

and ethical considerations. At the same time, however, these techniques play an important role in promoting the quality of ethnographic research. They provide a crucial resource in assessing typicality of examples, checking construct-indicator linkages, searching for negative cases, triangulating across different data sources and stages of the fieldwork, and assessing the role of the researcher in shaping the nature of the data and the findings. In short, they facilitate – but should not determine – the process of analysis, a topic to which we turn in the next chapter.

Chapter 8

The process of analysis

In ethnography the analysis of data is not a distinct stage of the research. In many ways, it begins in the pre-fieldwork phase, in the formulation and clarification of research problems, and continues through to the process of writing reports, articles, and books. Formally, it starts to take shape in analytic notes and memoranda; informally, it is embodied in the ethnographer's ideas and hunches. And in these ways, to one degree or another, the analysis of data feeds into research design and data collection. This iterative process is central to the 'grounded theorizing' promoted by Glaser and Strauss, in which theory is developed out of data analysis, and subsequent data collection is guided strategically by the emergent theory (Glaser and Strauss 1967; Glaser 1978; Strauss 1987; Strauss and Corbin 1990). However, much the same interactive process is also involved in other kinds of ethnographic research, including those which are directed not towards the generation of theory but to other research products, such as descriptions and explanations.

This commitment to a dialectical interaction between data collection and data analysis is not easy to sustain in practice, however; and much ethnographic research suffers from a lack of reflexivity in this respect. The data required to check a particular interpretation are often missing; the typicality of crucial items of data cannot be checked; or some of the comparative cases necessary for developing and testing an emerging set of analytic ideas have not been investigated. One reason for this is the influence of naturalism, with its emphasis on 'capturing' the social world in description (Hammersley 1992:ch.1). Naturalism reinforces what Lacey (1976:71) calls 'the it's all happening elsewhere syndrome', a common ailment in fieldwork where the

researcher feels it necessary to try to be everywhere at once and to stay in the setting for as long as possible. As a result of this, a great deal of data is collected but little time is left for reflection on the significance of the data and the implications for further data collection. Likewise, the naturalistic commitment to 'tell it like it is' tends to force the process of analysis to remain implicit and underdeveloped.

However, there are also practical constraints on achieving the sort of close interaction between analysis, research design, and data collection that is desirable. Fieldwork is a very demanding activity, and the processing of data is equally time-consuming. As a result, engaging in sustained data analysis alongside data collection is often very difficult. However, some level of reflexivity can and should be maintained, even if it is not possible to carry out much formal data analysis before the main fieldwork has been completed. Some reflection on the data collection process and what it is producing is essential if the research is not to drift along the line of least resistance and to face an analytical impasse in its final stages.

Ethnographic research should have a characteristic 'funnel' structure, being progressively focused over its course. Over time the research problem needs to be developed or transformed, and eventually its scope is clarified and delimited, and its internal structure explored. In this sense, it is frequently well into the process of inquiry that one discovers what the research is *really* about; and not uncommonly it turns out to be about something rather different from the initial foreshadowed problems. An extreme example is some research by Shakespeare (1994), which started from a concern with how members of a housing cooperative accounted for its history, but eventually came to focus on the discursive structure of the 'confused talk' displayed by people suffering from various kinds of dementia. Here we have a dramatic change in substantive focus, though there is a continuing concern with the structure of interview discourse. Usually, shifts in research focus are less dramatic than this, more along the lines illustrated by Bohannon (1981). He identifies the various stages of a research project on poor residents of inner-city hotels, illustrating the importance of preliminary analysis; and he reports how the research problem was progressively redefined:

We did indeed begin this project with the 'notion' (it was

actually more formal than that – it was a hypothesis that proved wrong) that elderly individuals living in run-down hotels in the center city have established support networks. By and large they have not. Their networks are shallow and transient. It is [generally] part of the life adjustment of these people to run from the commitment that a support network implies.

(Bohannon 1981:45)

Starting from a view based on 'disorganization' or 'dislocation', Bohannon and his research team came to reformulate their research in terms of 'adaptation'. In the course of this they were able to argue that welfare policies predicated on the former were not soundly based.

Progressive focusing may also involve a gradual shift from a concern with describing social events and processes towards developing and testing explanations or theories. However, different studies vary considerably in the distance they travel along this road. Some remain heavily descriptive, ranging from narrative life histories of an individual, group, or organization to accounts of the way of life to be found in particular settings. Of course, these are in no sense pure descriptions: they are constructions involving selection and interpretation. But they may involve little attempt to derive any general theoretical lessons, the theory they employ remaining implicit, being used as a tool rather than forming the focus of the research. Such accounts can be of great value. They may provide us with knowledge of cultures hitherto unknown, and thereby shake our assumptions about the parameters of human life or challenge our stereotypes. Herein lies the interest of much anthropological work and of sociological accounts revealing the ways of life of low-status and deviant groups.

A variation on this theme is to show the familiar in the apparently strange (Goffman 1961; Turnbull 1973) or the strange in the familiar (Garfinkel 1967). An interesting recent application of this latter idea is Rawlings' explication of her knowledge as a participant in a therapeutic community. She takes an apparently ordinary first few minutes of a community meeting and shows that in many respects they were far from ordinary, that their appearance as ordinary was an interactional accomplishment, albeit a routine one (Rawlings 1988). Alternatively, descriptive

accounts may contrast present conditions with an ideal, pointing up the discrepancy. Decision-making procedures within a political institution may be compared with an ideal type of democracy, for example; or personnel selection practices in a business organization may be compared with its official policy. Such comparisons are the stock-in-trade of ethnographic work.

By no means all ethnography remains at this descriptive level, however. Often, there is an attempt to draw out explanations or theoretical models of one kind or another. Here, features of the nature or history of the phenomenon under study start to be collected under more general categories. They are presented as examples of, for example, particular kinds of social orientation, discursive practice, interactional strategy, institutional form, etc. Going further, typologies may be developed identifying orientations, strategies, etc., of various related kinds which can be found in very different sorts of setting (Lofland 1971 and 1976). Finally, a whole range of analytic categories may be integrated into a model of some aspect of the social process (Glaser and Strauss 1967; Glaser 1978; Lofland and Lofland 1984). And this may then be subjected to test and further revision.

This is a long road to travel and there are many way-stations along its course. Moreover, as with all journeys, something is left behind. Concrete descriptions usually cover many different facets of the phenomena they describe: they give a rounded picture and open up all manner of theoretical possibilities. The development of explanations and theories involves a narrowing of focus and a process of abstraction. Theorized accounts provide a much more selective representation of the phenomena with which they deal. On the other hand, assuming that the theoretical ideas are well founded, they begin to give us much more knowledge about why events occur in the patterned ways they do.

In general, ethnographers deal with what is often referred to as 'unstructured' data. What this means is that the data are not already structured in terms of a finite set of analytic categories determined by the researcher, in the way that most survey research data are. Rather, they take the form of open-ended verbal descriptions in fieldnotes, of transcriptions of audio- or video-recordings, extracts of text from documents, etc. And the process of analysis involves, simultaneously, the development of a set of analytic categories that capture relevant aspects of

these data, and the assignment of particular items of data to those categories.

