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Organising Observational Research

Bearing in mind both Warren's and Whyte's work, it is now appropriate to think a little more systematically about how to organise an observational study. Simplifying, I will suggest five stages:

- beginning research
- writing fieldnotes
- looking as well as listening
- testing hypotheses
- making broader links

These steps are not arranged in any particular order. For instance, one should be making broader links at quite an early stage. Of course, making such links implies the relevance of theoretical perspectives on observational research – to be discussed in the subsequent section.

Beginning Research

In Chapter 1, I argued that premature definition of 'variables' was dangerous in field research. Early 'operational' definitions offer precision at the cost of deflecting attention away from the social processes through which the participants themselves assemble stable features of their social world. So, for instance, the qualitative social scientist may be reluctant to begin by defining, say, 'depression' or 'efficiency'. Instead, it may be preferable to examine how, in different contexts, 'depression' and 'efficiency' come to be defined.

The assumption that one should avoid the early specification of definitions and hypotheses has been common to field researchers since the 1930s. As Becker and Geer argued many years ago, for the field researcher:

a major part of . . . research must consist of finding out what problems he [sic] can best study in this organisation, what hypotheses will be fruitful and worth pursuing, what observations will best serve him as an indicator of the presence of such phenomena as, for example, cohesiveness or deviance. (Becker and Geer: 1960, 267)

However, this does not mean that the early stages of field research are totally unguided. The attempt to describe things 'as they are' is doomed to failure. Without *some* perspective or, at the very least, a set of animating questions, there is nothing to report. Contrary to crude empiricists, the facts *never* speak for themselves.

One way to assemble data is to begin with a set of very general

What is going on here? What do people in this setting have to know (individually and collectively) in order to do what they are doing? How are skills and attitudes transmitted and acquired, particularly in the absence of intentional efforts at instruction? (Wolcott: 1990, 32)

Already here, we can see that Wolcott's questions are guided by a particular theoretical focus on people's knowledge and skills. This emerges out of a set of assumptions common to many field researchers. These assumptions may be crudely set out as follows:

- 1 *Common sense* is held to be complex and sophisticated rather than naive and misguided.
- 2 *Social practices* rather than perceptions are the site where common sense operates: the focus is on what people are doing rather than upon what they are thinking, e.g. talking to one another, having meetings, writing documents, etc.
- 3 '*Phenomena*' are viewed within such inverted commas. This means that we seek to understand how any 'phenomenon' is locally produced through the activities of particular people in particular settings.

Of course, any such list glosses over the range of theoretical directions to be found in field research. Later we will look at two key theories deriving from the work of Erving Goffman and Harvey Sacks. For the moment, however, let us assume that we have established a particular focus. How then do we proceed?

Writing Fieldnotes

Let us assume that you are not using electronic recordings (audio- or videotapes) or that you wish to supplement such recordings with observational data. How should you write fieldnotes? (Working with transcripts deriving from recordings is discussed in Chapter 6.)

The greatest danger is that you will seek to report 'everything' in your notes. Not only does this overlook the theory-driven nature of field research, it gives you an impossible burden when you try to develop a more systematic analysis at a later stage: 'The critical task in qualitative research is not to accumulate all the data you can, but to "can" (get rid of) most of the data you accumulate. This requires constant winnowing' (Wolcott: 1990, 35).

At the outset, however, it is likely that you will use broad descriptive categories 'relating to particular people or types of people, places, activities and topics of concern' (Hammersley and Atkinson: 1983, 167). Moreover, items may be usefully assigned to more than one category in order to maximise the range of hypotheses that can be generated. To do this, it may help to make multiple copies of each segment of data, filed under several categories (*ibid*, 170).

One useful aid in filing and indexing is provided by computer software programs. ETHNOGRAPH allows you to code a text into as many as

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more flexible units and codes. NUDIST will store information in tree-structured index systems with an unlimited number of categories and highly complex index structures. You can then search your data by these indexes or look for overlap between data indexed under different categories. The NUDIST program thus helps in the generation of new categories and the identification of relationships between existing categories (see Richards and Richards: 1987, Tesch: 1991).

In order to make this discussion of note-taking more concrete, I want to give an example from a piece of research I carried out in the early 1980s (see Silverman: 1987, Chs. 1–6). The study was of a paediatric cardiology unit. Much of my data derived from tape-recordings of an outpatient clinic that lasted between two and four hours every Wednesday.

Secure in the knowledge that the basic data were being recorded, I was free to use my eyes as well as my ears to record more data to help in the analysis of the audio-tapes. Gradually, with the help of my co-worker Robert Hilliard, I developed a coding sheet to record my observations.

As an illustration of how I coded the data, I append in Table 3.1 below the full coding sheet used in this study. In order to show how we derived the categories, I have included explanations of some of the categories in square brackets.

I ought to stress that this coding form was only developed after observation of more than ten outpatient clinics and after extensive discussions between the research team. During this time, we narrowed down what we were looking for. Increasingly, we became interested in how decisions (or 'disposals') were organised and announced. It seemed likely that the doctor's way of announcing decisions was systematically related not only to clinical factors (like the child's heart condition) but to social factors (such as what parents would be told at various stages of treatment). For instance, at a first outpatients' consultation, doctors would not normally announce to parents the discovery of a major heart abnormality and the necessity for life-threatening surgery. Instead, they would suggest the need for more tests and only hint that major surgery might be needed. They would also collaborate with parents who produced examples of their child's apparent 'wellness'.

This step-by-step method of information-giving was avoided in only two cases. If a child was diagnosed as 'healthy' by the cardiologist, the doctor would give all the information in one go and would engage in what we called a 'search and destroy' operation, based on eliciting any remaining worries of the parent(s) and proving that they were mistaken. In the case of a group of children with the additional handicap of Down's Syndrome, as well as suspected cardiac disease, the doctor would present all the clinical information at one sitting, avoiding a step-by-step method. Moreover, atypically, the doctor would allow parents to make the choice about further treatment, while encouraging parents to focus on non-clinical matters like their child's 'enjoyment of life' or friendly personality (see Chapter 8, pp. 186–188 for more detail).

