

justify them without placing reliance on futile appeals to empiricism, of either positivist or naturalist varieties.

Reconstructing our understanding of social research in line with the implications of its reflexivity also throws light on the relationship between quantitative and qualitative approaches. Certainly there is little justification for the view, associated with naturalism, that ethnography represents a superior, alternative paradigm to quantitative research. On the other hand, it has a much more powerful contribution to make to social science than positivism allows.

Reflexivity is an aspect of all social research. It is one that has been given increasing attention by ethnographers and others in recent years, notably in the production of 'natural histories' of their research. (For examples of such natural histories, see Hammond 1964; Freilich 1970b; Bell and Newby 1977; Shaffir *et al.* 1980; Hammersley 1983a; Bell and Roberts 1984; Burgess 1984b, 1985a and b, 1988a, 1989, 1990 and 1992; Golde 1986; Whitehead and Conaway 1986; McKeganey and Cunningham-Burley 1987; Walford 1987 and 1991b; Shaffir and Stebbins 1991; Okely and Gallaway 1992.) The remainder of this book is devoted to spelling out what we take to be the implications of reflexivity for ethnographic practice.

Chapter 2

Research design: problems, cases, and samples

At first blush, the conduct of ethnography can seem deceptively simple. Indeed, some authors have reported being given little or no research advice before they set out on their fieldwork. Nader, for example, relates how at one time this had become a tradition among North American anthropologists:

Before leaving Harvard I went to see Kluckhohn. In spite of the confidence I had gained from some of my training at Harvard, this last session left me frustrated. When I asked Kluckhohn if he had any advice, he told the story of a graduate student who had asked Kroeber the same question. In response Kroeber was said to have taken the largest, fattest ethnography book off his shelf, and said, 'Go forth and do likewise.'

(Nader 1986:98)

Such non-advice seems to rest on the assumption that the conduct of ethnography is unproblematic, and requires little preparation and no special expertise.

One of the reasons for this reluctance to give advice about how to do ethnographic research is awareness of the fact that such research cannot be programmed, that its practice is replete with the unexpected, as any reading of the many published research biographies now available will confirm. More than this, all research is a practical activity requiring the exercise of judgment in context; it is not a matter of simply following methodological rules.

There is, however, another, less legitimate reason why the advice given to those about to embark upon ethnography is often simply to 'go and do it'. This is 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. Speaking of the study of animal behaviour, Tinbergen (1972:23) remarks that periods of exploratory, intuitive observation are of particular value 'when one feels in danger of getting out of touch with the natural phenomena, of narrowing one's field of vision'. Naturalists in sociology have sometimes appealed to natural history and ethology to legitimate their recommendation of exploratory observation and description (Lofland 1967; Blumer 1969; Speier 1973). It is important to remember, though, that observation in ethology is guided by a relatively well-defined set of assumptions derived from evolutionary theory. Darwin (quoted in Selltitz *et al.* 1959:200) himself remarks at one point: 'How odd it is that anyone should not see that observation must be for or against some view, if it is to be of any service.'

Certainly we must recognize that, even less than other forms of social research, the course of ethnography cannot be predetermined. But this neither eliminates the need for pre-fieldwork preparation nor means that the researcher's behaviour in the field can be haphazard, merely adjusting to events by taking 'the line of least resistance'. Indeed, we shall argue that research design should be a reflexive process which operates throughout every stage of a project.

FORESHADOWED PROBLEMS

Research always begins with some problem or set of issues, from what Malinowski refers to as 'foreshadowed problems':

Good training in theory, and acquaintance with its latest results, is not identical with being burdened with 'preconceived ideas'. If a man sets out on an expedition, determined to prove certain hypotheses, if he is incapable of changing his views constantly and casting them off ungrudgingly under the pressure of evidence, needless to say his work will be worthless. But the more problems he brings with him into the field, the more he is in the habit of moulding his theories according to facts, and of seeing facts in their bearing upon theory, the better he is equipped for the work. Preconceived

ideas are pernicious in any scientific work, but foreshadowed problems are the main endowment of a scientific thinker, and these problems are first revealed to the observer by his theoretical studies.

(Malinowski 1922:8-9)

Sometimes the starting point for research is a well-developed theory from which a set of hypotheses can be derived. Such theories are relatively rare in sociology and anthropology, but perhaps more frequent in social psychology. An example of a participant observation study in this mould is the work of Festinger *et al.* (1956). They tested cognitive dissonance theory by investigating the reaction of members of an apocalyptic religious group to the fact that the world did not end on the day predicted by their leader.

Most ethnographic research, however, has been concerned with producing descriptions and explanations of particular phenomena, or with developing theories rather than with testing existing hypotheses. A number of authors, most notably Glaser and Strauss (1967), have pointed to the advantages to be gained from developing theory through systematic data collection rather than by reliance on 'armchair theorizing'. Nevertheless, as Strauss (1970) himself has shown, considerable progress can sometimes be made in clarifying and developing research problems before fieldwork begins. As an illustration he examines Davis's (1961a) research on 'the management of strained interaction by the visibly handicapped':

Davis's theory is about (1) *strained* (2) *sociable* interaction (3) in *face-to-face* contact between (4) *two persons*, one of whom has a (5) *visible handicap* and the other of whom is (6) *normal* (no visible handicap). . . . The underlined terms in the above sentence begin to suggest what is explicitly or implicitly omitted from Davis's theoretical formulation. The theory is concerned with the visibly (physically) handicapped, not with people whose handicaps are not immediately visible, if at all, to other interactants. The theory is concerned with interaction between two people (not with more than two). . . . The interaction occurs in situations termed 'sociable'; that is, the relations between interactants are neither impersonal nor intimate. Sociable also means interaction prolonged enough to

permit more than a fleeting exchange but not so prolonged that close familiarity ensues.

(Strauss 1970:47-8)

Strauss goes on to show that by varying these different elements of the theory new research questions can be generated.