There is a wide variety of approaches to analysis of this sort. This arises partly from the diverse purposes of social research. Someone concerned with how the sequencing of contributions to everyday conversation is organized is likely to adopt a very different approach compared with someone interested in, say, the strength of the ties among an elite group and how this affects their exercise of power. Closely related to such differences in topic or purpose, of course, are differences in theoretical approach. There are those who would dismiss the first topic as trivial, just as there are those who would regard the second as beyond the realm of rigorous investigation, at least given the current state of social-scientific knowledge. Our approach here will be a catholic one, ruling out neither of these forms of research. However, we cannot cover the full range of varieties of qualitative analysis in detail. Instead, we will focus on what we take to be central to much of it.

GENERATING CONCEPTS

The initial task in analysing qualitative data is to find some concepts that help us to make sense of what is going on in the scenes documented by the data. Often we will not be sure *why* what is happening is happening, and sometimes we may not even understand *what* is going on. The aim, though, is not just to make the data intelligible but to do so in an *analytical* way that provides a novel perspective on the phenomena we are concerned with or which promises to tell us much about other phenomena of similar types.

The development of analytical categories and models has often been treated as a mysterious process about which little can be said and no guidance given. One must simply wait on the theoretical muse, it is sometimes implied. While we would certainly not wish to deny or downplay the role of creative imagination in research, we should point out that it is not restricted to the *emergence* of analytical ideas, but is equally important in devising ways of *developing and testing* these. Moreover, in neither case does recognition of the role of imagination negate the fact that there are general strategies available.

Besides obscuring the importance of strategies for generating

concepts and models, overemphasis on the role of creative imagination in the development of analytical ideas also leads us to forget the function that our existing knowledge of the social world performs in this process. It is only when we begin to understand that the imagination works via analogies and metaphors that this becomes plain. While it is rare for ethnographic analysis to begin from a well-defined theory, and indeed there are dangers associated with such a starting point, the process of analysis cannot but rely on the existing ideas of the ethnographer and those that he or she can get access to in the literature. What is important is that these do not take the form of prejudgments, forcing interpretation of the data into their mould, but are instead used as resources to make sense of the data. This requires the exercise of some analytic nerve, tolerating uncertainty and ambiguity in one's interpretations, and resisting the temptation to rush to determinate conclusions.

The first step in the process of analysis is, of course, a careful reading (indeed probably several readings) of the corpus of data, in order to become thoroughly familiar with it. At this stage the aim is to use the data to think with. One looks to see whether any interesting patterns can be identified; whether anything stands out as surprising or puzzling; how the data relate to what one might have expected on the basis of common-sense knowledge, official accounts, or previous theory; and whether there are any apparent inconsistencies or contradictions among the views of different groups or individuals, or between people's expressed beliefs or attitudes and what they do. Some such features and patterns may already have been noted in previous fieldnotes and analytic memos, perhaps even along with some ideas about how they might be explained. What sorts of pattern one is looking for depends, of course, on one's research focus and theoretical orientation. These will also affect how much data one collects and how one approaches the analysis. Some ethnographers, notably those employing conversation or discourse analysis, employ relatively small amounts of data and are concerned with local patterns visible within particular data sets. More usually, though, ethnographers collect quite large amounts of data of various kinds from different sources (observational fieldnotes and/or transcripts from various sites, interview notes and/or transcripts from different people, published and unpublished, official and personal documents, etc.);

and they seek relationships across the whole corpus. Here the aim is to compare and relate what happens at different places and times in order to identify stable features (of people, groups, organizations, etc.) that transcend local contexts.

Useful analytical concepts sometimes arise 'spontaneously', being used by participants themselves. And, indeed, unusual participant terms are always worth following up, since they may mark theoretically important or interesting phenomena (Becker and Geer 1957; Wieder 1974a and b; Becker 1993). Some forms of ethnography, especially those based on or influenced by 'ethnoscience', are devoted almost exclusively to the listing, sorting, and interpretation of such 'folk' terms. They are concerned with the more or less formal semantics of such inventories (see, for example, Tyler 1969). However, many ethnographies, while using folk types, attempt to do more than simply document their meaning. They are taken as evidence of knowledge, belief, and actions that can be located within more general analytic frameworks.

Alternatively, concepts may be 'observer-identified' (Lofland 1971); these are categories applied by the ethnographer rather than by members themselves. In the development of such classifications, the analyst may draw together under the aegis of a single type what for members is a diverse and unrelated range of phenomena. The formulation of such types can draw on general, common-sense knowledge and on personal experience. Equally, though, they can be generated by borrowing or adapting existing concepts from the literature. For instance, in their research on the transition of students from middle to high schools, Measor and Woods (1983) found that a variety of stories about life at the high school circulated among middle-school students, the most common one being that new entrants 'get their heads flushed in the loo' by older students. These stories had a standard form and seemed to reappear each year. Measor and Woods came to regard such stories as myths, and drew upon anthropological literature to understand the role they played in students' lives.

Sometimes, ethnographers find it necessary to formulate new terms to capture and characterize observer-identified types. Hargreaves provides an example with his development of the notion of 'contrastive rhetoric'. This

refers to that interactional strategy whereby the boundaries of normal and acceptable practice are defined by institutionally and/or interactionally dominant individuals or groups through the introduction into discussion of alternative practices and social forms in stylized, trivialized and generally pejorative terms which connote their unacceptability.

(Hargreaves 1981:309)

Hargreaves uses the notion to analyse talk in a school staff meeting (Hargreaves 1981:314), but he notes that many parallel applications are to be found in the sociology of the mass media and of deviance. He also draws attention to the similarities with the 'atrocity stories' sometimes produced by those in subordinate positions in medical settings (Stimson and Webb 1975; Dingwall 1977a).

At this stage in their development, the concepts will not usually be well-defined elements of an explicit analytical model. Rather, they will probably take the form of a loose collection of 'sensitizing concepts' (Blumer 1954). These contrast with what Blumer calls 'definitive concepts', which 'refer precisely to what is common to a class of objects, by the aid of the clear definition of attributes or fixed bench-marks'. A sensitizing concept, on the other hand, lacks such specificity, and 'it gives the user a general sense of reference and guidelines in approaching empirical instances. Where definitive concepts provide prescriptions of what to see, sensitizing concepts merely suggest directions along which to look' (Blumer 1954:7). Sensitizing concepts are an important starting point; they are the germ of the analysis, and they can provide a focus for further data collection.

Reading through the corpus of data and generating concepts which make sense of it are the initial stages of ethnographic analysis. Very often, the concepts used to start with will be relatively mundane ones. Later, more analytically significant ones may be added. For instance, in his analysis of teachers' talk in a school staffroom Hammersley developed categories that ranged from the very concrete (teacher talk about students, about teaching, about national political events, etc.) to rather more abstract and analytic ones (trading news about students, exchanging mutual reassurances, accounting for decline and crisis, defending teacher competence, etc.). Needless to say, the process of coding the data is a recurrent one; as new categories

emerge, previously coded data must be recoded to see if they contain any examples of the new codes. The ultimate aim, of course, is to reach a position where one has a stable set of categories and has carried out a systematic coding of all the data in terms of those categories. As we saw in the previous chapter, while there is no computer software which will do the coding automatically, there are various programs that facilitate the process and allow rapid retrieval or relating of data relevant to particular categories (see Dey 1993).

Having acquired some concrete and analytic categories for organizing the data, the next task is to begin to work on those which seem likely to be central to one's analysis, with a view to clarifying their meaning and exploring their relations with other categories. One strategy here is what Glaser and Strauss (1967) call the 'constant comparative method'. In this procedure, the analyst examines each item of data coded in terms of a particular category, and notes its similarities with and differences to other data that have been similarly categorized. This may lead to vaguely understood categories being differentiated into several more clearly defined ones, as well as to the specification of sub-categories. In this way, new categories or sub-categories emerge and there may be a considerable amount of reassignment of data among the categories.