The coding form in Table 3.1 allowed us to identify these patterns. For instance, by relating item 14 on the scope of the consultation to the decision-format (item 20), we were able to see differences between consultations involving Down's children and others. Moreover, it also turned out that there were significant differences between these two groups in both the form of the elicitation question (item 16) and the diagnosis statement (item 19).

The coding form in Table 3.1 followed a practice described elsewhere which derives from:

that well-established style of work whereby the data are inspected for categories and instances. It is an approach that disaggregates the text (notes or transcripts) into a series of fragments, which are then regrouped under a series of thematic headings. (Atkinson: 1992, 455)

As Atkinson points out, one of the disadvantages of coding schemes is that, because they are based upon a given set of categories, they furnish 'a powerful conceptual grid' (459) from which it is difficult to escape. While this 'grid' is very helpful in organising the data analysis, it also deflects attention away from uncategory activities.

In these circumstances, it is helpful to return occasionally to the original data. In our research, we had our tapes and transcripts which offered endless opportunities to redefine our categories. Lacking tapes of his data on medical education, Atkinson returned to his original fieldnotes. He shows how the same, original data can be reread in a quite different way.

Atkinson's earlier method had been to fragment his fieldnotes into relatively small segments, each with its own category. For instance, a surgeon's description of post-operative complications to a surgical team was originally categorised under such headings as 'unpredictability', 'uncertainty', 'patient career' and 'trajectory'. When Atkinson returns to it, it becomes an overall narrative which sets up an enigma ('unexpected complications') which is resolved in the form of a 'moral tale' ('beware, unexpected things can always happen'). Viewed in this way, the surgeon's story becomes a text with many resemblances to a fairytale, as we shall see in Chapter 4, pp. 73–75.

There is a further 'moral tale' implicit in using Atkinson's story. The field researcher is always torn between the need to narrow down analysis through category construction and to allow some possibility of reinterpretation of the same data. So, while the rush to categorise is laudable, it should always occur in the context of a solid body of original data. The ideal form for this is a tape-recording or original document. Where these cannot be used, the field researcher must attempt to transcribe as much as possible of what is said and done – and the settings in which it is said and done.

In such transcription, Dingwall (personal correspondence) notes how important it is to record *descriptions* rather than mere impressions. In

Table 3.1: *Outpatient Analysis*

1	Name of patient
2	Age
3	Clinic and date
4	Doctor
5	Family present
6	Non-family present
7	Length of co-presence of doctor and family [we wanted to record the time of the encounter not including periods when the doctor was out of the room]
8	Diagnosis
9	Stage of treatment: 1st consultation Pre-inpatient Post-catheter [test requiring inpatient stay] Post-operation
10	Outcome of consultation: Discharge or referral elsewhere Non-inpatient follow-up Possible eventual catheter or surgery Catheter Surgery No decision
11	Consultation stages [this derived from Robert Hilliard's attempt to identify a series of stages from a greeting exchange to elicitation of symptoms, through to examination and diagnosis statement (see Silverman: 1985, especially pp. 265-269)]: Stage Questions asked Topics covered Notes/Markers
12	Does doctor invite questions? No Yes (When:)
13	Use of medical terminology: Stage Doctor/Family
14	Scope of consultation:
	<i>Family</i> <i>Doctor</i>
	Prior treatment history
	Extra-cardiac physical states
	Child development
	Child behaviour
	Family's practicalities of treatment or attendance

Table 3.1: *Continued*

	<i>Family</i>	<i>Doctor</i>
	Doctor's practicalities of treatment or attendance	
	Anxieties and emotional problems of family	
	Social situation of family	
	External treatment agencies	
15	Family's presentation of a referral history	
16	Format of doctor's initial elicitation question [e.g. how is she? is she well?]	
17	Patency [this referred to whether symptoms or diseases were visible or 'patent' to the family]: Family's presentation of problems/symptoms Dr's mention of patent symptoms Family's assent to problems/symptoms 'Not patent'?	
18	Location of examination: desk couch side-room	
19	Diagnosis statement: (a) Use of 'well' (Dr/Family/Both) (b) Use of 'normal' (Dr/Family/Both) (c) Possible diagnoses mentioned (0/1/ > 1)	
20	Decisions: (a) Possible disposals mentioned (0/1/> 1) (b) Medical preference stated (Yes/No) (c) Medical intention stated (Yes/No) (d) Family assent requested (Yes/No) (e) Family allowed to make decision (Yes/No) (f) Family wishes volunteered (Yes/No) (g) Family dissent from doctor's proposed disposal (Yes/No)	
21	Uncertainty expressed by Dr: (a) over diagnosis (b) over treatment	

what people have said or done, using verbatim quotations and 'flat' (or unadorned) descriptions.

Looking as Well as Listening

The attentive reader will have recognised that the coding frame used in Table 3.1 depended, in part, upon what we could see as well as hear (for

the examination). As we have seen, W.F. Whyte (1949) also reaped rich rewards by paying attention to the spatial organisation of activities.

Using his observation of hospital wards, Anssi Peräkylä (personal correspondence) notes how spatial arrangements differentiate groups of people. There are the wards and patient rooms, which staff may enter anytime they need to. Then there are patient lounges and the like, which are a kind of public space. Both areas are quite different from areas like the nurses' room and doctors' offices where patients enter only by invitation. Finally, if there is a staff coffee room, you never see a patient there.

As Peräkylä points out, one way to produce different categories of human beings in a hospital is the allocation of space according to categories. At the same time, this allocation is reproduced in the activities of the participants. For instance, the perceptive observer might note the demeanour of patients as they approach the nurses' room. Even if the door is open, they may stand outside and just put their heads round the door. In doing so, they mark out that they are encroaching on foreign territory.