Often the relevant literature is less developed even than in the case referred to by Strauss. However, the absence of detailed knowledge of a phenomenon or process itself represents a useful starting point for research. MacIntyre (1977) provides an example in her study of the 'pregnancy careers' of single women:

Approximately one fifth of all conceptions, and an even higher proportion of first conceptions, in Britain in the early 1970s were to single women. There were four common outcomes of pregnancy for single women: marriage to the putative father; induced abortion; remaining single and keeping the baby; and remaining single and giving the baby up for adoption. It is known that the incidence of these outcomes has changed from time to time, as have, of course, the relevant social attitudes, social policy and legislation, and these have been the subject of demographic and historical studies. *Yet little is known about how these outcomes are reached, or how these may be affected by social attitudes, policies and legislation.*

(MacIntyre, 1977:9; our emphasis)

Alternatively, the stimulus may be a surprising fact or set of facts. Thus, Measor (1983) noted that not only did girls tend to fare worse than boys in science examinations, but that the gap was even greater in the case of the Nuffield science course, a course emphasizing discovery learning. She set out to investigate why this was the case through participant observation in Nuffield science lessons and by interviewing both boys and girls about their attitudes to these lessons.

As this example illustrates, the significance of the initial problem may be not so much theoretical as political or practical. Even where the starting point is not current social theory, however, elaboration of the problem soon draws such theory in, as Freilich's work on 'Mohawk heroes' indicates:

New Yorkers sometimes read in their newspapers about a unique phenomenon in their midst: the Mohawk Indians who

work on the steel structures of various buildings in and around their city. Articles, at times accompanied by pictures of smiling Indians, discuss these 'brave' and 'sure-footed' Mohawks. The question of why so many Mohawks work in structural steel is one that is often researched by students enrolled in colleges located in and around New York. In 1956, this problem was, in fact, my first professional research assignment. I used A.F.C. Wallace's paper 'Some Psychological Determinants of Culture Change in an Iroquoian Community' as the foil in my proposal for research support. Wallace's paper suggested that Mohawks lack a fear of heights, and that this lack of fear explains their involvement with the steel industry. I argued that a negative trait (lack of fear) cannot have specific positive consequences (lead a tribe into steel work). I argued further that there is no functional value in a lack of fear of heights for steel work, and that in actuality the opposite is true: a normal fear of high places leads to caution that saves lives. A more plausible argument seemed to be that Mohawks frequently act as if they have no fear of heights. In presenting a subsidiary problem, 'Why these acts of daredevilry?', I put forth my theoretical belief that socio-cultural factors explain social and cultural phenomena better than do psychological factors. I had a vague notion that Mohawks in steel work represented some kind of cultural continuity. Thus, the questions I posed were (1) why is it good, culturally, for a Mohawk male to be a structural steel worker? and (2) How does such a cultural 'goodness' relate to Mohawk cultural history?

(Freilich 1970a:185-6)

Social events themselves may also stimulate research, providing an opportunity to explore some unusual occurrence or to test a theory. Notable here are what are sometimes called 'natural experiments': organizational innovations, natural disasters, or political crises that promise to reveal what happens when the limiting factors that normally constrain a particular element of social life are breached. At such times social phenomena that are otherwise taken for granted become visibly problematic for the participants themselves, and thus for the observer. Schatzman and Strauss (1955) provide an example in their discussion of the problems of inter-class communication arising subsequent

to a tornado. Studying the origins and consequences of organizational innovations is even more common. An example is Walford and Miller's study of Kingshurst School, the first City Technology College in Britain, established as part of the educational reforms of the late 1980s (Walford 1991a; Walford and Miller 1991).

Even chance encounters or personal experiences may provide motive and opportunity for research. Henslin came to do research on the homeless as a result of meeting someone for whom the problem of homelessness had become a consuming passion:

When [he] found out that I was a sociologist and that I was writing a textbook on social problems, he asked me to collaborate on a book about the homeless. He felt that my background might provide an organizing framework that would help sort out his many experiences and observations into a unified whole. During our attempt at collaboration, he kept insisting that as a sociologist I owed it to myself to gain first-hand experience with the homeless. Although I found that idea somewhat appealing, because of my heavy involvement in writing projects I did not care to pursue the possibility. As he constantly brought up the topic, however, I must admit that he touched a sensitive spot, rubbing in more than a little sociological guilt. After all, I was an instructor of social problems, and I did not *really* know about the homeless. . . . With the continued onslaught, I became more open to the idea. (Or perhaps I should say that I eventually wore down.) When he invited me on an expense-paid trip to Washington, DC, and promised that I would see sights hitherto unbeknownst to me – such as homeless people sleeping on the sidewalks in full view of the White House – firing my imagination, he had pierced my armor through. With the allure of such an intriguing juxtaposition of power and powerlessness, of wealth and poverty, how could I resist such an offer?

(Henslin 1990:52)

By contrast, Curren (1992:4–5) began her research on Pathan mothers in Britain as a result of her own experience as an English mother in Peshawar, Pakistan. Her research questions arose initially from what she saw as the parallels between her own former position and that of the people she chose to study, and from her sympathy for them. It is also common for research

to be stimulated by previous experience in temporary or permanent jobs. Thus Olesen traces the origins of her research on temporary clerical workers to her own experience supporting herself as a student by working in a typing pool (Olesen 1990:214). Of course, research interest may equally arise from difference, conflict, and negative feelings. Van Maanen (1991:33) reports that his long career investigating police culture began in part because he had been 'subject to what I regarded as more than my fair share of police attention and hence viewed the police with a little loathing, some fear, and considerable curiosity'.

Stimuli such as these are not usually sufficient in themselves for the formulation of a research problem. For this to occur, experiences prior to entering the field must be subjected to analytic reflection. Experiences are rendered interesting or significant by theoretical ideas: the stimulus is not intrinsic to the experiences themselves. However, there are no hard-and-fast rules for deciding how far the initial research problem can be elaborated before the collection of data begins. Exploring the components and implications of a general foreshadowed problem with the help of whatever secondary literature is available is certainly a wise first step. Relevant here are not only research monographs and journal articles but also official reports, journalistic exposés, autobiographies, diaries, and 'non-fiction novels' (see Chapter 6). There comes a point, however, when little more progress can be made without beginning the collection of primary data – though reflection and the use of secondary literature should of course continue beyond that point.