As this process of systematic sifting and comparison develops, so the mutual relationships and internal structures of categories will be more clearly displayed. However, the development of analytical ideas rarely takes the purely inductive form implied by Glaser and Strauss (heuristically useful though their approach is). Theoretical ideas, common-sense expectations, and stereotypes often play a key role. Indeed, it is these that allow the analyst to pick out surprising, interesting, and important features in the first place. Blanche Geer's (1964) famous account of her 'first days in the field' is a classic exemplification of the place of assumptions and stereotypes in the development of analytic themes.

Where a category forms part of a typology or model developed by others, however loosely constructed, relations with other categories may be implied that can be tentatively explored in the data. Where the fit is good and the model is well developed, it may even be possible to set about rigorously testing it. However, only rarely are sociological models suf-

f clothes) ready made

ficiently well developed for hypotheses to be derived and tested in this way. Generally, the process of testing requires considerable further development of the theory or explanation as a precondition, and in particular specification of what would be appropriate indicators for its concepts. (For discussions of the nature of theory development in ethnography, indicating some areas of disagreement, see Woods 1985 and 1987; Hammersley 1987a and b. And for questions about what constitutes theory in ethnography, see Hammersley 1992:ch.1.)

Of course, the ethnographer need not limit him- or herself to a single theory as a framework within which to analyse the data. Indeed, there are advantages to be gained from what Denzin (1978) terms 'theoretical triangulation', approaching data with multiple perspectives and hypotheses in mind. Bensman and Vidich (1960) provide an interesting example of this from their community study of Springdale. They report that they subjected their data to theoretical perspectives derived from Redfield, Weber, Tonnies, Veblen, Merton, Lynd, Warner, Mills, Sapir, and Tumin. In each case they asked themselves: 'What in [these] theories would permit us to comprehend our data?' Theories were not simply taken as off-the-peg solutions to research problems, but were used to provide focus for the analysis and for further fieldwork. They go on to note that

When one set of theories does not exhaust the potentialities of the data, other sets can be employed to point to and explain the facts which remain unexplained. Thus, for any initial statement of the field problem a whole series of theories may be successively applied, each yielding different orders of data and each perhaps being limited by the special perspectives and dimensions on which it is predicated.

(Bensman and Vidich 1960:165-6)

Not all ethnographers accept the validity of this approach; some see different theories as mutually incompatible, or rule out some theoretical approaches as incompatible with ethnography (Fielding and Fielding 1986; Silverman 1993:157). However, our view is that one should use whatever resources are available which help to make sense of the data.

DEVELOPING TYPOLOGIES

Very often the categories that have emerged in the analysis will be used simply to produce a description and/or explanation of the case or cases investigated. But sometimes ethnographers attempt to develop more systematic typologies that hold out the prospect of application to data from other situations. Here, a more or less exhaustive set of sub-types of some general category is identified. A very common pattern is the specification of various strategies which some category or group of actors adopt, or could adopt, to deal with a problem that they face routinely. However, typologies can have other sorts of foci too. For instance, Karp (1993) develops a typology of responses by patients to the prescription of anti-depressant drugs. These are: resistance, trial commitment, conversion, disenchantment, and deconversion. Rather than seeing these as alternative strategies he treats them as phases through which most patients go in their 'depressive careers', though of course there is the possibility of patients taking somewhat different routes through this set of responses. Karp explicitly draws a parallel with Robbins's (1988) work which identifies stages of recruitment, conversion, and deconversion of people to a variety of religious groups.

These are the sorts of relationships among categories that ethnographers look out for. And once having produced typologies like these they may become interested in why particular strategies are adopted by particular sorts of people in particular circumstances, or why particular kinds of people follow particular career patterns.

Typologies in ethnographic accounts vary considerably in the degree to which they have been systematically developed. Lofland has complained that in this respect much ethnographic research suffers from 'analytic interruptus'. In their development of categories, Lofland suggests, many analysts fail 'to follow through to the implied logical conclusion... to reach [the] initially implied climax' (1970:42). Taking the example of typologies of strategies, Lofland argues that the investigator must take the time and trouble

- 1 to assemble self-consciously all his materials on how a [problem] is dealt with by the persons under study;
- 2 to tease out the variations among his assembled range of instances of strategies;

- 3 to classify them into an articulate set of... types of strategies; and
- 4 to present them to the reader in some orderly and preferably named and numbered manner.

(Lofland 1970:42-3)

Elsewhere, Lofland has provided an extended discussion of the varieties of typology and how they might be developed (Lofland 1971).

Lazarsfeld and Barton (1951) go even further in their recommendations for the systematic development of typologies. An initial set of categories differentiating a particular range of phenomena can be developed into a systematic typology, they argue, by specifying the dimensions underlying the discriminations it makes. Not only does this force clarification and perhaps modification of the categories already identified, it throws up other categories that may also be of importance.

We can illustrate this by reference to Glaser and Strauss's typology of 'awareness contexts'. They developed this concept to characterize the different kinds of social situation found among terminally ill hospital patients, their families, and medical personnel. The idea refers to the differential distribution of knowledge and understanding of the dying person's condition, from situations of 'closed awareness', when the patient is not informed of the diagnosis and prognosis, to 'open awareness', where the knowledge is shared openly by all parties. The idea of an awareness context is thus closely linked to the dynamics of information control characteristic of many medical encounters. However, in the following extract the notion is treated as a more general formal category. In such a formulation it is clearly applicable to a much wider range of social settings approximating to the general type of 'information games' (cf. Scott 1968). It is, for instance, directly applicable to the substantive issue of 'coming out' among homosexuals, and the management of the revelation or concealment of such an identity (Plummer 1975:177-96). Glaser and Strauss write:

We have singled out four types of awareness context for special consideration since they have proved useful in accounting for different types of interaction. An open awareness context obtains when each interactant is aware of the other's true identity and his own identity in the eyes of

the other. A closed awareness context obtains when one interactant does not know either the other's identity or the other's view of his identity. A suspicion awareness context is a modification of the closed one: one interactant suspects the true identity of the other or the other's view of his own identity, or both. A pretense awareness context is a modification of the open one: both interactants are fully aware but pretend not to be.

(Glaser and Strauss 1964:669)

PARTY A				
PARTY B	Knows	Pretends not to know	Suspects	Doesn't know
Knows	<i>Open</i>		<i>Suspicion</i>	<i>Closed</i>
Pretends not to know		<i>Pretence</i>	X	Y
Suspects	<i>Suspicion</i>			Z
Doesn't know	<i>Closed</i>			

Figure 2 Typology of awareness contexts

By identifying the dimensions underlying this typology, along the lines suggested by Lazarsfeld and Barton, we find that there are rather more possibilities than Glaser and Strauss's initial typology allows (see Figure 2). Furthermore, some of these new possibilities look fruitful, such as cell X, where one party pretends while the other suspects, and cells Y and Z, where one pretends while the other does not know. Glaser (1978) warns us against what he calls the 'logical elaboration' of categories, and he is right to do so. Typologies should not be extended beyond their analytic value. Nonetheless, specification of the dimensions underlying a typology encourages us to think seriously and systematically about the nature of each category and its relationships with others. It may help us to spot previously unconsidered possibilities, or unsuspected relationships among categories. (For a useful discussion of the exploration of relationships among categories, in the context of using microcomputers for handling qualitative data, see Dey 1993.)

