paper used in this chapter before producing a draft covered two whole walls.

The same procedure of ordering and re-ordering applied to the order of chapters even where a reasonable draft had been achieved. Several ways were considered before the final order was arrived at.

Thus analysis continues right up to the very final stages of 'writing up'. In practice, analysis and writing go hand in hand.

It has been indicated that 'writing up' is difficult, to say the least. Some general remarks on the process of writing up by the other writers reinforces the conclusion. These also show it to be, as the researcher found it, a difficult process. Barzun and Graff (1970) and Woods (1986) both express views about the process of writing, and writing up. The former argued

that writing was similar to:

"doing sculpture from memory in that neither had a concrete visible object to copy directly. The subject is hidden in the block of material."

(Barzun and Graff, 1970, p. 23)

This seemed an apt simile to the researcher.

Woods argued that the 'writing up' process was a difficult one because ideas do not "simply emerge", or are "immediately revealed". (Woods, 1986, p. 147). This researcher considers this to be an understatement and that ideas have to be dragged, kicking and screaming from the increasingly reluctant brain (particularly at 2.30 in the morning). It was certainly the case for this researcher that it seemed to take a long time before ideas came together and when there seemed some order to the material. Barzun and Graff, however, do not appear to find anything odd about this 'slowness' and considered that:

"you are choking the car so a spluttering is of no consequence."

(Barzun and Graff, 1970, pp. 382-3)

There were times when the researcher felt that the problem went a little deeper than this and that instead there was something basically wrong with the transmission.

Some of the problems mentioned above were noted in the researcher's diary:

"Have been working on the 'Practice in the classroom' chapter for months now but there are problems in trying to organise the material and nothing seems to hang together. The main themes do not stand out and it appears muddled - but cannot pinpoint the source of the muddle - making no progress at all ...!"

(Research Diary, 1987)

The researcher needs a certain degree of stamina and sheer determination to 'get through' the 'writing up' stage, and an ability to be able to cope with the difficulties as noted by Woods (1986). There were times,

however, especially during the final stages of 'writing up' when determination did waver and 'faith in one's ability' to complete the task were a little thin. There were many moments when it was impossible to imagine that the research would ever be completed. There were times when the researcher felt discouraged and it was heartening to read that even great writers such as Trevelyan, the historian, suffered similar periods of depression during writing. (Barzun and Graff, 1970).

Of all the procedures of ethnographic research, that of 'writing up' is the worst. Analysis is often categorised as 'messy', as noted previously. From experience, this researcher considers that the 'writing up' stage is the messiest of all. What comes out as a relatively orderly presentation of ideas is arrived at by a mixture of blood, sweat and tears, not to mention 'profanity' and throwing the whole collection over the room like one of the infant children having a classroom tantrum!

Overall, this section has been concerned to show how analysis was carried out and how the written material presented in the thesis was derived from 'raw data' of various kinds.

It discussed ideas of other writers on analysis first, showing how ideas about analysis were developed, and then showed in detail, the 'technical' procedures actually used.

In general the conclusion is that analysis is a very difficult stage of the research process, and one with few signposts for the researcher travelling through it. However, the researcher has tried to give enough evidence of the way it was done to enable a reader to follow her journey.

The final section of this chapter deals with personal reflections.

SECTION EIGHT : PERSONAL REFLECTIONS

Doing research is a considerable undertaking whatever its form. It is a process which has its highs and lows, satisfactions and sorrows, triumphs

and despairs. No account of research can adequately convey what it actually feels like at the time, it is something which has to be experienced. When in the middle of research, one is too 'busy' to reflect much on what is happening on a personal level, or on the methods in use except in a pragmatic sense of what is appropriate at a particular time, or the technical aspects of the particular method at that time. Only when the dust has settled do some more objective general comments come to mind.

The 'Overview' or time scale presented in this chapter has outlined the personal circumstances of the researcher during the research, and pointed to the stress these provoked. Therefore, these reflections are concerned mainly with the problems of 'interpretive' or 'ethnographic' research.