THE DEVELOPMENT OF RESEARCH PROBLEMS

The aim in the pre-fieldwork phase and in the early stages of data collection is to turn the foreshadowed problems into a set of questions to which an answer can be given, whether this be a narrative description of a sequence of events, a generalized account of the perspectives and practices of a particular group of actors, or a more abstract theoretical formulation. Sometimes in this process the original problems are transformed or even completely abandoned in favour of others, as Dollard illustrates:

My original plan was to study the personality of Negroes in

the South, to get a few life histories, and to learn something about the manner in which the Negro person grows up. It was far from my wish to make a study of a community, to consider the intricate problem of the cultural heritage of the Negro, or to deal with the emotional structure of a specific small town in the deep South. I was compelled, however, to study the community, for the individual life is rooted in it. Only a few days of five months in Southern town had passed before I realized that whites and whiteness form an inseparable part of the mental life of the Negro. He has a white employer, often white ancestors, sometimes white playmates, and he lives by a set of rules which are imposed by white society. The lives of white and Negro people are so dynamically joined and fixed in one system that neither can be understood without the other. This insight put an end to the plan of collecting Negro life histories in a social void. Negro life histories refer at every point to a total situation, i.e. to Southern town itself, the surrounding county, the southeastern culture area, and in a strict sense the whole region which is bound to American cotton economy. This observation came as a very unwelcome perception, since it necessitated getting a perspective on the community and the county, and informing myself incidentally on many apparently remote matters. Study of the social context of the lives of Negroes has crowded out the original objective of the research, as least so far as the publication of specific life histories is concerned.

(Dollard 1957:1-2)

Change in research problems stems from several different sources. As with Dollard, it may be discovered that the original formulation of the problem was founded on erroneous assumptions. Equally, it may be concluded that, given the current state of knowledge, the problem is not soluble. Medawar comments:

Good scientists study the more important problems they think they can solve. It is, after all, their professional business to solve problems, not merely to grapple with them. The spectacle of a scientist locked in combat with the forces of ignorance is not an inspiring one if, in the outcome, the scientist is routed. That is why some of the most important biological problems have not yet appeared on the agenda of practical research.

(Medawar 1967:7)

Periodically, methodologists rediscover the truth of the old adage that finding the right question to ask is more difficult than answering it (Merton 1959). Much of the effort that goes into data analysis is concerned with formulating and reformulating the research problem in ways that make it more amenable to investigation.

Problems vary in their degree of abstractness. Some, especially those deriving from practical or political concerns, will be 'topical' (Lofland 1976), being concerned with types of people and situations readily identified in everyday language. Others have a more 'generic' cast. Here the researcher is asking questions such as 'Of what abstract sociologically conceived class of situation is this particular situation an instance?' and 'What are the abstract features of this kind of situation?' This distinction between topical and generic research problems is closely related to the distinction between substantive and formal analyses outlined by Glaser and Strauss:

By substantive theory, we mean that developed for a substantive, or empirical, area of sociological inquiry, such as patient care, race relations, professional education, delinquency, or research organizations. By formal theory, we mean that developed for a formal, or conceptual, area of sociological inquiry, such as stigma, deviant behaviour, formal organization, socialization, status incongruency, authority and power, reward systems, or social mobility.

(Glaser and Strauss 1967:32)

In ethnographic research there is frequently a constant interplay between the topical and the generic, or the substantive and the formal. One may begin with some formal analytic notion and seek to extend or refine its range of application in the context of a particular new substantive application. This can be illustrated by reference to the work of Hargreaves *et al.* (1975) on deviance in school classrooms. Starting from the formal concepts of 'labelling theory', Hargreaves and his colleagues sought to extend the use of this analytic framework to, and examine its value for, the study of student deviance in secondary schools. They were able to derive from it a sort of 'shopping list' of issues. This list of topics moves the focus of concern from the formal towards the substantive, from the generic towards the topical. Their list reads:

Rules. What are the rules in schools and classrooms? Which rules are allegedly broken in imputations of deviance? Who makes the rules? Are the rules ever negotiated? How are the rules communicated to members? What justifications are given for the rules, by whom, to whom, and on what occasions? Do teachers and pupils view the rules in the same way? Are some rules perceived as legitimate by some teachers and some pupils? How do members know that certain rules are relevant to (i.e. are 'in play' in) a given situation? How do members classify the rules? What differences do members see between different rules? For example, do rules vary in importance?

Deviant acts. How do members link an act to a rule to permit the imputation of deviance? How do teachers know that a pupil has broken a rule? That is, what is the interpretive work undertaken by teachers to permit the categorization of an act as deviant? Similarly, how do pupils know that their acts are deviant? ...

Deviant persons. How do teachers link deviant acts to persons so that persons are defined as deviant? What is the relationship between different labels? Why is one label used rather than another? ...

Treatment. What treatments are made by teachers in relation to acts or persons defined as deviants? On what grounds and with what justifications do teachers decide on one treatment rather than another? ...

Career of the deviant. What is the structure of the career of the deviant pupil? What are the contingencies of such careers? How are such careers initiated and terminated?

(Hargreaves *et al.* 1975:23-4)

Such a list of problems clearly draws on the authors' prior knowledge of sociological work on schools and deviance, and reflects an interplay between formal and substantive interests. Of course, these questions do not constitute a research design as such. Similarly, one would not expect such a list to be a definitive one: in some ways it would probably prove to be over-ambitious, and in others it would undoubtedly omit unforeseen problems.

One can also develop research problems by extending the use of an analytic framework from one substantive area to another.

This is one feature of the classic study of the Kansas medical school by Becker *et al.* (1961). They adopted a perspective from industrial sociology – that industrial workers attempt to set their own 'level and direction of effort' – and applied it to the topical situation of the medical students, who, faced with overwhelming academic demands, likewise attempt to negotiate manageable levels of effort, and to establish appropriate directions for their efforts.

Just as one can formulate problems by moving from the formal to the substantive, so one can move from the substantive to the formal or generic. This can be illustrated in part from a research project in which one of us was involved (Atkinson 1981b). It was concerned with the investigation of 'industrial training units' designed to ease the transition from school to working life for 'slow learners'. The research included a number of strands, including participant observation in two such industrial units, interviews with a range of officials, documentary sources, and so on. The project was not simply a 'one-off' case-study, but one of a number of similar pieces of research being undertaken in Britain. These other projects were also investigating innovative interventions to facilitate the transition from school to work.

The formulation of the interests of the research began with foreshadowed problems that were primarily substantive or topical in origin. In an exploratory orientation, the research team began the fieldwork phase with general interests of this sort: How is the day-to-day work of the unit organized? How are the students selected and evaluated? What sort of work do they do, and what sort of work are they being prepared for?