Unfortunately, we have all become a little reluctant to use our eyes as well as our ears when doing observational work (possible reasons for this are discussed in Chapter 4, p. 70). Notable exceptions are Humphrey's (1970) *Tea Room Trade* (a study of the spatial organisation of gay pick-up sites) and Lindsay Prior's (1988) work on hospital architecture. Michel Foucault's (1977) *Discipline and Punish* offers a famous example of the analysis of prison architecture, while Edward Hall's (1969) *The Hidden Dimension* coined the term 'proxemics' to refer to people's use of space – for instance, how we organise an appropriate distance between each other.

However, these are exceptions. Stimson (1986) has noted how 'photographs and diagrams are virtually absent from sociological journals, and rare in sociological books' (641). He then discusses a room set out for hearings of a disciplinary organisation responsible for British doctors. The Professional Conduct Committee of the General Medical Council sits in a high-ceilinged, oak-panelled room reached by an imposing staircase. There are stained-glass windows, picturing sixteen crests and a woman in a classical Greek pose. As Stimson comments:

This is a room in which serious matters are discussed: the room has a presence that is forced on our consciousness. . . . speech is formal, carefully spoken and a matter for the public record. Visitors in the gallery speak only, if at all, in hushed whispers, for their speech is not part of the proceedings. (Stimson: 1986, 643–644)

In such a room, as Stimson suggests, even without anything needed to be said, we know that what goes on must be taken seriously. Stimson aptly contrasts this room with a McDonald's hamburger restaurant:

Consider the decorations and materials – plastic, paper, vinyl and polystyrene, and the bright primary colours. (Everything) signifies transience. This tempor-

institutionally casualised dress of staff and the seating that is constructed to make lengthy stays uncomfortable. (*ibid*, 649–650)

Exercise 3.2

This is a research exercise to improve your observational skills. These are your instructions:

- 1 Select a setting in which you regularly participate – good examples would be a student restaurant, a bus or train or a supermarket check-out queue.
- 2 Make a sketch map of the site. What sort of activities does the physical lay-out encourage, does it discourage or is it neutral towards? (Think of Stimson's comparison of the room for medical hearings and MacDonald's.)
- 3 How do people use the space you are studying? What do they show they are attending to? How do they communicate with one another or avoid communication? Do they look at one another or avoid it? What distance do they keep between one another?
- 4 In what ways are people using the space to co-operate with one another to *define* themselves (e.g. as a restaurant crowd but not bus passengers)?
- 5 Is there any difference between how people organise their activities when they are on their own, in pairs or in a crowd?
- 6 How do people use the setting as a resource for engaging in activities not specifically intended (but not necessarily inappropriate) in that setting (e.g. displaying particular personal characteristics such as wanting to communicate or not wanting to communicate)?

In a setting like McDonald's, we know that casual enjoyment and informality are appropriate. In addition to all its other differences from the oak-panelled room, this restaurant is not an area for confidences, cut off from the public gaze, but offers an open vista from street to kitchen. Imagine attempting to conduct a disciplinary hearing in such a setting!

Testing Hypotheses

One of the strengths of observational research is its ability to shift focus as interesting new data become available. For instance, during a study of two cancer clinics at a British National Health Service hospital, I unexpectedly gained access to a 'private' (fee-paying) clinic run by one of the doctors in his spare time. I was thus able to change my research focus towards a comparison of the 'ceremonial orders' of public and private medicine (Silverman: 1984).

However, a strength can also be a weakness. Some qualitative research can resemble a disorganised stumble through a mass of data. full of

survey of qualitative papers in two journals in the area of health and social science, I was struck by the number of articles based on one or two 'convincing' examples (Silverman: 1989a).

There is absolutely no reason why observational research cannot combine insight with rigour. In other words, it is right to expect that such research should be *both* original *and* valid. This will involve testing hypotheses that we have generated in the field. Increasingly, however, as our knowledge of micro-social processes expands, it will mean that we can enter the field with a hypothesis we already want to test. So, in my comparative study of medical practice, Strong's (1979a) work on the 'ceremonial orders' of doctor-patient interaction gave me a clear hypothesis which became testable when I gained access to a private clinic.

But how then do we test hypotheses using qualitative data? Many years ago, Becker and Geer (1960) gave us some useful guidelines. In a study of the changing perspectives of medical students during their training, they found three ways of testing their emerging hypotheses:

- 1 Comparison of different groups at one time and of one cohort of students with another over the course of training. For instance, it could only be claimed with confidence that beginning medical students tended to be idealists if several cohorts of first year students all shared this perspective.
- 2 Ensuring that the responses given in interviews were also replicated by what students said and did in more 'naturally-occurring' situations (e.g. speaking to one another in classrooms and over lunch).
- 3 A careful inspection of negative or deviant cases leading to the abandonment, revision or even reinforcement of the hypothesis. For instance:

if it can be shown that the person who acts on a different perspective is socially isolated from the group or that his deviant activities are regarded by others as improper, unnecessary, or foolish, then one can argue that these facts indicate use of the perspective by all but deviants, hence, its collective character. (Becker and Geer: 1960, 289)

- 4 The use of simple tabulations where appropriate. For instance, counting statements and activities by whether they were generated by the observer or were more naturally occurring.

More than thirty years later, Dingwall (1992) underlines this search for validity via the comparative method and the use of deviant cases. He adds two further ways of establishing validity:

- 5 The provision of sufficient 'raw' data (e.g. in long transcripts) to allow the reader to separate data and analysis. As Dingwall comments:

Clearly, it is no more possible to reproduce all the data than it is for a filmmaker to show every inch of film . . . What I am taking exception to, though, is the kind of report that is purely a redescription of the researcher's impressions or sensations. Empathy has its uses, but it is not a substitute for

should enter after recording rather than being confused with it. (Dingwall: 1992, 169)

- 6 Avoiding the temptation, at its height in the 1960s, to favour the 'underdog' at the expense of everybody else. One should have doubts about a study which fails to deal even-handedly with the people it describes or to recognise the interactive character of social life. Dingwall's ethic of 'fair dealing' implies that we should ask of any study: 'Does it convey as much understanding of its villains as its heroes? Are the privileged treated as having something serious to say or simply dismissed as evil, corrupt or greedy without further enquiry?' (*ibid*, 172). Clearly, this is as much a scientific as an ethical issue.