Each of the methods used in such research has its own problems, as has been shown when discussing them. However, the combination of them does tend to correct the biases which may distort any one individually. Nevertheless there are difficulties associated with interpretive research quite apart from questions of validity. For one thing, it places heavy demands on the individual researcher, both psychologically and in terms of time, in the efforts to build up a 'thick' description. It is a process which is greatly dependent for its effectiveness on both the personality of the researcher and the degree of interpersonal skills possessed. Also, physically, it is an extremely tiring process. It is exhausting concentrating on what is being seen or heard. The writing up of notes, constant checking of these for anything puzzling, or any gaps, is also demanding.

As the research proceeds, alertness tends to decrease because of this tiredness. When this is exacerbated by problems external to the research, as well as any within it, the cumulative effect is that at the end of the field-work the researcher can feel both physically and emotionally exhausted.

This researcher certainly felt that way. Little did she know that the tiredness induced by field-work was as nothing compared to that produced by the problems of ordering the data and, in particular, those of 'writing up'.

With these issues in mind, this researcher offers these comments.

The first is that she feels that Lutz is perhaps correct in stating that 'ethnographic' research needs careful training and preparation.

"particularly participant observer field methods."

(Lutz, 1986, p. 113)

There are more books now available on the process and problems of 'ethnographic' research than when the researcher began field-work, although no book can adequately prepare, it can only indicate a few possible pitfalls. Nevertheless some more realistic accounts are now produced of what was done in the field. However, these do not discuss the prior training, if any, of the researchers.

The researcher did her best, as others do, 'muddling through' when in doubt. It is now considered that it would have been useful if, for example, a system had been in operation allowing for mock observations, interviews and recording of data, in order to show up gaps in skills, including personality deficiencies. Such simulation exists in many fields, notably social work training, counselling and some business management courses, as shown on educational television.

Such training and preparation might help to overcome the drop out problem for sociological higher degree research. In this connection, some uniformity in supervisory practices could also help in the training of researchers. If the implicit contract between the university and the research student in this respect were clearly spelled out, there would be fewer grounds for misunderstanding later. As Phillips and Pugh indicated, supervisors and students at present may have different expectations of the relationship. (Phillips, E. M., and Pugh, D. S., 1987, pp. 82-120).

The second comment is that 'interpretive' research could be more of a joint effort. This could be beneficial for two reasons. One, it would place less strain on the available resources (of all kinds) of an individual researcher. Second, the presence of another researcher could help to bring in more checks and balances. Another pair of eyes and ears, and a different personality, might pick up what the other missed or glossed over, or establish relationships where the other found difficulties. The presence of another student in one of the research schools for part of this research brought out this feature. Thus, skills could be amplified. Research in science may sometimes be shared in this way, and also forms of 'survey' research.

The third comment is partly related to the previous one. While 'interpretive' or 'ethnographic' research can throw valuable light on the internal processes of organisations such as schools, it is, because of the time needed to build up relationships, by its nature a lengthy process. It is one that fits uneasily into the 'standard format' of three year research degrees.

Apart from these issues, there are ethical problems in doing an 'ethnographic' study, because the research demands and human feelings can conflict at times. For example, it was difficult not to sympathise with those teachers who were themselves facing problems. Yet the research process demands not necessarily 'objectivity' but certainly a critical eye, not taking things for granted nor accepting statements at face value. Data has to be found or there is no report possible. This in a sense entails 'ripping off' respondents for no visible benefit to them. This can and did produce feelings of guilt about the whole enterprise.

Scientists studying the behaviour of atoms do not have this problem.

On the other hand, some people appeared to find it beneficial to have someone relatively 'neutral' to chat to, or to be able to explain ideas and

practice to an interested observer. So there are swings and roundabouts, and perhaps some respondents gain something.

Another problem in this form of research is that a researcher always has to be aware of personal likes and dislikes, inevitable in personal interaction. As stated when discussing personality and validity, the researcher did experience problems in this area. It would have been nice to be able to 'blow off steam' about this. Instead, the researcher kept it mostly bottled up, and felt it was her fault. Some would have been, perhaps, but not all.

Writers talk about the dangers in 'ethnographic' work of 'going native' as if this, getting too close to events, was the only problem. However, this researcher while in the main school felt unable to get close enough, and at the same time got too close to her own problems to be able to judge them effectively and put them into perspective. Again, more training or experience might have enabled her to see them as perhaps not arising just from her own weaknesses but also because she was perhaps getting too close to delicate issues in the school. Her then supervisor in fact suggested this.