CONCEPTS AND INDICATORS

There is little point in developing highly systematized typologies and models if they provide little purchase on one's data. The development of an effective typology is not a purely logical or conceptual exercise: there must be constant recourse to the material one is analysing. As the categories of the analysis are being clarified and developed in relation to one another, so also must the links between concepts and indicators be specified and refined. Sensitizing concepts must be turned into something more like definitive concepts. (This is a controversial proposal: there are those who argue that sensitising concepts render definitive concepts unnecessary in ethnographic research; see Williams 1976. However, it is unclear to us how sensitising concepts can be adequate for the later stages of analysis; see Hammersley 1989a and b.)

In moving between data and concepts we must take great care to note plausible alternative links to those made in the emerging analysis, and these need to be tested. While in no sense is it necessary, or even possible, to lay bare all the assumptions involved in concept-indicator linkages, it is important to make explicit and to examine those assumptions to which strong challenges can be made.

We can illustrate this by reference to Willis's (1977) classic research on the adaptations of working-class boys to school. Willis argues that the 'lads' he studied displayed a counter-culture, an 'entrenched, general and personalized opposition to "authority"'. In supporting this claim he uses descriptions of the behaviour of the 'lads' as well as quotations from group interviews such as the following comments about teachers:

JOEY: ... they're able to punish us. They're bigger than us, they stand for a bigger establishment than we do, like, we're just little and they stand for bigger things, and you try to get your own back. It's, uh, resenting authority I suppose.

EDDIE: The teachers think they're high and mighty 'cos they're teachers, but they're nobody really, they're just ordinary people ain't they?

BILL: Teachers think they're everybody. They are more, they're higher than us, but they think they're a lot higher and they're not.

SPANKSY: Wish we could call them first names and that... think they're God.

PETE: That would be a lot better.

PW: I mean you say they're higher. Do you accept at all that they know better about things?

JOEY: Yes, but that doesn't rank them above us, just because they are slightly more intelligent.

BILL: They ought to treat us how they'd like us to treat them....

JOEY: ... the way we're subject to their every whim like. They want something doing and we have to sort of do it, 'cos, er, er, we're just, we're under them like. We were with a woman teacher in here, and 'cos we all wear rings and one or two of them bangles, like he's got one on, and out of the blue, like, for no special reason, she says, 'take all that off'.

PW: Really?

JOEY: Yeah, we says, 'One won't come off', she says, 'Take yours off as well.' I said, 'You'll have to chop my finger off first.'

PW: Why did she want you to take your rings off?

JOEY: Just a sort of show like. Teachers do this, like, all of a sudden they'll make you do your ties up and things like this. You're subject to their every whim like. If they want something done, if you don't think it's right, and you object against it, you're down to Simmondsy (the head), or you get the cane, you get some extra work tonight.

PW: You think of most staff as kind of enemies...?

— Yeah.

— Yeah.

— Most of them.

JOEY: It adds a bit of spice to yer life, if you're trying to get him for something he's done to you.

(Willis 1977:11-12)

In assessing the way in which Willis links the concept of counter-culture with the various indicators he uses, we need to consider whether, for example, students' expressions of opposition to teachers reflect a general opposition to 'authority' as such, or only to particular types of authority. And, indeed, in the course of doing this, we may need to clarify the concept of

authority itself. Would it make any sense, for example, to argue that Joey, who seems to be the leader of the 'lads', has authority among them? Whether or not we use the concept of authority in a broad or narrow sense, we need to be clear about exactly what it is that the analysis claims the 'lads' are rejecting.

Another question we might ask is whether the 'lads' are opposed to all aspects of teachers' authority or only to those teacher demands that they regard as overstepping its legitimate boundaries. For example, the 'lads' complain about rules relating to their personal appearance, a complaint also reported in a similar study by Werthman (1963), dealing with members of urban gangs in the United States. However, whereas Willis takes such complaints as an indicator of a general antipathy to authority, Werthman interprets them as signifying the boundaries of what the boys he studied regarded as a teacher's legitimate area of control. Clearly, such alternative interpretations have serious implications for the character and validity of the analysis produced.

While the nature of the alternative interpretations that must be considered will vary between studies, we can point to a number of issues that must be borne in mind when examining concept-indicator links. These correspond to the dimensions we discussed in Chapter 2 in relation to sampling within cases.

Social context

The issue of context is at the heart of the conflicting interpretations of student behaviour to be found in the work of Willis and Werthman. For Willis, opposition characterized the 'lads' contacts with all forms of authority. For Werthman, on the other hand, the behaviour of gang members towards teachers varied across contexts according to the actions of the teacher and how these were interpreted.

We shall focus our discussion here on one of the most important elements of context: the audience to which the actions or accounts being used as data were directed. One important possible audience is, of course, the ethnographer. This is most obvious in the case of interviewing, an interactional format in which the researcher plays a key role through the questions he or she asks, however non-directive the interview is. In interviews the very structure of the interaction forces participants to be aware

of the ethnographer as audience. Interviewees' conceptions of the nature and purposes of social research, of the particular research project, and of the personal characteristics of the interviewer may, therefore, act as a strong influence on what they say.

This can be both a help and a hindrance in the production of relevant data and valid interpretations of them. 'Well-trained' informants and respondents can act as highly effective research assistants in reporting relevant data, data of which the ethnographer might not otherwise become aware. They will also make the data collection process much more efficient, since they can select out what is relevant from the mass of irrelevant data that is available to them.

There are some dangers here, though. The more 'sophisticated' the interviewee the greater the tendency for him or her to move away from description into analysis. While there is no such thing as pure description, it is essential to minimize the inference involved in descriptions used as data in order to provide for the possibility of checking and rechecking, constructing and reconstructing, interpretations of them. If the interviewee gives heavily theorized accounts of the events or experiences he or she is describing, however interesting or fruitful the theoretical ideas are, the database has been eroded.

Spradley (1979) provides a particularly good example of this problem, that of Bob, an informant he worked with in the course of his study of tramps. Bob had spent four years on skid row; he was also a Harvard graduate, and had gone on to do post-graduate work in anthropology. Spradley recounts:

On my next visit to the treatment center I invited Bob into my office. We chatted casually for a few minutes, then I started asking him some ethnographic questions. 'What kind of men go through Seattle City Jail and end up at this alcoholism treatment center?' I asked. 'I've been thinking about the men who are here,' Bob said thoughtfully. 'I would divide them up first in terms of race. There are Negroes, Indians, Caucasians, and a few Eskimo. Next I think I would divide them on the basis of their education. Some have almost none, a few have some college. Then some of the men are married and some are single.' For the next fifteen minutes he pro-

ceeded to give me the standard analytic categories that many social scientists use.

(Spradley 1979:53)

Where the researcher is particularly interested in the categories in terms of which participants view the world, this sort of account is of limited value. We must be careful, then, in analysing our material to be alert for the effects of audience in terms of people's views of the researcher's interests.

Even when the ethnographer is acting as observer, he or she may be an important audience for the participants, or at least for some of them. Informal questioning often forms part of participant observation, and Becker and Geer (1960) have pointed to the importance of distinguishing between solicited and unsolicited statements when assessing evidence. However, as we noted in Chapter 5, such a distinction is too crude. We cannot assume that unsolicited statements are uninfluenced by the researcher's presence. The same applies to other actions. It is now a central tenet of the sociological literature that people seek to manage impressions of themselves and of settings and groups with which they are associated (Goffman 1959). In a study of an Indian village community, Berreman (1962) only discovered the extent to which his data were the product of impression management by the villagers when he was forced to change his interpreter. This change modified his relationship with them, and produced different kinds of data.