Provided it attends to these sorts of issues, observational research can produce findings every bit as 'hard' as those derived from other methods. Indeed, sometimes it can deliver valid information on topics which are intractable when we are limited by purely quantitative methods. For instance, Bloor *et al* (1991) show that it is possible to establish a reliable estimate of the proportion of drug-injecting female street prostitutes using observational methods on a cohort of women and identifying new fieldwork contacts.

How we test hypotheses in qualitative research is a crucial matter which I have only touched upon here. It is treated in much greater depth in Chapter 7.

Exercise 3.3

This exercise is meant to encourage you to think about how you would test hypotheses derived from observational research. You need to go through the following steps:

- 1 Review your answers to Exercise 3.2 and consider how you might go about testing each of your conclusions, e.g.
 - comparison of different settings or of different groups or activities within the same setting
 - the use of simple tabulations
 - the use of negative or deviant cases.
- 2 Turn your answers to 3.2 into hypotheses (i.e. give them the form: if A then B). Return to the field and try to gather the kind of data which might test your hypotheses.
- 3 Distinguish those hypotheses which have been confirmed from those which have been disconfirmed and those which you remain unsure about.
- 4 What kind of further data (from this setting or other settings) would allow you (a) to test your initial hypotheses more thoroughly and (b) to generate other testable hypotheses?

Making Broader Links

No hypotheses are ever 'theory-free'. We only come to look at things in certain ways because we have adopted, either tacitly or explicitly, certain ways of seeing. This means that, in observational research, data collection, hypothesis-construction and theory-building are not three separate things but are interwoven with one another.

This process is well described by using an analogy with a funnel:

Ethnographic research has a characteristic 'funnel' structure, being progressively focused over its course. Progressive focusing has two analytically distinct components. First, over time the research problem is developed or transformed, and eventually its scope is clarified and delimited and its internal structure explored. In this sense, it is frequently only over the course of the research that one discovers what the research is really 'about', and it is not uncommon for it to turn out to be about something quite remote from the initially foreshadowed problems. (Hammersley and Atkinson: 1983, 175)

For instance, my research on the two cancer clinics unexpectedly led into a comparison of fee-for-service and state-provided medicine. Similarly, my observation of a paediatric cardiology unit moved unpredictably in the direction of an analysis of disposal decisions with a small group of Down's Syndrome children.

We may note three features which these two cases had in common:

- 1 The switch of focus – through the 'funnel' – as a more defined topic arose.
- 2 The use of the comparative method as an invaluable tool of theory-building and testing.
- 3 The generation of topics with a scope outside the substantive area of the research. Thus the 'ceremonial orders' found in the cancer clinics are not confined to medicine, while the 'democratic' decision-making found with the Down's children had unexpected effects of power with a significance far beyond medical encounters.

Working this way parallels Glaser and Strauss' (1967) famous account of grounded theory. A simplified model of this involves these stages:

- an initial attempt to develop categories which illuminate the data
- an attempt to 'saturate' these categories with many appropriate cases in order to demonstrate their relevance
- developing these categories into more general analytic frameworks with relevance outside the setting.

Glaser and Strauss use their research on death and dying as an example. They show how they developed the category of 'awareness contexts' to refer to the kinds of situations in which people were informed of their likely fate. The category was then saturated and finally related to non-medical settings where people learn about how others define them (e.g. schools).

'Grounded theory' has been criticised for its failure to acknowledge implicit theories which guide work at an early stage. It also is clearer about the generation of theories than about their test. Used unintelligently, it can also degenerate into a fairly empty building of categories (aided by the computer software programs already discussed) or into a mere smoke-screen used to legitimise purely empiricist research (see Bryman: 1988, 83–87). At best, 'grounded theory' offers an approximation of the creative activity of theory-building found in good observational work, compared to the dire abstracted empiricism present in the most wooden statistical studies.

Styles of Theorising in Observational Work

Throughout this chapter, I have used relevant examples to give concrete illustrations of the methodological issues I have been covering. Now it is time to examine the competing claims of two different theories underlying observational research: interactionism and ethnomethodological ethnography.

However, I do not want to provide a purely theoretical discussion of rival 'schools' of sociology. My solution is to offer some illustrations of how different sociological traditions have provided different but fruitful ways of thinking about observational data.

Interactionism

Interactionist principles: Interactionism is concerned with the creation and change of symbolic orders via social interaction. For instance, Goffman (1964) has shown how social stigma is recognised by the rest of us and how stigmatised people manage their status. In another famous study, Goffman (1961a) outlines what he calls a 'mortifying process' whereby 'total institutions' (like armies, boarding schools and monasteries) strip away previous identities in order to create an identity that is consistent with the institution.

This concern with identity and the symbolic order has an important implication for how interactionists view methodology. While positivists can view methods as mere techniques of more or less efficient data-gathering, the interactionist is bound to view research itself as a symbolic order based on interactions. Consequently, Denzin properly points out that for him, as an interactionist, 'Methodology . . . represents the particular ways the sociologist acts on his environment' (1970, 5).

For Denzin, methods cannot be neutral instruments because they define how the topic will be symbolically constituted and how the researcher will adopt a particular definition of self vis-à-vis the data. For instance, interactionists are likely to define themselves in a subject-to-subject

Denzin presents seven methodological principles which stem from this perspective. I have amalgamated some of his points in Table 3.2, while citing some examples for each principle taken from an early study by Becker (1953) 'Becoming a Marijuana User' (see p. 33, above).