As said before, the research process makes severe demands on the individual researcher. As noted in the Overview, this research was dogged by periods of illness, some of them resulting from long-standing problems, others the consequence of being extremely tired and therefore perhaps vulnerable to infection. The researcher's personal situation was also somewhat precarious. Also, quite apart from any physical factors, the research at Moorland induced feelings of acute depression, of extreme reluctance to go on, and a sense of failure. The process of 'writing up' the data recreated those feelings, compounded by the realisation that mistakes had been made and could not be rectified.

However, the research process was not always a scene of gloom and despondency.

In the pilot study schools, the researcher felt cheerful and quite excited. Larkway was actively enjoyed, the feeling of release from tension there was palpable, so that the researcher rediscovered her nerve and initial enthusiasm for the enterprise.

Even at Moorland some people were found helpful.

In assessing the effect of these personal ups and downs, the researcher can only say that she honestly tried not to allow feelings to affect her research, either in presenting too critical a picture or too rosy a one, but did try to stand back and view events as even-handedly as possible, remaining aware of her role as researcher.

It is the need for constant checking of one's own subjective impressions, as well as taking account of the subjective meanings and intentions and impressions of others, as far as can be ascertained, while at the same time keeping a relatively objective stance towards events, which makes 'interpretive' research difficult. Also, long term research, far from allowing time to overcome initial problems and get past attempts to present 'fronts', may cause these to intensify if the researcher is seen as a threat, or lacks the experience to find ways of overcoming problems.

The same alternations of depression and excitement characterised the 'writing up'. As stated, this was a horrendous experience. The early sorting of data was quite pleasant, and organisation into broad themes not too difficult. As noted in discussing analysis, it was the actual writing of chapters, and the ordering of the thesis which was in fact the worst part of the analytical process. In the case of theories and methods, which was tackled last, there were difficulties because the researcher had no clear idea of what was expected. However, in spite of all the difficulties, which at times brought the researcher near to despair, there was still a real thrill and satisfaction when ideas were finally put into a form which seemed clear and said what the researcher wanted to say.

Altogether, the research process undergone was a switchback ride, at times exhilarating, at times chastening, and at times downright frightening.

CONCLUSION

While other chapters of this thesis are concerned in different ways with presenting the empirical findings of the research, this chapter has been devoted to a discussion of those concerns which underpin the empirical. It has thus been concerned to present the theoretical and methodological aspects of the research, to show as fully as possible why, and perhaps more importantly, how the research was done.

What the chapter set out to do was show why the research was undertaken in the first instance, and the ideas which were initially held about forms of research in terms of methods, and the ideas thought to be related to these.

In theoretical terms, this chapter has attempted to show the study of theory, or rather theories, as a learning process, showing how ideas were developed in relation to reading, rather than as the process of developing a particular theory, and to indicate which were found useful in the research.

In terms of methods, it has been concerned to show in some detail both the ideas held about 'how to do research' in the form chosen initially, and the actual carrying out of the research. It has tried to show what was done, and why, and the efforts to try and ensure the validity of the empirical data presented, so that the reader may assess the degree to which the conclusions are justified.

As stated earlier, the researcher did not have any particular theory/hypothesis in mind when the research began, nor any particular view about 'society'.

However, perhaps because of something inherent in her nature, the researcher did have, from the outset, an instinctive dislike of being put into

a particular category. Also, because of the reading of the literature, although she was critical of some aspects of certain forms of research, she also came to feel that to dismiss work not done within a particular approach, just because of this, was not right. Useful ideas could be gained from other approaches.

So, quite early in the research process, there was a concern to explore further the theoretical background of the research approach chosen, along with reading in the 'other' tradition, in order to extend knowledge. Section Two of this chapter tried to show how this reading was 'made sense of' in a learning process.

This process not only led to a discovery of diversity within the 'interpretive' approach, but also of disagreements within the other about 'positivism' and 'the scientific method'. There was also a recognition of similarities within the supposed 'scientific' tradition with some aspects of the 'interpretive'.

These discoveries led the researcher to certain conclusions about the relationship of theory and the choice of methods.

First, those 'new sociologists', who were critics of 'interpretive' and 'the scientific method', and who wrote of 'interpretive' research as if it was based on a completely separate theoretical standpoint and used completely separate methods, were mistaken.