Sometimes participants will actually tell an ethnographer that they have been presenting a front. Bogdan and Taylor quote the comment of an attendant in a state institution for the 'mentally retarded' made to an ethnographer at the end of the first day of fieldwork: 'Yeah, we didn't do a lot of things today that we usually do. Like if you wasn't here we woulda snitched some food at dinner and maybe hit a couple of 'em around. See before we didn't know you was an ok guy' (Bogdan and Taylor 1975:89). Of course, such admissions do not necessarily indicate that full access has finally been granted. While over the course of an extended stay in a setting participants generally acquire increasing trust in the ethnographer and find it more and more difficult to control the information available to him or her, members' creation and management of their personal fronts can prove a persistent problem. Thus, Punch (1979) reports how, at

a party he attended some months after completing intensive and long-term fieldwork on police work in Amsterdam, one of his informants revealed to him, under the influence of alcohol, that he had been kept well away from evidence of police corruption. In the case of observational data too, then, one must be aware of the possible effects of the ethnographer as audience.

However, this concern with reactivity, with the effects of the researcher on the nature of the data he or she collects, can be somewhat misleading. Much as quantitative researchers seek to minimize reactivity through standardization, under the influence of naturalism ethnographers sometimes regard any effects of their presence or actions on the data simply as a source of bias. It is true that it can be a threat to the validity of inferences. However, participants' responses to ethnographers may also be an important source of information. Data in themselves cannot be valid or invalid; what is at issue are the inferences drawn from them. The point is that the ethnographer must try continually to be aware of *how* his or her presence may have shaped the data.

Similar considerations even apply in interpreting documents and data produced through secret research. Here too we must bear in mind the ways in which audience considerations may have shaped the actions and accounts produced. In secret participant observation, assuming cover has not been 'blown', the ethnographer cannot be an audience, as such. However, he or she may be an important audience in one or another participant identity. And we must remember that documents are always written for *some* audience, perhaps for several different ones simultaneously. This will shape the nature of the document, through what is taken as relevant, what can be assumed as background knowledge, what cannot or should not be said, and what must be said even if it is untrue. In the same way, in open participant observation and interviewing, consideration of the effects of audience must be extended beyond the role of the ethnographer. (One of the strengths of even open participant observation, of course, is that in 'natural' settings other audiences are generally much more powerful and significant for participants than the ethnographer, and their effects are likely to swamp those of the research.)

The significance of audience is heightened by the fact that the participants in a setting rarely perceive themselves as a homogeneous grouping. Different categories, groups, or factions

are often involved, in relation to whom different fronts need to be maintained. And even within these divisions there will be more informal networks of communication that include some participants and exclude others, as Hitchcock shows in the case of a primary school's staff:

On many occasions throughout the fieldwork, staff's comments would be prefaced by such statements as 'I know it's unprofessional of me talking like this...', 'I don't suppose I should really be telling you this', '... don't tell him I said this for goodness sake'. On other occasions when staff told me things these prefaces were not present; it was rather assumed that I wouldn't 'blow the scene' by telling someone else what had been said about them. That is, I was 'trusted' to keep things quiet or to keep what was said to myself.

(Hitchcock 1983:30)

Different things will be said and done in different company. In particular we must interpret differently what is done 'in public' and what is done 'in private', since the category to which an action belongs may have important consequences for how it relates to actions and attitudes in other contexts. Of course, whether something is 'private' or 'public' is not always obvious, and there is a subtle shading between the two. One may have to know a setting very well in order to be able to recognize the public or private status of actions and even then it is easy to be mistaken. Indeed, what was public and private may get redefined retrospectively.

Even in the case of interviews, the ethnographer may not be the most significant audience, as we noted in Chapter 5. To one degree or another, and whatever assurances of confidentiality the ethnographer gives, interviewees may regard what they say as 'public' rather than 'private'; they may expect it to be communicated to others, or recorded for posterity. Krieger (1979a) provides an example from her research on a radio station. Reflecting on interviewees' confidence or trust, she remarks:

I came to think it reflected an expectation that this telling in the interview situation was more than to one person, it was a telling to the world at large, and not only a bid for recognition by that world, but also perhaps for forgiveness.

(Krieger 1979a:170-1)

Analysing data for the effects of audience is not, then, simply a matter of assessing the impact of the researcher, but also one of assessing the impact of any other audience the actor might be addressing, consciously or subconsciously. This applies to all forms of data and it is a crucial consideration if invalid inferences are to be avoided.

Time

What people say and do is produced in the context of a developing sequence of social interaction. If we ignore what has already occurred or what follows we are in danger of drawing the wrong conclusions. However, the temporal context of actions includes not only the host of events that occur before and after them but also the temporal framework in terms of which the people involved locate them. Glaser and Strauss (1968) provide a striking example in their study of how dying patients are dealt with by hospital staff. They note how staff construct and reconstruct conceptions of the dying trajectories of patients and how these play a key role in shaping their attitudes to the treatment of patients. Moreover, deviations from expected patterns can cause problems. How hospital staff react to signs of improvement in a patient, then, is dependent on the temporal context in terms of which they read those signs. Relevant here are not only what has happened in the past, but also estimates of what is likely to happen in the future. Nor is this restricted to the staff; patients' families may not always welcome signs of improvement in their condition, because these are seen as part of a painful and lingering death (Wright 1981).

Time is also an important consideration in the interpretation of interview data. Not only may what is said at one point in an interview be influenced by the interviewee's interpretation of what has been said earlier and what might be asked later, but it is also affected by what has happened to the person prior to the interview and what is anticipated in the near future. Ball (1983) has pointed out that many organizations are characterized by short- and long-term temporal cycles. Most universities and schools, for example, have terms whose beginnings and endings are important benchmarks for staff and students. Moreover, the different terms are not equivalent; they form part of a longer cycle based on the year – the autumn term is very different in

many ways from the spring term, for example. For students, the years form part of an even larger cycle, their first year as freshers being very different in status from their final year as seniors. Data, of whatever kind, recorded at different times need to be examined in light of their place within the temporal patterns, short or long term, that structure the lives of those being studied. (For more on such patterns, see Roth 1963 and Zerubavel 1979).

From this point of view there are considerable advantages to be gained from combining interviews with participant observation. Each may provide information about temporal contexts whose implications for interpreting data can be assessed. The dangers of neglecting the effects of time are particularly great where reliance is placed upon a single data source, especially interviews or documents. Where interviews are used alone it is wise to give over some interview space to casual conversation about current events in the interviewee's life. Indeed, this may be a useful way of opening the interview to build rapport.

Once again, it is not a matter of accepting or rejecting data, but rather of knowing how to interpret them; there is a great temptation to assume that actions, statements, or interview responses represent stable features of the person or of settings. This may be correct, but it cannot be assumed. Actions are embedded in temporal contexts and these may shape them in ways that are important for the analysis.