Table 3.2: *Interactionism's Methodological Principles*

Principle	Implication	Example
1 Relating symbols and interaction	Showing how meanings arise in the context of behaviour	Behaviour of marijuana users in the presence of non-users (Becker 1953)
2 Taking the actors' points of view	Learning everyday conceptions of reality; interpreting them through sociological perspective	Becker's observations of a drug culture
3 Studying the 'situated' character of interaction	Gathering data in naturally-occurring situations	Observing people in their own environments
4 Studying process as well as stability	Examining how symbols and behaviour vary over time and setting	Studies of 'moral careers' (Becker 1953, Goffman 1961a)
5 Generalising from descriptions to theories	Attempting to establish universal interactive propositions	Goffman (1981) on 'forms' of interaction

Source: adapted from Denzin: 1970, 7-19

Following a practice common to interactionists, Denzin uses the term 'participant observation' rather than 'ethnography' to index the research methodology most appropriate to his perspective. Such a method involves sharing in people's lives while attempting to learn their symbolic world. The way it is used will depend on the precise role carved out by the researcher, varying from a 'complete participant' to the 'complete observer'.

Denzin rightly suggests that participant observation embodies the principles as set out in Table 3.2. It involves taking the viewpoint of those studied, understanding the situated character of interaction and viewing social processes over time, and can encourage attempts to develop formal theories grounded in first-hand data. Unlike survey research, Denzin points out, 'the participant observer is not bound in his field work by pre-judgements about the nature of his problem, by rigid data-gathering devices, or by hypotheses' (*ibid*, 216).

Unlike some interactionist work which may fail to improve upon good descriptive journalism, Denzin's principle 5 proposes that a description of content serves only as a prelude to analytic work. Basing himself on Glaser and Strauss' (1967) distinction between 'substantive' and 'formal' theory, he reminds us that the intrinsic fascination of much ethnographic data should be a stepping stone towards the attempt to establish 'universal interactive propositions' (Denzin: 1970, 19).

In this respect, Denzin's approach shares the analytic breadth that we found in cognitive anthropology. It underlines the point that good ethnography should not limit itself to a set of descriptions about how people behave in different settings. On the contrary, ethnography shares the social science programme of producing general, possibly even law-like, statements about human social organisation.

Denzin also notes that participant observation is not without its own difficulties. First, its focus on the present may blind the observer to important events that occurred before his entry on the scene. Second, as Dalton (1959) points out, confidants or informants in a social setting may be entirely unrepresentative of the less open participants. Third, observers may change the situation just by their presence and so the decision about what role to adopt will be fateful. Finally, the observer may 'go native', identifying so much with the participants that, like a child learning to talk, he cannot remember how he found out or how to articulate the principles underlying what he is doing.

It is now time to turn from principles and consider some exemplary interactionist studies.

Interactionist studies: When you observe face-to-face behaviour within a part of your culture with which you are familiar, it may all strike you as terribly 'obvious' and unremarkable. Perhaps that was your experience when you attempted some of the early exercises in this chapter.

If so, you would have been helped by reading the early work of Erving Goffman. Goffman shows us two recurrent kinds of rules used to organise social interaction:

- 1 Rules of courtesy, manners and etiquette (who is able to do and say what to whom and in what way?).
- 2 Depending upon the definition of the situation, rules of what is relevant or irrelevant within any setting.

As Goffman points out, these rules give us a clue to understanding what is going on in definitions of situations in face-to-face encounters. For: 'instead of beginning by asking what happens when this definition of the situation breaks down, we can begin by asking what perspectives this definition of the situation *excludes* when it is being satisfactorily sustained' (Goffman: 1961b, 19, my emphasis). In Goffman's later terminology, rules of relevance and irrelevance constitute the *frames* through which a setting is defined.

Viewing 1000 doctor-parent consultations in Scottish clinics in the 1970s, Strong at first struggled to see anything remarkable about what was going on. Only when he gathered some comparative data on private and 'charity' clinics in the United States did the relevance of 'frame' become so apparent. Now he saw cases where...

ists. He also saw cases (in the 'charity' clinic) where mothers' 'good intentions' were openly challenged by doctors.

Strong (1988) reports that interaction in the Scottish clinics could now be seen to be framed in the following ways:

- 1 Parents were portrayed as passive and technically ignorant.
- 2 Nonetheless parents' behaviour towards their children was never publicly challenged: 'every Scottish mother was nominally treated as loving, honest, reliable and intelligent' (*ibid*, 240).
- 3 Mothers were portrayed as more knowledgeable than fathers and mothers of many children or foster-mothers were held to be more reliable witnesses than other mothers.
- 4 Doctors were anonymous and interchangeable – enjoying 'collegial' authority.

Strong's study shows how Goffman's concept of frame allows the observer to generate important questions. Following Sherlock Holmes, what may be most significant is 'the dog that did not bark at night', i.e. what does *not* happen or seems irrelevant.

In a more recent study of a ward for terminally ill patients, Anssi Peräkylä (1989) has shown how staff can use four different frames to define themselves and their patients:

- 1 *The practical frame* defines staff in terms of the practical tasks they need to carry out in the ward; patients become the mere objects of such tasks.
- 2 *The medical frame* defines staff in terms of the activities of diagnosis and therapeutic intervention; once again patients become objects.
- 3 *The lay frame* makes staff into ordinary people, able to feel anguish and grief; it redefines the patient as a feeling and experiencing subject.
- 4 *The psychological frame* defines staff as objective surveyors of the emotional reactions of patients; patients are both subjects (who feel and experience) and objects (of the knowing psychological gaze).

However, Peräkylä's study, which is very rich, goes beyond a mere attempt to catalogue different frames. First, he shows the contingencies associated with a move between different frames. It turns out, for instance, that the psychological frame is a powerful means of resolving the identity-disturbances found in other frames – where a patient is resisting practical or medical framing, for instance, this can be explained in terms of his psychological state. Second, the psychological frame also seemed to be a convenient means for the staff to talk about their activities to Peräkylä himself and to define his presence to each other and to patients.