This conclusion was reached because 'positivism' has been shown to be an ambiguous term, and one referring to different things at different times. Also, 'the scientific method' associated with it by its critics was equally ambiguous. Moreover, not only did scientists disagree about what this method was, but some also rejected the idea that 'understanding', and taking account of the perceptions influencing scientists, was not a part of science.

Moreover, whatever 'scientific method' was, it did not preclude forms of research which were often associated with the 'other' tradition, namely, 'participant observation'. Also, it was found that much work of value to both 'traditions' had been done in 'survey research' on the problem common to both approaches, of trying to discover what people 'really' think.

In the 'interpretive' tradition, it was found that those who were heralded by the 'new' sociologists as intellectual gurus, such as Husserl and Schutz, were both as equally concerned to be 'objective' and 'scientific' as any 'positivist', though concerned with 'understanding' the motives of actors.

Therefore, arising from reading, what the researcher 'understood' was the idea that 'ethnographic' and 'scientific' methods were not mutually exclusive terms.

Secondly, this learning process led to the conclusion that there is nothing wrong with being eclectic as to methods. A pragmatic rather than a dogmatic approach to research problems seems more likely to be fruitful than a rejection of one method or another just because of its alleged theoretical 'tainting'. In the 'interpretive' field in particular, it has been concluded that the latter position is untenable anyway, because 'ethnographic' field-work was basically derived from social anthropology, which was heavily dominated by 'structural functionalism', which itself was associated with positivism.

After looking at the supposed 'two' traditions, the remainder of the chapter discussed the actual methods used in this research. It was found that while there may be problems with 'survey research' in getting at 'what really happens', or 'meanings', 'ethnographic' research is by no means free of similar problems. For example, although 'personality' factors enter into any research situation, it was found in the researcher's experience that the

research form actually used is very much affected by these. The success or failure of these methods, used over a longer time than in many surveys, is heavily dependent on the personality as well as the ability of the researcher, and the 'participant' method particularly makes considerable psychological demands on the researcher, who is generally working alone. Where personality clashes occur between the researcher and any of the researched, this can perhaps affect validity, as was indicated.

In discussing the methods of observation, interviews and the use of documents, the researcher tried to show how and from what sources ideas about 'how to do research' were gathered, and what use was made of them, as well as how the researcher was alerted to certain problems. It was pointed out that there were few guidelines when the research began. Therefore, the individual methods were first discussed in terms of reading, then the development of ideas, and their application.

It has been pointed out that all the methods could give rise to bias. In the case of interviews, this could arise from the personal interpretations each of the participants brought to the interview situation, and from reactions within it. Observations could similarly be affected by subjective factors. With documents, it was pointed out that there was no way in which the intentions of the writer could always be known, and assessment could therefore be difficult.

The procedures for recording data have been given. The problems in this area, particularly when a tape recorder was not used, were pointed out. These were first, the difficulty of recording everything, hence the need for 'focusing' on particular aspects. Bias was possible in such choice. There was also the question of perceptual mistakes, whether the things observed were actually seen or heard. Finally there was the problem of interpretation, which is a subjective process.

In discussing validity, it was shown that all these factors could

affect the degree to which reliance could be placed on the empirical findings. It has been shown what efforts were made to overcome bias, in particular 'triangulation'. Not only were different methods used, but it was shown that efforts were made to discuss the same events with different participants. Efforts were also made, as shown, to 'check' ideas with participants. However, it has been indicated that where there is disagreement over a reported event between researcher and researched, there is ultimately no way of resolving the argument. The conclusion was that all that could be done, in terms of validation, was to endeavour to be as honest as possible in describing the situation, as the researcher has tried to be when discussing methods, showing the problems as well as showing how attempts were made to overcome these.

As noted, few guidelines were available about the procedures, particularly the analysis of data. In discussing this aspect of the research, the conclusion has been that it is a continuous process right from the first observations up to and including the writing up, and is also not a linear process. It has been pointed out that it may often be difficult to decide how an idea was developed. The influence of 'serendipity' on this was indicated. This part of the chapter showed in detail the actual procedures used during analysis of the data, believing this to be useful.

The remaining chapters are concerned in one way or another with presenting the empirical findings, although the next two are slightly separate. The one immediately following, 'Setting the Scene', gives a glimpse of the infant schools seen, both internally in terms of the school layout and externally in terms of the catchment area. The following chapter, although derived from the empirical work, is abstracted for reasons of clarity.