Personnel

Who is doing or saying things is an equally important consideration when it comes to assessing the relationship between concept and evidence. People's identities or social locations (that is, the patterns of social relationships in which they are enmeshed) can have two kinds of effect on the nature of the accounts or actions they produce. First, social locations determine the kind of information available to people. They clearly affect what it is possible for people to see and hear 'at first hand'; they also determine what people will get to know about, and how they will get to know things 'second hand'. The other way in which identities affect actions and accounts is through the particular perspectives that people in various social locations tend to generate and that will filter their understanding and

knowledge of the world, and shape their actions in it. In particular, the interpretation of information available to a person is likely to be selected and slanted in line with his or her prevailing interests and concerns. There may even be a strong element of wish-fulfilment involved. One must be aware of the possible effects of social location on all kinds of data, including ethnographers' own observational reports: we too occupy particular locations and what we observe, what we record, and how we interpret it will be influenced by these.

The implications of identity vary somewhat between information and perspective analysis. In the first, one is concerned with what information an account can provide about the cases being investigated. Here social location may be an important source of knowledge, but it is also a potential source of bias: it is a threat to validity that must be monitored. This kind of consideration must underlie the selection of informants and the interpretation of the data they provide, as well as the treatment of data from other sources. In perspective analysis, on the other hand, social location is no longer a source of bias, it is a key element in the analysis. Here the aim is precisely to document the perspectives of those in different social locations.

Of course, as we saw in Chapter 5, these two forms of analysis are complementary. And in the case of observational data produced by the ethnographer, their interaction is the essence of reflexivity.

The relationships between concepts and indicators must be assessed, then, by considering alternative interpretations of the data, and by following through the implications of particular interpretations to see if these are confirmed. And it is important here to take account of the dimensions of social context, time, and the people involved. However, some ethnographers have proposed more direct ways of testing these relationships. We will discuss two commonly mentioned strategies here: respondent validation and triangulation.

Respondent validation

A recognition of the importance of actors' social locations leads directly to the issue of 'respondent validation', a notion that has an uncertain and sometimes contested place in ethnographic

analysis. Some ethnographers have argued that a crucial test for their accounts is whether the actors whose beliefs and behaviour they are describing recognize the validity of those accounts (Lincoln and Guba 1985). The aim is therefore to 'establish a correspondence between the sociologist's and the member's view of the member's social world by exploring the extent to which members recognize, give assent to, the judgments of the sociologist' (Bloor 1978:548-9).

In his own research on the decision rules employed by ENT (ear, nose, and throat) specialists, Bloor sent each specialist he studied a report describing their assessment practices. This was accompanied by a letter that asked each specialist to 'read through the report to see how far it corresponded with his own impressions of his clinic practice'. Bloor then discussed the reports in interviews with the doctors. He argues that for the most part the exercise was successful: 'some respondents endorsed my description of their practices, and where they did not the nature of the exercise was such as to enable me to correct the analysis so that this assent was no longer withheld' (1978:549). Using a different strategy, Ball (1981 and 1984), in his study of Beachside Comprehensive School, held two seminars for the school's staff at which he presented some of his findings. Ball's experience was rather less happy and fruitful, and suggests that while there is merit in the strategy, it is far from being problem-free.

The value of respondent validation lies in the fact that the participants involved in the events documented in the data may have access to additional knowledge of the context – of other relevant events, of temporal framework, of others' ulterior motives, for example – that is not available to the ethnographer. They may be part of information networks that are more powerful than those accessible to the ethnographer. In addition, they have access to their own experience of events, which may be of considerable importance. Such additional evidence may materially alter the plausibility of different possible interpretations of the data. Thus, Moffat (1989:329) reports how the conclusions of his research on students at Rutgers University were modified by their responses when he taught preliminary versions in his anthropology classes there.

At the same time, it is important to recognize the limitations of respondent validation. The information people receive

through their networks may be false. Equally, we cannot assume that anyone is a privileged commentator on his or her own actions, in the sense that the truth of their account is guaranteed. As Schutz (1964) and others have noted, we can only grasp the meanings of our actions retrospectively. Moreover, these meanings must be reconstructed on the basis of memory; they are not given in any immediate sense. Nor will the evidence for them necessarily be preserved in memory. Much social action operates at a subconscious level, leaving no memory traces. Thus, in the case of Bloor's specialists, we cannot assume that they are consciously aware of the decision rules they use, or even that, infallibly, they can recognize them when someone documents them. In short, while people are well-placed informants on their own actions, they are no more than that; and their accounts must be analysed in the same way as any other data, with close consideration being given to possible threats to validity.

This is reinforced once we recognize that it may be in a person's interests to misinterpret or misdescribe his or her own actions, or to counter the interpretations of the ethnographer. Both Bloor and Ball point out that participants generally interpret data in the light of different concerns to, and sometimes by criteria at odds with, those of the ethnographer. Bloor acknowledges, for instance, that

I had expected the specialists to respond to the reports in a manner similar to that of an academic colleague when one asks him to criticize a draft paper one has written. I became aware of having made this assumption when it was violated – I suspected that some of the specialists had not read the report in the expected critical spirit. They had read the report, I felt, in the way that we today might read a nineteenth century religious tract – with a modicum of detached, superficial interest, with a feeling that it displayed a certain peculiar charm perhaps, but without being so moved by its content as to feel the necessity to define one's own beliefs and practices in accordance with it or in contrast to it. They were unversed in the conventions of academic sociological criticism and they were perhaps only marginally interested in the content of the reports.

(Bloor 1978:550)

As with all data collection and analysis, then, people's reactions to the ethnographer's account will be coloured by their social position and their perceptions of the research act. In the case of Bloor's doctors, they only had a marginal interest; Ball's school teachers, on the other hand, displayed a keener commitment. But this, too, was directly related to their social locations, and was at odds with that of the researcher:

many of the staff had apparently read my chapter solely in terms of what it had to say about them or their subject. There was little or no discussion of the general issues I was trying to raise or the overall arguments of the chapter. . . . I had taken as my task as ethnographer the description and analysis of large scale trends which extended as I saw them across the whole school, an overview. The staff responded from their particular view of the school, from the vantage point of the position they held.

(Ball 1984:18-19)

Ball's teachers interpreted his work as critical, and queried the validity of his findings. (Scarth reports a similar experience: Scarth 1986:202-3.)

Such feedback, then, can be highly problematic. Whether respondents are enthusiastic, indifferent, or hostile, their reactions cannot be taken as direct validation or refutation of the observer's inferences. Rather, such processes of so-called 'validation' should be treated as yet another valuable source of data and insight.

Triangulation

Respondent validation represents one kind of triangulation: the checking of inferences drawn from one set of data sources by collecting data from others. More specifically, data-source triangulation involves the comparison of data relating to the same phenomenon but deriving from different phases of the field-work, different points in the temporal cycles occurring in the setting, or, as in respondent validation, the accounts of different participants (including the ethnographer) differentially located in the setting. This last form of data-source triangulation can be extended indefinitely by showing each participant the others' accounts and recording his or her comments on them (Adelman

1977). This is very time-consuming but, besides providing a validity check, it also gives added depth to the description of the social meanings involved in a setting.

The term 'triangulation' derives from a loose analogy with navigation and surveying. For someone wanting to locate their position on a map, a single landmark can only provide the information that they are situated somewhere along a line in a particular direction from that landmark. With two landmarks, however, one's exact position can be pinpointed by taking bearings on both; one is at the point on the map where the two lines cross. In social research, if we rely on a single piece of data there is the danger that undetected error in our inferences may render our analysis incorrect. If, on the other hand, diverse kinds of data lead to the same conclusion, we can be a little more confident in that conclusion. This confidence is well founded to the degree that the different kinds of data have different likely directions of error built into them.