During the course of the fieldwork a number of issues were identified with more precision, and new categories were developed. At the same time, it became apparent that there was a need to formulate these ideas in terms that were more general than their local manifestations in our own project. A more pressing reason for this was the desirability of generating concepts that would permit of principles for, and systematic comparison between, the different research projects in Britain. A research memorandum put the issue in this way:

During our last meeting ... we talked about the possibility of developing and working with some general analytic

categories. The idea I was putting forward . . . was that evaluation projects were doomed to be little more than one-off, local affairs, unless we were able to work with ideas and frameworks of more general applicability. Such 'generalization' would not imply that all projects should work within 'the same' research design, or collect 'the same' data by 'the same' technique. Clearly, particular evaluations must remain sensitive to local conditions and responsive to changing circumstances. Nor should such a suggestion be interpreted as a plea for a straitjacket of predetermined questions and categories. Such categories should only be thought of as 'sensitizing' concepts – indicating some broad dimensions for comparison between projects, and for the development of general frameworks to tie together disparate projects and evaluation.

(Atkinson 1981b)

The issues of comparison and generalization touched on in this memorandum will be developed elsewhere. For the time being we simply wish to illustrate the general rationale for attempting to move from the local to the more generic, in so far as it directs attention towards comparison, and draws on the work of other analysts. We shall not attempt to detail all the ideas drawn on and alluded to in this particular project. The following extracts from the same research memorandum are illustrative of how these ideas were used to categorize some key issues in the research, and to stimulate the posing of further topical questions:

Gatekeepers. By gatekeepers I mean actors with control over key sources and avenues of opportunity. Such gatekeepers exercise control at and during key phases of the youngster's status passage(s). Such gatekeepers' functions would actually be carried out by different personnel in the different organization settings. . . .

The identification of the general class of 'gatekeepers' would then allow us to go on to ask some pertinent questions of a general nature. For instance: What resources do gatekeepers have at their disposal? What perceptions and expectations do gatekeepers have of 'clients'? Are these perceptions mutually compatible or are there systematic differences of opinion? Do gatekeepers believe that their expectations of

clients are met or not? Do they have an implicit (or even an explicit) model of the 'ideal client'?

What is the information-state of gatekeepers? For example, what sort of model of the labour market are they operating with? What views of working life do they bring to bear? How accurate are their assessments of the state of local labour markets?

What sort of routines and strategies do gatekeepers employ? For instance, what criteria (formal and informal) are used to assess and categorize 'clients'? What bureaucratic routines are used (if any)? What record-keeping procedures are used, and how are such data interpreted in practice?

(Atkinson 1981b)

Closely allied with this outline of 'gatekeepers' as a general sensitizing device, the memorandum also included the following:

Labelling. This general category clearly overlaps with the gatekeepers' practical reasoning, and with some issues in definitions of client populations. To what extent is there a danger of self-fulfilling prophecies, as a result of the identification of target populations? To what extent do projects themselves help to crystallize racial, gender or ability categorizations and stereotypes?

Do employers and potential employers operate with stigmatizing stereotypes? Do projects overcome, or do they help to confirm, such stereotypes? What particular aspects of projects and the youngsters do 'gatekeepers' such as employers seize on and react to?

Do the youngsters label themselves and each other in accordance with formal or informal labels attached to them? Are the professionals involved in projects themselves subject to stigma in the views of other professionals and agencies?

(Atkinson 1981b)

Obviously, these extracts from a research memorandum do not constitute more than the beginning of an exhaustive analysis of projects aimed at easing the transition to work, or at coping with the problem of youth unemployment. Our reference to it here is an attempt to exemplify one stage in the process whereby

ideas are formulated. While many of the questions that are posed here are fairly concrete or topical in content, the general tenor of the document draws attention to generic concepts such as gatekeepers, labelling, stigma, routines, strategies, practical reasoning, and self-fulfilling prophecies.

This research memorandum, then, helps to 'freeze' the process of problem formulation during an intermediate stage in a research project. The initial fieldwork has suggested a number of potentially important aspects to be identified more thoroughly, and some potentially useful analytic ideas. Thus, research problems are identified more precisely. At the same time, such identifications permit new research questions to be posed, or for them to be posed more systematically. Hence guidelines for further data collection are also laid down.

One must beware of over-simplifying the distinction between topical and generic levels of analysis. One does not simply progress in a uni-directional way from one to the other. In the conduct of an actual project, one would not expect simply to progress from a series of substantive issues, and end up with one's formal categories, or vice versa. There will normally be a constant shuttling back and forth between the two analytic modes. Particular substantive issues may suggest affinities with some formal concept that will, in turn, indicate substantive issues as deserving new or further attention, and so on.

SELECTING SETTINGS AND CASES

There is another factor that often plays a significant role in shaping the way in which research problems are developed in ethnography: the nature of the setting or settings chosen for study. Sometimes the setting itself comes first – an opportunity arises to investigate an interesting setting; and foreshadowed problems spring from the nature of that setting. This is true, for example, in the case of research on 'natural experiments' and other kinds of 'opportunistic research' (Riemer 1977). Here, the selection of a setting for study hardly arises, and the research problem and the setting are closely bound together. The same is true in the case of professional practitioners doing research on the settings in which they work:

The decision of where to locate an ethnographic case study is

normally a matter of careful consideration and assessment with the advantages and disadvantages of various locales being carefully considered. . . . Because of my circumstances, my choice reduced to a straightforward decision between doing my research at the school at which I worked or abandoning my desire to do an ethnographic study.

(Pollard 1985:218)

However, even where a setting is selected on the basis of foreshadowed problems, the nature of the setting may still shape the development of the research questions. This arises because, as we noted earlier, in ethnographic research the development of research problems is rarely completed before fieldwork begins; indeed, the collection of primary data often plays a key role in that process of development.

At the same time, it is often found that some of the questions into which the foreshadowed problems have become decomposed or transformed are not open to investigation in the setting selected. The researcher is then faced with the choice of either dropping these questions from the investigation or re-starting the research in a setting where they *can* be investigated. While on occasion the importance of a problem may lead to the latter course, generally researchers stay where they are and select problems that can be investigated there. After all, as in the case of Hargreaves *et al.* (1975), more questions are usually generated than can be tackled in a single study. Moreover, not only does moving to another setting involve further delay and renewed problems of access, but there is also no guarantee that the new setting will turn out to be an appropriate one in which to investigate the preferred problem. Everett Hughes is reported to have remarked, only half jokingly, that the researcher should select the research problem for which the setting chosen is the ideal site!

All this does not mean that the selection of settings for study is unimportant, simply that the ethnographer is rarely in a position to specify the precise nature of the setting required. It is a matter of identifying the sorts of setting that would be most appropriate for investigation of the research problem, as currently formulated. Moreover, when a type of setting has been decided on, it is advisable (if possible) to 'case' possible research sites with a view to assessing their suitability, the feasibility

of carrying out research there, and how access might best be accomplished should they be selected (Schatzman and Strauss 1973:19). This involves collecting and subjecting to preliminary analysis any documentary evidence available about the setting, interviewing anyone who can be easily contacted who has experience or knowledge of the setting, and perhaps making brief visits to the setting, covertly or overtly.