As used by Strong and Peräkylä, within a broadly interactionist perspective, Goffman's concept of 'frame' offers a powerful way to ask questions about observational data. Moreover, in related studies like Dingwall and Murray (1983) and Silverman (1984), an ever-growing, *cumulative* body of knowledge is emerging about how framing works in professional-client settings.

Exercise 3.4

This is an exercise to give you some experience of using the concept of 'frame' which is concerned with what people treat as 'relevant' or 'irrelevant' in any social activity.

Choose any setting with which you are familiar (e.g. a meeting with friends in a restaurant or bar, a family gathering, a college class, a religious ceremony, a television soap-opera). When you next participate in (or watch) this setting, consider the following questions:

- 1 Which frames are people using to organise their activities?
- 2 What functions are served by each frame?
- 3 How and why do people move between frames?
- 4 Do any 'out-of-frame' activities occur? If so, how are these handled? If not, what would they look like?

As Peräkylä (1989) shows, such studies open up a series of questions with a strongly practical relevance. Following my work on the hidden 'power-plays' of apparently 'democratic' consultations, Peräkylä reveals that the 'psychological' frame serves a multiplicity of purposes which is not coterminous with 'greater understanding'. As he argues:

Instead of arguing for or against the use of social-psychological models in medicine, sociology should explicate the way these models are used, the circumstances that they are applied in, and the intended and unintended consequences of their use. These are social issues, permeated by power relations. (Peräkylä: 1989, 131)

It is now time to turn to an alternative way of exploring these questions.

Ethnomethodological Ethnography

Ethnomethodological principles: Several decades ago, the links between good interactionist ethnography and ethnomethodology were noted by Harvey Sacks:

Instead of pushing aside the older ethnographic work in sociology, I would treat it as the only work worth criticising . . . where criticising is giving some dignity to something . . . the relevance of the works of the Chicago sociologists is that they contain a lot of information about this and that. And this-and-that is what the world is made up of. (Sacks: 1989, 254)

In this transcript of a lecture given in the 1960s, Harvey Sacks suggests that there is a continuity between the older Chicago School work and the then newly emerging ethnomethodological studies. The basis for this continuity, Sacks suggests, is not theoretical but methodological.

Sacks admires the painstaking attention to detail (this and that) of the

He implies that, in this respect, it surpasses that kind of sociology which deals merely in huge generalisations.

Like the older ethnographers, Sacks also rejected the crass empiricism of certain kinds of quantitative sociology. In particular, its assumptions that research is based on finding some indices and explaining why they rise and fall by *ex post facto* interpretations of significant correlations.

Sacks was convinced that serious work paid attention to detail and that, if something mattered, it should be observable. For instance, in a fascinating passage, Sacks noted the baleful influence on sociology of Mead's proposal that we need to study things which are not available to observation, e.g. 'society', 'attitudes'. He comments:

But social activities are observable, you can see them all around you, and you can write them down. The tape recorder is important, but a lot of this can be done without a tape recorder. If you think you can see it, that means we can build an observational study. (1992a, 28)

If Chicago School ethnography ultimately fails, its failure lies in not going far enough in its pursuit of detail. The proper model of scientific enquiry, Sacks assumes, derives from the natural sciences in the sense that such sciences provide precise information about their data and research instruments. The aim is to give the reader as much information as the author and the ability to reproduce the analysis. This leads Sacks to identify two methodological limitations of Chicago School ethnography:

- 1 It fails to reproduce its data in a form which allows the reader to reproduce the analysis.
- 2 It often depends on using native informants. This reveals the categories that people use but 'they're not investigating the categories by attempting to find them in the activities in which they're employed' (*ibid*, 254).

Sacks' problem is that most ethnography depends upon generalisations made on the basis of truncated data extracts and/or responses elicited from informants. There are, he implies, five simple solutions:

- 1 Provide the reader with transcripts based on tape-recordings of naturally occurring activities.
- 2 Always focus on what is *observable*, like behaviour, and avoid that which is not observable, like motivations or attitudes (although we may study how people talk about one another's 'motivations' and 'attitudes').
- 3 Avoid abstractions and early generalisations, while carefully sorting through your material:

the more material you have at your command, the more you ought to be able to pick up items and see their recurrence and get some idea of what they might be doing. But the way to proceed is item by item. (*ibid*)

- 4 Reject the anti-scientific position of some...

laboratory studies only for their lack of success, not for their aim of producing a 'science of society'. So laboratory studies of, say, 'short-term memory' fail because they ask the wrong question, namely 'do people understand what somebody else says?'. Instead, researchers should be asking: 'is there some *procedure* people use which has, as its product, a showing that they heard and understood?' (Sacks: 1992b, 30, my emphasis).

- 5 Make common sense, as Garfinkel (1967) put it, a 'topic' not just a tacit 'resource'. Thus the problem with *both* survey research and much ethnography is that they fail to topicalise their understandings. As Sacks says:

Now (w)hat I want to do is turn that around: to use what 'we' know, what any Member [i.e. member of society] knows, to pose us some problems. What activity is being done, for example. And then we can see whether we can build an apparatus which will give us those results. (1992a, 487)

Ethnomethodological studies: I have only space for two examples of the kind of work that follows from Sacks' recommendations. One example is sociological (Maynard: 1989) and one is anthropological (Moerman: 1974).

Writing twenty years after Sacks, Maynard notes how ethnographers are still trying to picture how people see things rather than focussing on what is observable. As he puts it:

In doing ethnography, researchers attempt to draw a picture of what some phenomenon 'looks like' from an insider's account of the phenomenon and for some audience who wants to know about it. The ethnographer, in general, is in the business of describing culture from the members' point of view. (Maynard: 1989, 130)

Maynard notes how such concerns have shaped research in one part of the sociology of law. 'Plea bargaining' has been identified as a process by which defendants plead guilty to a 'lesser' offence, thereby minimising their punishment and speeding up the work of the courts (evidence does not need to be heard if the defendant pleads guilty). Ethnographers have assumed that this process works on the basis of shared perceptions held by prosecution and defence lawyers.