There are a number of other kinds of triangulation besides that relating to participants' accounts. First, there is the possibility of triangulating between different researchers. While team research has sometimes been used by ethnographers, often the data generated by different observers have been designed to be complementary, relating to different aspects of a setting or different settings, rather than intended to facilitate triangulation. Nevertheless, team research offers the opportunity for researcher triangulation. Of course, to maximize its potentialities the observers should be as different as possible, for example adopting very different roles in the field. Second, there is technique triangulation. Here, data produced by different data collection techniques are compared. To the extent that these techniques involve different kinds of validity threat, they provide a basis for checking interpretations. Ethnography often involves a combination of techniques and thus it may be possible to assess the validity of inferences between indicators and concepts by examining data relating to the same concept from participant observation, interviewing, and documents.

In triangulation, then, links between concepts and indicators are checked by recourse to other indicators. However, triangulation is not a simple test. Even if the results tally, this provides no guarantee that the inferences involved are correct. It may be that all the inferences are invalid, that as a result of systematic

or even random error they lead to the same, incorrect, conclusion. What is involved in triangulation is not the combination of different kinds of data *per se*, but rather an attempt to relate different sorts of data in such a way as to counteract various possible threats to the validity of our analysis.

One should not, therefore, adopt a naively 'optimistic' view that the aggregation of data from different sources will unproblematically add up to produce a more complete picture. Although few writers have commented on it, differences between sets or types of data may be just as important and illuminating. Lever (1981) provides a valuable insight into this. Researching sex differences in children's play, she collected data by means of questionnaires and diaries. The former suggested greater sex differences than the latter. Lever argues that this reflects varying effects of stereotyping according to 'the nature of the method or the posing of the question'. She claims that this is why the children's statements of what they 'usually do' collected in her questionnaire show stronger sex differences than the information about what they 'actually do' collected in diaries. In short, Lever suggests that 'abstract or unconditional inquiries yield responses that more closely correspond to a person's perceptions of social norms than inquiries of a concrete or detailed nature' (1981:205).

The lesson to be learned here, once again, is that data must never be taken at face value. It is misleading to regard some as true and some as false. Rather, as Lever's research indicates, what is involved in triangulation is a matter not of checking whether data are valid, but of discovering which inferences from those data are valid. Incidentally, it is worth noting that the sort of remarks offered by Zelditch (1962) on the suitability of different methods for field research, and by Becker and Geer (1957) on participant observation and interviewing, can be read in this light. These papers and others like them are normally cited either to advocate one method against another, or to commend the combination of different methods, but even more they lend weight to the idea of reflexive triangulation.

THEORIES AND THE COMPARATIVE METHOD

Ethnographers have sometimes been reluctant to admit that one of their concerns is the production of causal models. This stems

in part, no doubt, from the positivist connotations of the term 'causality', and perhaps also from a recognition of the extreme difficulty of assessing the validity of theoretical claims about causal relations. Nevertheless, theories implying causal relationships, not always clearly marked or expressed, are common in ethnographic work. It is important that the presence and significance of such theories are recognized and that they are explicated as fully as necessary, and, at some point, systematically developed and tested. (For a useful guide to the explication of causal models, see Hage and Meeker 1988.)

There is only one general method for testing causal relations – the comparative method – though there are different ways of using it. By assessing the patterning of social events under different circumstances, we can test the scope and the strength of the relationships posited by a theory. One version of the comparative method is the experiment. Here, at its simplest, a particular factor is varied across situations that are identical in all respects considered relevant. By holding constant factors involved in plausible rival theories, and by varying the cause specified in the theory being tested, the existence and strength of the presumed causal relation can be checked. Experiments are the most powerful means of assessing the validity of claims about causal relations. However, one can never be certain that all relevant variables have been controlled; and there are some serious disadvantages to the experimental method, notably its tendency to low ecological validity (its artificial character), as well as the political and ethical limits on its use. Given this, it is important to emphasize that experiments are not the only way in which the comparative method can be used to test causal hypotheses, even though they are taken as the ideal by positivism.

The positivist emphasis on the experiment as the model of scientific inquiry goes hand in hand with what Becker (1970) has called the 'single study model', which prescribes that all research be devoted to the rigorous testing of theoretical hypotheses. While ethnography can certainly be used to test theories, by no means all ethnographies are, or need to be, directed to this goal. As we saw earlier, instead they often provide relatively concrete descriptions or rather more developed typologies and models. And there is no obligation on the part of an ethnographer to engage in systematic theory testing in any particular

study. At the same time, it should be said that theories do require rigorous testing, and that many theoretical models developed in ethnographic research are still waiting in vain for such treatment. In this respect, ethnography as a whole suffers from an even more serious form of 'analytic interruptus' than that which Lofland (1970) diagnosed (Hammersley 1985, 1987a and b).

There has been some ethnographic work that has grappled explicitly with the problems of testing theories. The procedural model usually adopted here is that of analytic induction. This involves the following steps:

- 1 An initial definition of the phenomenon to be explained is formulated (for example, addiction to opiate drugs, embezzlement, etc.).
- 2 Some cases of this phenomenon are investigated, documenting potential explanatory features.
- 3 A hypothetical explanation is framed on the basis of analysis of the data, designed to identify common factors across the cases.
- 4 Further cases are investigated to test the hypothesis.
- 5 If the hypothesis does not fit the facts from these new cases, either the hypothesis is reformulated or the phenomenon to be explained is redefined (so that the negative cases are excluded).
- 6 This procedure of examining cases, reformulating the hypothesis, and/or redefining the phenomenon is continued until new cases continually confirm the validity of the hypothesis, at which point it may be concluded that the hypothesis is correct (though this can never be known with absolute certainty).

This procedure is represented in Figure 3.

There are relatively few accounts of this method in use. Cressey's (1950) work on 'trust violation' provides a good example, as does that of Lindesmith (1947) on drug addiction. Analytic induction was originally developed by Znaniecki (1934) in explicit opposition to the statistical method. He claimed that it was the true method of the physical and biological sciences, and asserted its superiority on the grounds that it produces universal not probabilistic statements. However, Znaniecki's argument is not convincing. As Robinson (1969) has pointed

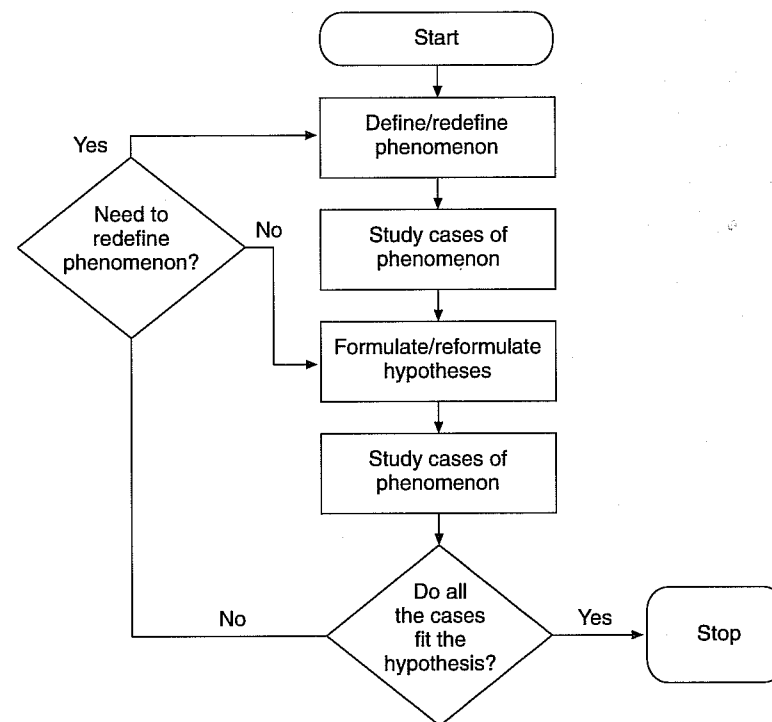


Figure 3 The process of analytic induction

Source: Hammersley 1989b:170

out, he drew too sharp a distinction between analytic induction and statistical method; and in fact the capacity of analytic induction to produce universal statements derives from being concerned only with necessary, and not with sufficient, conditions.