'Casing the joint' in this fashion not only may provide information about settings in which the research might be carried out, but also feeds into the development and refinement of the research problem. It may be discovered that what had been assumed to be a homogeneous category of people must be broken down into a number of sub-types who have different characteristics and who are likely to be found in very different places. Warren provides an example:

The first decision that must be made by a researcher who wishes to study the gay community – unless he has unlimited time and money to spend – is which gay community he wishes to study: the world of exclusive private gay clubs for businessmen and professionals? or the dope addict transvestites so vividly depicted in *Last Exist to Brooklyn*? or the sado-masochistic leather boys? Any extended preliminary observation will make it objectively obvious that 'the' gay community is divided – fairly loosely at the boundaries – into a hierarchy linked to some extent with status and class criteria in the 'real' world.

(Warren 1972:144)

The role of pragmatic considerations must not be underestimated in the choice of a setting. While by no means absent in hypothesis-testing research, they are likely to play an especially important role in research concerned with theory development. This is because here the criteria specifying suitability are usually much less determinate: there is generally a very wide range of relevant settings. As a result, contacts with personnel promising easy access, the scale of the travel costs likely to be involved, and the availability of documentary information, etc. are often major considerations in narrowing down the selection. (See, for example, Fox's 1964 discussion of her choice of Belgium as the site for a study of European medical research.)

Sometimes, the search for an appropriate setting can take

unpredictable turns, as Campbell's account of his research in Greece in the 1950s illustrates. He set out to study one of the villages in a mountain region north-east of Jannina. However, he found the populations of the villages much depleted as a result of civil war, and that his English background led to suspicions that he was a spy. A fortuitous event transformed his research plans. Sarakatsan transhumant shepherds lived on the hills above the village, and relations between them and the villagers were uneasy:

Our own contacts with them had not gone beyond formal greetings when one day in the heat of summer a young shepherd-boy returning from school had stopped at the village spring to drink, and was there set upon by larger village boys. . . . At this point, the anthropologist's wife entered indignantly to rescue the victim. This small adventure had its consequences. We received an invitation to visit a Sarakatsan encampment and the relationship prospered. When some weeks afterwards the time arrived for the Sarakatsani to take their flocks and families down to the plains of Thesprotia for the winter, one family sent us a peremptory message. We were to accompany them and they would build us a hut.

(Campbell 1992:152)

This example also illustrates how occasionally researchers find that they have effectively been chosen to research a setting by one or more of the people involved in it, though usually with rather more strings attached than in this case. In such circumstances, the ethnographer must balance the ease of initial access offered against the desirability of the site in other respects, as well as against any problems that such direct sponsorship by a gatekeeper might cause.

Usually ethnographers study only one or a small number of settings, and usually ones that are geographically close to where they are based. Often this is forced by the cost of using more remote sites and the limited resources available. This is not always the case, however. An exception is Henslin's study of the homeless. He decided to do a national study, but found that setting off with his family in a motor home to combine research with sight-seeing led to little fieldwork being done. Fortunately, an alternative arose:

I heard about a 'fly-anywhere-we-fly-as-often-as-you-want-for-21-days' sales gimmick from Eastern Airlines. I found their offer was legitimate, that for \$750 I could pack in as many cities as I could stand – actually more than I could stand as it turned out. . . . It was the method itself, participant observation, that became the key for making this research affordable. Obviously, the homeless spend very little money, which dovetailed perfectly with my situation and desires. I was able to stay in the shelters at no financial cost. (The shelters, however, exacted a tremendous cost in terms of upsetting my basic orientational complacencies.) In addition to a free bed and a shower, the shelters usually provided morning and evening meals. Although those meals were not always edible, I was able to count on the noon meal being of quality, and that was already included in the price of my airline ticket. . . . I primarily focused on major cities in the Western part of the United States, later adding cities in other areas during subsequent travels. My purpose was to obtain as good a 'geographical spread' as I could.

(Henslin 1990:55)

Generally speaking, of course, the more settings studied the less time can be spent in each. The researcher must make a trade-off here between breadth and depth of investigation.

It is important not to confuse the choice of settings with the selection of cases for study. The vocabulary of studying 'fields' and 'settings' is widely used in talking and writing about ethnography. The main source of this tendency to regard natural settings as the object of study is of course naturalism, and it can be found, for example, in the work of the Chicago School:

[The sociological study of Chicago] was nursed as a cartographic exercise studying Little Sicily, the Jewish ghetto, Polonism, the Gold Coast, the slums, Hobohemia, rooming-house districts and the gangs of the city. Each of these areas was treated as a symbolic world which created and perpetuated a distinctive moral and social organization. Each was subjected to an interpretative analysis which attempted to reproduce the processes by which that organization was brought into being. They were collectively identified as natural areas: 'natural' because they were themselves part of the natural evolution and selection which shaped society;

because they were different from the structures produced by planning and science; and because they represented a unit which allegedly framed American thinking on social and political life.

(Rock 1979:92)

In other sociological contexts, too, similar appeals are made to models of relatively self-contained groups of 'communities'. In the past, the anthropological tradition, for instance, tended to lay stress on the investigation of small-scale 'face-to-face' societies and local collectivities (such as 'the village'). This, and the cognate tradition of 'community studies', has often rested on a *Gemeinschaft*-like view of the local society, emphasizing its internal stability and its relative discreteness.

However, settings are not naturally occurring phenomena, they are constituted and maintained through cultural definition and social strategies. Their boundaries are not fixed but shift across occasions, to one degree or another, through processes of redefinition and negotiation.