However, Maynard suggests that such ethnographic work, based on the identification of people's perceptions, has at least three deficiencies:

- 1 It depends upon common-sense knowledge: 'ethnographers rely on unnoticed abilities to record and recognise such features, just as participants rely on basically uninvestigated abilities in producing them' (*ibid*).
- 2 It glosses over what 'plea bargaining' actually is – the diversity of discourse that gets called 'plea bargaining'.
- 3 It fails to treat the common orientation of the parties concerned as an

Instead, following Sacks' emphasis on what is observable, Maynard studies 'how a sense of mutuality is accomplished' (*ibid*). This involves examining how plea bargaining sequences are introduced into the talk. For instance, a bargaining proposal can be solicited or it can be announced, as shown in Table 3.3.

Table 3.3: *Two Forms of Plea Bargaining*

SOLICITATION	
(solicit)	PD ¹ : Is there an offer in this case?
(proposal)	DA ² : I would say in this case a fine, seventy five
ANNOUNCEMENT	
(announcement)	PD: I'll propose a deal to you
('go-ahead' signal)	DA: Tell me what ya got
(proposal)	PD: If ya dismiss the 242, I might be able to arrange a plea to 460 for a fine

¹ PD = Public Defender.

² DA = District Attorney.

Source: Maynard 1989, 134

Maynard's study draws attention to how the phenomenon of 'plea bargaining' is itself locally constituted in the activities of the participants. As I argued earlier, in Chapter 2, there is a danger that, if ethnography reduces social life to the definitions of the participants, it becomes a purely 'subjectivist' sociology which loses sight of *social* phenomena.

Instead, the point is to narrow the focus to what people are *doing*. As Maynard puts it:

The question that ethnographers have traditionally asked – 'How do participants see things?' – has meant in practice the presumption that reality lies outside the words spoken in a particular time and place. The . . . [alternative] question – 'How do participants do things?' – suggests that the microsocial order can be appreciated more fully by studying how speech and other face-to-face behaviours constitute reality within actual mundane situations. (Maynard: 1989, 144)

Maynard underlines Sacks' position on the need to provide transcripts, to focus on what is observable and to proceed in a cautious, step-by-step manner. We can see the full force of Sacks' arguments in the experiences of one ethnographer who attempted a study of a tribe living in Thailand. As a cognitive anthropologist, Michael Moerman was interested in learning the categorisation systems employed by this native people.

Like most anthropologists and Chicago School ethnographers, he used native informants. His aim was to elicit from them what 'being a Lue' (the name of the tribe) meant to them. However, he was troubled about what sense to read in their accounts.

First, his questions often related to issues which were either obvious or irrelevant to the respondents. As he puts it: 'To the extent that answering an ethnographer's question in an unambiguous way is a

reason from a native's answer to his *normal* categories or ascriptions' (Moerman: 1974, 66, my emphasis).

Second, it was not so straightforward to switch to observational methods while pursuing the tribe's 'identity'. Even when the ethnographer is silent and merely observes, his presence indicates to people that matters relevant to 'identity' should be highlighted. Consequently, people may pay particular attention to what both he and they take to be relevant categorisation schemes – like ethnic or kinship labels. In this way, the ethnographer may have 'altered the local priorities among the native category sets which it is his task to describe' (*ibid*, 67).

What, then, is to be done? A clue is given by the initially opaque sub-headings of his article: 'Who are the Lue?' 'Why are the Lue?' 'When are the Lue?'

Moerman argues that there are three reasons why we should *not* ask: 'Who are the Lue?' First, it would generate an inventory of traits. Like all such inventories it could be endless because we could always be accused of having left something out. Second, lists are retrospective. Once we have decided that the Lue *are* a tribe, then we have no difficulty in 'discovering' a list of traits to support our case. Third, the identification of the Lue as a tribe depends, in part, on their successful presentation of themselves as a tribe. As Moerman puts it: 'The question is not "Who are the Lue?" but rather when how and why the identification "Lue" is preferred' (62).

Moerman adds that this does *not* mean that the Lue are not really a tribe or that they fooled him into thinking they were one. Rather their ethnic identity arises in the fact that people in the area use ethnic identification labels some of the time that they are talking about each other.

Of course, some of the time is not all the time. Hence the task of the ethnographer should be to observe when and *if* ethnic identification labels are used by the participants being studied.

Moerman neatly summarises ethnomethodology's alternative to other forms of ethnography:

Anthropology [has an] apparent inability to distinguish between warm . . . human bodies and one kind of identification device which some of those bodies sometimes use. Ethnic identification devices – with their important potential of making each ethnic set of living persons a joint enterprise with countless generations of unexamined history – seem to be universal. Social scientists should therefore describe and analyse the ways in which they are used, and not merely – as natives do – use them as explanations. (Moerman: 1974, 67–68)

Conclusion: The Unity of the Ethnographic Project

I want to conclude this chapter by trying to locate points of contact between the two forms of ethnography that I have been considering. Consonant with the argument deployed throughout this book, researchers have more to learn by exploring the interstices between analytic positions

would be entirely mistaken to believe that all the certainties in observational work derive from ethnomethodological insights.

In fact, as I have argued already, a number of ethnographers have either taken on board many of these insights or reached them independently. For instance, a recognition that social phenomena are locally constituted (through the activities of participants) is not confined to Moerman and Maynard. Using the example of studies of the 'family', I want to show another direction from which one can draw the same conclusion.

Exercise 3.5

This exercise is meant to test how well you have acquired the skills outlined in the work of Sacks, Maynard and Moerman.