Besides the inclusion of sufficient as well as necessary conditions, there is another element we might add to analytic induction. The geneticist William Bateson is reported to have advised

his students: 'Treasure your exceptions!' He argues that they are 'like the rough brickwork of a growing building which tells that there is more to come and shows where the next construction is to be' (quoted in Lipset 1980:54). Both Cressey and Lindesmith do this, but they do not seem actively to have searched for exceptions, a strategy rightly recommended by Popper (1972). While no number of confirming instances can ever guarantee the validity of a theory, we can increase the chances of our acceptance of it being well founded if we adopt this strategy.

Analytic induction, developed to cover both necessary and sufficient conditions, and to include the search for negative evidence, seems a plausible reconstruction of the logic of theoretical science, not just of ethnography concerned with the production of theory. In this sense Znaniecki was almost certainly correct in the claims he made for it. In many respects it corresponds to the hypothetico-deductive method. Where it differs from this, and most importantly, is in making clear that the testing of theoretical ideas is not the end point of the process of scientific inquiry but is generally only one step leading to further development and refinement of the theory. (Some accounts of the hypothetico-deductive method recognize this; see, for example, Hempel 1966.)

At the same time, however, we need to recognize what is presupposed by analytic induction. It assumes that social phenomena are governed by deterministic, albeit conditional, laws; such that if conditions X, Y, and Z occur, then event A will be produced in all circumstances. There are objections to this from several directions; and among ethnographers in particular the concept of deterministic laws is often rejected on the grounds that it denies the manifest capacity of people to make decisions about how to act. As we saw in Chapter 1, this is a key element of naturalism. In one of the most influential discussions of this issue, Matza (1969) argues that while people can behave in a manner that is predictable by laws, human life proper involves a transcendence of determining conditions. (For a discussion of the history and current status of analytic induction in the light of these problems, see Hammersley 1989b.)

TYPES OF THEORY

We have emphasized that by no means all ethnographic work is, or need be, concerned explicitly with the refinement and

testing of theories. Equally, we should note the range of different types of theory with which ethnographers may be concerned. For example, in sociology there is a well-established, though by no means always clearly expressed, distinction between macro and micro levels of analysis.

'Macro' refers to theories that apply to large-scale systems of social relations. This may involve tracing linkages across the structure of a national society or even relations among different nation-states. Micro research, by contrast, is concerned with analysing more local forms of social organization, whether particular institutions or particular types of face-to-face encounter. What we have here, then, is a dimension along which the scale of the phenomena under study varies.

While in many respects ethnography is better suited to research on micro theory, it can play an important role in developing and testing macro theories (see, for example, Willis 1977 and 1981). Macro theories necessarily make claims about processes occurring in particular places and times that can be tested and developed through ethnographic inquiry. There have also been attempts to integrate macro and micro levels in various ways or to show that there is in fact only one level, not two. (See Knorr-Cetina and Cicourel 1981; also Hammersley 1984b.)

Cross-cutting the macro-micro dimension is the distinction that Glaser and Strauss (1967) make between substantive and formal theory. While macro-micro relates to variation in the scope of the cases under study, the substantive-formal dimension concerns the generality of the categories under which cases are subsumed. Formal categories subsume substantive categories. Thus, for example, the substantive study of taxi-drivers and their 'fares' can be placed under more formal categories such as 'service encounters' or 'fleeting relationships' (Davis 1959). Similarly, the study of a particular society can be used as an initial basis for theory about a general type of social formation; thus, Britain may be taken as an instance of capitalist, industrial, or even postmodern society.

Given these two dimensions, we can identify four broad types of theory, and, indeed, examples of all of these can be found in the work of ethnographers. Analyses of the structure, functioning, and development of societies in general, such as those of Radcliffe-Brown (1948b) and Harris (1979), are macro-formal.

Studies of particular societies, for instance Malinowski (1922) or Chagnon (1968), fall into the macro-substantive category. Micro-formal work consists of studies of more local forms of social organization. Examples would be Goffman on the 'presentation of self' (1959) and 'interaction ritual' (1972); Glaser and Strauss (1971) on 'status passage'; and Sacks on the organization of conversation (Sacks *et al.* 1974). Finally, there is micro-substantive research on particular types of organization or situation: for instance, Strong (1979) on 'doctor-patient interaction' or Piliavin and Briar (1964) on 'police encounters with juveniles'. All these types of theory are worthwhile, but it is important to keep clearly in mind the kind of theory one is dealing with, since each would require the research to be pursued in a different direction. (For a discussion of the development of formal as opposed to substantive theory, see Glaser and Strauss 1967; Glaser 1978.)

CONCLUSION

In this chapter we have looked at the process of analysis in ethnography, tracing it from foreshadowed problems and the initial examination of a body of data, through the generation of concepts of various kinds, to the development of typologies and theories. In addition, we examined the relationship between concepts and indicators in ethnographic research, and the testing of theoretical ideas by means of the comparative method. We stressed that there are different sorts of theory and that theories are not the only product of ethnographic work; equally common and important are descriptions and explanations. We must not forget, however, that typically all the various products of ethnographic work, whatever their other differences, take the form of texts: ethnographic analysis is not just a cognitive activity but a form of writing. This has some important implications, as we shall see in the next chapter.

Chapter 9

Writing ethnography

THE DISCIPLINES OF READING AND WRITING

One cannot ignore the work of reading and writing in the construction of ethnographic research. It is now widely recognized that 'the ethnography' is produced as much by how we write as by the processes of data collection and analysis; equally, how we write is linked directly to how we read.

The writing of ethnography – like any writing – demands discipline and work. There is no more damaging myth than the idea that there is a mysterious 'gift', or that writing is a matter of 'inspiration'. As Brodkey (1987) has pointed out, there is a pervasive romantic image of the writer as an essentially solitary figure struggling with a recalcitrant muse. Such views are dangerous and misleading. They inhibit systematic reflection on writing (and reading) as necessary aspects of the disciplinary or craft skills of social scientists. Given the reflexivity of social inquiry, it is vital to recognize that ethnographers construct the accounts of the social world to be found in ethnographic texts, rather than those accounts simply mirroring reality. And those accounts are constructed on the basis of particular purposes and presuppositions. Equally, one must recognize the significance of how those texts are read by social scientists, students, and others.

As more and more scholars have come to realize, then ethnography is inescapably a textual enterprise. It is not just a matter of writing, of course. When Clifford Geertz announces that 'ethnographers write' he offers a revealing half-truth: ethnographers do more than that. But writing is at the heart of the ethnographic enterprise. It is, therefore, important that a