There is another reason too why it is potentially misleading to talk of 'studying a setting'. It is not possible to give an exhaustive account of any object. In producing descriptions we always rely on criteria of selection and inference. There is an important sense, then, in which even in the most descriptively oriented study the case investigated is not isomorphic with the setting in which it takes place. A setting is a named context in which phenomena occur that might be studied from any number of angles; a case is those phenomena seen from one particular angle. Some features of the setting will be given no attention at all, and even those phenomena that are the major focus will be looked at in a way that by no means exhausts their characteristics. Moreover, a setting may contain several cases. Thus, for example, in studying the effects of various kinds of external examinations on secondary school teaching, it will be particular examination courses within the school that constitute the cases under investigation rather than the school as a whole (Scarth and Hammersley 1988). Conversely, a case may not be contained within the boundaries of a setting; it may be necessary to go outside of the setting to collect information on important aspects of it. In studying gangs among male prisoners (Jacobs 1974), it may be necessary to explore their links with groups outside if

the manner in which they came to be formed and in which they continue to recruit new members is to be understood. While it may seem innocent enough, then, the naturalistic conception of studying fields and settings discourages the systematic and explicit selection of aspects of a setting for study, as well as movement outside of it to follow up promising theoretical leads. And, of course, the process of identifying and defining the case under study must proceed side by side with the refinement of the research problem and the development of the analysis.

One of the limitations often raised in connection with ethnographic work is that because only a single case, or at any rate a small number of cases, is studied, the representativeness of the findings is always in doubt. This can be an important point, but it is not always so. Sometimes, ethnographic research is concerned with a case that has intrinsic interest, so that generalization is not the primary concern. This is most obviously true with action research and evaluation studies, where the target is the characteristics of particular situations. And, occasionally, ethnographic work involves the study of a relatively large number of cases, thereby often providing a substantial basis for generalization. For instance, Strong (1979) studied 1000 cases of paediatric consultation in three hospitals, two in Britain and one in the United States. However, even where generalization is a goal of ethnographic research but only a small number of cases is studied, various strategies can be used to deal with the problem, more or less adequately. How it should be dealt with depends on whether the research is directed towards the development and testing of a theory or whether the aim is generalization about a finite population of cases, whether actually existing or possible in the future (Schofield 1990).

Where the concern is theory development and testing, the strategic selection of cases is particularly important. This can take a variety of forms. One is what Glaser and Strauss (1967) call 'theoretical sampling'. The primary concern of these authors is the generation and elaboration of theory, and they argue that the selection of cases should be designed to produce as many categories and properties of categories as possible and to relate categories to one another. They recommend two complementary strategies: minimizing the differences between cases to highlight basic properties of a particular category; and then subsequently maximizing the differences between cases in order

to increase the density of the properties relating to core categories, to integrate categories and to delimit the scope of the theory. As an illustration they cite their research on the awareness contexts surrounding patients dying in hospital:

Visits to the various medical services were scheduled as follows: I wished first to look at services that minimized patient awareness (and so first looked at a premature baby service and then a neurosurgical service where patients were frequently comatose). I wished next to look at dying in a situation where expectancy of staff and often of patients was great and dying was quick, so I observed on an Intensive Care Unit. Then I wished to observe on a service where staff expectations of terminality were great but where the patient's might or might not be, and where dying tended to be slow. So I looked next at a cancer service. I wished then to look at conditions where death was unexpected and rapid, and so looked at an emergency service. While we were looking at some different types of services, we also observed the above types of services at other types of hospitals. So our scheduling of types of service was directed by a general conceptual scheme – which included hypotheses about awareness, expectedness and rate of dying – as well as by a developing conceptual structure including matters not at first envisioned. Sometimes we returned to services after the initial two or three or four weeks of continuous observation, in order to check upon items which needed checking or had been missed in the initial period.

(Glaser and Strauss 1967:59)

Strategic selection of cases can also be employed in *testing* theoretical ideas. Here the aim is to select cases for investigation which subject theories to relatively severe test. An example is the sequence of studies by Hargreaves, Lacey, and Ball (Hargreaves 1967; Lacey 1970; Ball 1981; see also Abraham 1989a). They argue that the way in which schools differentiate students on academic and behavioural grounds, especially via streaming, tracking, and banding, polarizes them into pro- and anti-school subcultures. These subcultures, in turn, shape students' behaviour inside and outside school and affect their levels of academic achievement. This theory is tested in examples of three types of secondary school: secondary modern

(Hargreaves), grammar (Lacey), and comprehensive (Ball). Moreover, in the case of the grammar school, because the students entering the school have been strongly committed to school values at their junior schools, variables at the heart of competing explanations for the process of polarization – such as attitude to school, aspects of home background, etc. – are partially controlled. Similarly, in his study of Beachside Comprehensive, Ball examines the effects of a shift from banding to mixed ability grouping within a single case (some factors thereby remaining constant), this representing a weakening of differentiation. (For further discussion, see Hammersley 1985.)

Where the aim is generalization to some finite set of cases, rather than the development and testing of theory, it may be possible to assess the typicality of the case or cases studied by comparing their relevant characteristics with information about the target population, if this is available in official statistics or in other studies. Thus, in his investigation of religious intermarriage in Northern Ireland, Lee sought to check the representativeness of his snowball sample of couples by comparing some of their characteristics with a special tabulation of the census data. This revealed that his sample 'showed a sharp bias towards young, recently married couples, mostly without children and with relatively high levels of educational attainment' (Lee 1992:133). While he was not able to correct this sampling bias, because of the problem of gaining access to couples whose position was delicate in the Northern Ireland situation, he was able to allow for it in his analysis.

It may even sometimes be possible to carry out a small-scale survey on a larger sample of the population to gather information to assess the typicality of the cases being studied. Thus, in his study of students at Rutgers University, Moffatt used a survey to assess the extent to which they had a vocational orientation, and he was able to compare the results with those of a national study (Moffatt 1989:331). Another possibility is to combine in-depth study of a small number of cases with more superficial checks on other cases. For example, in his study of law enforcement agencies, Skolnick concentrated on those in one city, but he made a brief investigation of agencies in another to check the likely generalizability of his findings (Skolnick 1966).

The appropriate strategy to adopt in selecting cases may vary

over the course of the research. In the early phases, which cases are chosen for investigation may not matter greatly. Later on, it may come to acquire considerable importance. Certainly, initial decisions may have to be revised. Klatch reports how in her research on women involved in right-wing political organizations she began with 'a neat fourfold table comparing four organizations: two Old Right groups and two New Right groups; two "religious" and two "secular organizations"'. However, she soon faced some problems. In particular, she discovered that:

the chosen organizations for my original design did *not* in fact divide along secular versus religious lines. . . . Furthermore, I noticed a general pattern developing between the 'home-maker' type of woman active in many religious/pro-family groups . . . and the 'professional' type of women active in the more secular conservative groups. . . . The final design continued to rely on in-depth interviews, participant observation, and a textual analysis of right-wing literature, but I broadened the sample to include a much wider range of conservative groups in order to increase the variation among the female activists, thereby gaining a better understanding of the broader divisions within the Right.