Your task is to describe the outline of a small-scale observational study of a group of people near at hand to you (e.g. students, family, workers).

- 1 What is your topic (i.e. which group are you going to study and what are you interested in about them)?
- 2 What methods can you use to gather data (e.g. observation, tape-recording, interview, etc.)? What will each method tell you?
- 3 How can you study 'when', 'how' and 'why' this is a group rather than 'who' or 'what' is the group?
- 4 What will you learn in this way that you would not learn by asking 'who' or 'what' is the group?
- 5 Imagine that you later discover that the members of the group do not agree with your conclusions. Should you modify them?

In a paper on methodological issues in family studies, Gubrium and Holstein (1987) show how much sociological work assumes that 'family life' is properly depicted in its 'natural' habitat – the home. Conversely, they argue that 'the family' is not a uniform phenomenon, to be found in one setting, but is 'occasioned' and 'contexted'.

We can see more clearly what they are saying in Table 3.4, which contrasts the 'conventional understanding' with Gubrium and Holstein's alternative.

Gubrium and Holstein's alternative direction for family studies closely fits Sacks' approach, while opening up a number of fascinating areas for family studies, as set out below:

- 1 Once we conceive of 'the family' in terms of a researchable set of descriptive practices, we are freed from the methodological and ethical nightmare of obtaining access to study families 'as they really are', i.e. in their own households.
- 2 We can now study how the structures of family organisation are depicted in different milieux (e.g. employment agencies, schools, clinics, etc.).

Table 3.4: *Two Ways of Describing 'Family Life'*

THE CONVENTIONAL UNDERSTANDING

- 1 Families have 'inner' and 'outer' sides
- 2 The 'inner' side is located in the household
- 3 Outside households we obtain only a 'version' of this 'prime reality'
- 4 Members of the household have a privileged access to family order
- 5 Participant observation is required to obtain 'authentic understanding' of family life

AN ALTERNATIVE

- 1 'Family' is a way of interpreting, representing and ordering social relations
- 2 The family is not private but inextricably linked with public life
- 3 The household does not locate family life
- 4 The household is not 'trivial' because it is often appealed to by laypeople and professionals alike as the determinant of family life

Source: adapted from Gubrium & Holstein: 1987

- 3 This links to studies of the social distribution of 'knowledge' about the family (e.g. when, where and by whom theories of the nature and consequences of 'broken homes' are employed).
- 4 It also ties in with the study of how different organisational routines constrain particular depictions of family order.

As already noted, issues of household location and privileged access now become redefined as topics rather than troubles – for example, we might study the claims that professionals make for such access. This underlines Gubrium and Holstein's point that family knowledge is never purely private. Family members themselves appeal to collective representations (like maxims and the depictions of families in soap-operas) to explain their own behaviour. Family members also present the 'reality' of family life in different ways to different audiences and in different ways to the same audience (see Gubrium and Holstein: 1987 for a fuller elaboration of this argument).

Gubrium and Holstein offer an exciting prospectus for family studies and an appropriate way to conclude this chapter on observation. For this kind of work (elsewhere termed 'articulative ethnography' by Gubrium: 1988), together with ethnomethodology, offers three crucial insights for observational studies, as follows:

- 1 It switches attention away from a more psychological orientation around what people are thinking towards what they are doing.
- 2 It shows the analytic issues that lie behind methodological puzzles.
- 3 It firmly distinguishes social science observational work from journalism and common sense, thus, in a certain sense, it is a more scientific approach.

As Michael Moerman once commented: 'Folk beliefs have honourable status but they are not the same intellectual object as a scientific analysis' (Moerman: 1974, 55).

Exercise 3.6

This exercise encourages you to use the 'alternative' version of describing family life.

Imagine that you wish to do an observational study of the family. Now consider the following questions:

- 1 What are the advantages and disadvantages of obtaining access to the family household?
- 2 In what ways may families be studied outside the household setting? What methodology might you use and what questions could you ask?
- 3 What might observation tell you about 'the family' in each of the following settings:
 - law courts
 - doctor-patient consultations
 - television soap-operas?

(EITHER do a study of ONE of these settings OR write hypothetically about all THREE.)

- 4 What does it mean to say you are studying 'the family' (i.e. within inverted commas)?

4

Texts

British and American social scientists have never been entirely confident about analysing texts. Perhaps, in (what the French call) the Anglo-Saxon cultures, words seem too ephemeral and insubstantial to be the subject of scientific analysis. It might seem better, then, to leave textual analysis to literary critics and to concentrate on definite social phenomena, like actions and the structures in which they are implicated.

This uncertain, occasionally cavalier, attitude to language is reflected in the way in which so many sociological texts begin with fairly arbitrary definitions of their 'variables'. The classic model is Durkheim's *Suicide* which offers a 'conclusive' definition of the phenomenon in its first few pages and then rushes off to investigate it in these terms. As Atkinson (1978) has pointed out, this method rules out entirely any analysis of the very social processes through which suicide is socially defined – particularly in the context of coroners' own definitional practices.

In most sociology, then, words are important simply as a jumping-off point for the real analysis. Where texts are analysed, they are usually presented as 'official' or 'common-sense' versions of social phenomena, to be undercut by the underlying social phenomena displayed in the sociologist's analysis of social structures. The model is: people say X, but we can show that Y is the case.

There are four exceptions to this general rule:

Content Analysis

Content analysis is an accepted method of textual investigation, particularly in the field of mass communications. It involves establishing categories and then counting the number of instances when those categories are used in a particular item of text, for instance a newspaper report.

Content analysis pays particular attention to the issue of the *reliability* of its measures – ensuring that different researchers use them in the same way – and to the *validity* of its findings – through precise counts of word use (see Selltitz *et al.*: 1964, 335–342). However, its theoretical basis is unclear and its conclusions can often be trite. Because it is a quantitative method, it will not be discussed in detail in this text. However, I will later present a study of political articles (Silverman: 1982) which combines qualitative textual