(Klatch 1988:75)

Research design in ethnography, both as it relates to the selection of cases for study and in other respects too, is a continuous process. The match between research problems and cases selected must be continually monitored.

SAMPLING WITHIN THE CASE

Selecting cases for investigation is not the only form of sampling involved in social research. Equally important, often, is sampling *within* cases. At least this is true where cases are not so small that they can be subjected to exhaustive investigation, as for example in Strong's study of paediatric consultations. Decisions must be made about where to observe and when, who to talk to and what to ask, as well as about what to record and how. In this process we are not only deciding what is and is not relevant to the case under study but also usually sampling from the data available in the case. Very often this sampling is

not the result of conscious deliberation, but it is important to make the criteria employed as explicit and as systematic as possible, so as to try to ensure that data about the case have been adequately sampled. There are three major dimensions along which sampling within cases occurs: time, people, and context.

Time

Time may seem a dimension of obvious importance in social life, but it has often been neglected. Attitudes and activities often vary over time in ways that are highly significant for social theory. Berlak *et al.* provide an example from their research on 'progressive' primary schools in England:

During our first weeks in the English schools we gradually began to understand that the images of the schools conveyed in the literature were to some extent distorted. The way in which this understanding developed is exemplified by our experience during the first weeks of our study of Mr Thomas's classroom. In his classroom, in a school in an affluent suburban area, we observed thirty children on a Wednesday morning who, after a brief discussion with the teacher, went about their work individually: some began to work on 'maths', others to study spelling or to write original stories in much the way [that the literature describes]. We observed no teacher behavior on that morning which appeared to direct the children to what they were to do. It appeared that the children were pursuing their own interests. However, during the following days, we observed events and patterns which appeared to account for the behavior observed on that Wednesday morning. On the following Monday morning we observed Mr Thomas set work minimums in each subject for the week. . . . On the following Friday morning we saw him collect the children's work 'diaries' where each child had recorded in detail the work he had completed during the week. Over the weekend, Mr Thomas and, as we were to later discover, sometimes the head, checked each record book and wrote comments in the diaries such as 'good', 'more maths', or the ominous 'see me'. Such items, which explained

some of the apparently spontaneous classroom behavior, had not appeared in the literature.

(Berlak *et al.* 1975:218)

The general issue of the social construction and distribution of time is quite beautifully demonstrated in Zerubavel's (1979) study of time in hospitals. In Zerubavel's work, the organization of time is not an incidental feature or a background to a substantive focus on other organizational matters. Rather, it is an exercise, in the tradition of Simmel, on the formal category of time itself:

Following the methodological guidelines which I derived from Simmel's formal sociology, I focused my observations on only one aspect of hospital life, namely, its temporal structure, deliberately ignoring – for analytical purposes – the history of the hospital, its national reputation, the quality of its patient care, its architectural design and spatial organization, its finances, the religious and ethnic makeup of its staff, and so on.

(Zerubavel 1979:xvii)

Zerubavel's is thus an unusually sparse ethnography. Yet the single-mindedness of his observations and his formal analyses enable him to reveal the complex patterning of temporal orders within the organization of daily life in the hospital. He foreshadows their diversity in the introduction:

The list of sociological aspects of temporality which can be discussed within the context of hospital life is almost endless: the temporal structure of patients' hospital careers; the relations between time and space; deadlines and strategies of beating the schedule; the temporal relations among the various hospital units; the impact of organizational time on hospital personnel's life outside the hospital; and so on.

(Zerubavel 1979:xxi)

To follow Zerubavel's example, think hypothetically about the casualty department of an urban general hospital. Any systematic study here would almost certainly reveal different patterns of work and activity according to the time of day or night, and according to the day of the week. The nature of the referrals and emergency presentations would vary too. Saturday nights

would probably be characterized by very different rates and patterns of admission from Sunday nights, and so on. Time in our casualty department would also relate to changing shifts of nursing staff, rotations among junior doctors, and so forth. Very similar considerations would apply in many other settings: in factories, prisons, educational settings, and residential homes, for example.

It should be apparent, therefore, that any attempt to represent the entire range of persons and events in the case under study will have to be based on adequate coverage of the various temporal divisions. On the other hand, it is impossible to conduct fieldwork round the clock, and some degree of time sampling must be attempted. It may be possible to undertake the occasional period of extended fieldwork, but these are hard to sustain. (These remarks do not apply in quite the same way to anthropological fieldwork, or to practitioner research, where the ethnographer is in principle 'in play' all day, every day – though, even here, the fieldworker will need to 'escape' periodically in order to write up notes, file material, and simply relax.) In any event, long uninterrupted periods of fieldwork are not always to be encouraged. The production of decent fieldnotes, transcribing audio- or video-recordings, the indexing and filing of material, writing memoranda and reflexive notes are all time-consuming and demanding activities. Very long periods of observation will thus become quite unmanageable. The longer the time between observation and recording, the more troublesome will be the recall and recording of adequately detailed and concrete descriptions. Long bursts of observation, uninterrupted by periods of reflexive recording, will thus tend to result in data of poor quality.

Hence, all ethnographers have to resist the very ready temptation to try to see, hear, and participate in everything that goes on. A more selective approach will normally result in data of better quality, provided the periods of observation are complemented by periods of productive recording and reflection. Rather than attempting to cover the entire working day, for instance, one may be able to build up an adequate representation by following the sort of strategy outlined by Schatzman and Strauss:

If the researcher elects to observe work around the clock, he

can first observe a day shift for several days, then evenings and then nights, for a period of consecutive days until he is reasonably familiar with all three shifts. Or he may cover events at any given sub-site by 'overlapping' time on consecutive dates – for example, 7:00 a.m. to 9:00 a.m., 8:00 a.m. to 10:00 a.m., 9:00 a.m. to 11:00 a.m. – and over a period of days cover the organization around the clock.

(Schatzman and Strauss 1973:39)

Over and above these procedures for establishing adequate coverage, the researcher will probably identify particularly salient periods and junctures: the change-over between shifts, for instance, might prove crucial in the organization of work, the sharing of information, and so on. Such crucial times should then come in for particular attention.

Similar considerations to those outlined above will also apply to larger-scale temporal dimensions, such as seasonal or annual cycles, and patterns of recruitment of new cohorts, although overall constraints of time and resource will obviously prove limiting here.

Hitherto we have referred primarily to issues relating to fieldwork in organizations and the like. It should also be apparent that similar considerations might apply to fieldwork in less formally defined settings. The patterns of urban life, 'relations in public', the use of public settings, and patterns of deviant activity all follow temporal dimensions: the seasons, the days of the week, and the time of day or night all play their part. Likewise, it may be important to pay some attention to special occasions, such as seasonal festivals and carnivals, ceremonies and rituals, rites of passage, and social markers of status passage.

In organizing the sampling of time, it is as important to sample the routine as it is to observe the extraordinary. The purpose of such systematic data collection procedures is to ensure as full and representative a range of coverage as possible, not just to identify and single out the superficially 'interesting' events.

People

No setting will prove socially homogeneous, and the adequate representation of the people involved in a particular case will normally require some sampling (unless the whole population of relevant actors can be studied adequately and in equal depth). The sampling of persons may be undertaken in terms of fairly standard 'face-sheet' demographic criteria. That is, depending on the particular context, one may sample persons by reference to categories of gender, 'race', ethnicity, age, occupation, educational qualifications, and so on. However, these face-sheet categories are of importance only as they are relevant to the emerging analysis or to rival theories, or to ensuring representation in terms of some larger population, and they will usually be complemented by other categories of analytic relevance. Such emergent categories may be either 'member-identified categories' or 'observer-identified categories'. The distinction is drawn from Lofland (1976): 'member-identified categories' refers to typifications that are employed by members themselves, that is, they are 'folk' categories that are normally encapsulated in the 'situated vocabularies' of a given culture; 'observer-identified categories' are types constructed by an observer.

Some cultures are particularly rich in member-generated categories. Spradley (1970), for instance, in his work on tramps, identifies the following taxonomy of terms that are used to identify major types: ding, bore car tramp, bindle stiff, working stiff, airedale, home guard tramp, mission stiff, and rubber. The taxonomy also includes the sub-types: harvest tramp, tramp miner, fruit tramp, construction tramp, sea tramp, nose diver, and professional nose diver. Similarly, in her study of a women's prison, Giallombardo (1966) documents the following collection of labels that the prisoners themselves use to categorize the inmates: snitchers, inmate cops, and lieutenants; squares, jive bitches; rap buddies, homeys; connects, boosters; pinners; penitentiary turnouts, lesbians, femmes, stud broads, tricks, commissary hustlers, chippies, kick partners, cherries, punks, and turnabouts. These labels are applied on the basis of 'the mode of response exhibited by the inmate to the prison situation and the quality of the inmates' interaction with other inmates and staff', including styles of sexual orientation (Giallombardo 1966:270).

On the other hand, the observer may construct hypothetical categories, on the basis of the fieldwork. In a study of waiting behaviour, for instance, Lofland identified the following key types:

- 1 *The Sweet Young Thing*. (Generally a female.) Once having taken a position, normally a seated one, she rarely leaves it. Her posture is straight; potentially suggestive or revealing 'slouching' is not dared.
- 2 *The Nester*. Having once established a position, such persons busy themselves with arranging and rearranging their props, much in the manner of a bird building a nest.
- 3 *The Investigator*. Having first reached a position, the investigator surveys his surroundings with some care. Then ... he leaves his position to begin a minute investigation of every inanimate object in sight.
- 4 *The Seasoned Urbanite* ... is easy and relaxed ... within the confines of legitimate setting use and proper public behavior.
- 5 *The Maverick* ... is a non-style. ... Its users are those who either do not know, are not able, or do not care to protect themselves in public settings. ... There are three types ... : *children* ... ; the *constantly stigmatised* ... ; and *eccentrics*.

(Lofland 1966; cited in Lofland 1971:35)

Whether the sampling of persons takes place on the basis of member-identified or observer-identified categories (and often both are used), the process is inextricably linked with the development of analytical ideas and the collection of data.

Context

Taking account of variations in context is as important as sampling across time and people. Within any setting people may distinguish between a number of quite different contexts that require different kinds of behaviour. Some of these will be fairly obvious, others less so. In schools, for example, it is well known that the behaviour of teachers often differs sharply between classrooms and staffrooms (Woods 1979; Hammersley 1980). This contrast is an example of a more abstract distinction between frontstage and backstage regions developed by Goffman:

A back region or backstage may be defined as a place, relative to a given performance, where the impression fostered by the performance is knowingly contradicted as a matter of course. There are, of course, many characteristic functions of such places. It is here that the capacity of a performance to express something beyond itself may be painstakingly fabricated; it is here that illusions and impressions are openly constructed. Here stage props and items of personal front can be stored in a kind of compact collapsing of whole repertoires of actions and characters. Here grades of ceremonial equipment, such as different types of liquor or clothes, can be hidden so that the audience will not be able to see the treatment accorded them in comparison with the treatment that could have been accorded them. Here devices such as the telephone are sequestered so that they can be used 'privately'. Here costumes and other parts of personal front may be adjusted and scrutinized for flaws. Here the team can run through its performance, checking for offending expressions when no audience is present to be affronted by them; here poor members of the team, who are expressively inept, can be schooled or dropped from the performance. Here the performer can relax; he can drop his front, forgo speaking his lines, and step out of character.

(Goffman 1959:114-15)

Goffman illustrates his argument by reference to a wide range of settings from hotel restaurants to shipyards.

It is important, however, not to mistake places for contexts. We must remember, again following Goffman (1963), that architectural structures are merely props used in the social drama; they do not determine behaviour in a direct fashion. What we think of, for example, as 'staffroom behaviour' may also occur in other parts of a school where conditions are right, or even in the bar of a local public house. Conversely, behaviour typical of the staffroom may not occur while visitors, or even the head-teacher, are there. If we are to ensure that we are not led into false generalizations about attitudes and behaviour within a case through contextual variability, we must identify the contexts in terms of which people in the setting act, recognizing that these are social constructions not physical locations, and try to ensure that we sample across all those that are relevant.

Up to this point we have talked for the most part as though it were simply up to the researcher to select the settings and cases for study, and to sample them appropriately. But, of course, the cases we might wish to select may not be open to study, for one reason or another; and, even if they are, effective strategies for gaining access to the necessary data will need to be developed. Similarly, not all the people we wish to observe or talk to, nor all the contexts we wish to sample, may be accessible – certainly not at the times we want them to be. The problem of gaining access to data is particularly serious in ethnography since one is operating in settings where the researcher generally has little power, and people have pressing concerns of their own which often give them little reason to co-operate. It is to this problem that we turn in the next chapter.