
ETHNOGRAPHY
Principles in Practice

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only important methodological principles in social research, nor do they privilege ethnography over other methods in any absolute way. While it is tempting to react against the tendency of some methodologists to treat ethnography as outside science by constructing an 'alternative paradigm', which places it at the centre and banishes these other methods to the margins, this is a misguided response. It claims too much, while yet conceding ground to those who seek to identify science exclusively with experimentation and quantification. Methods must be selected according to purposes; general claims about the superiority of one technique over another have little force.

In writing this book we have sought to steer a course between an abstract, methodological treatise and a practical 'cookbook'. This is because for us methodology and method, like social theory and empirical research, feed into one another. Neither can be discussed effectively in isolation. In the opening chapter we outline some of the different methodological frameworks through which ethnography has been viewed, and spell out the implications of what we take to be the most important feature of social research: its reflexivity, the fact that it is part of the social world it studies. Subsequent chapters focus on more concrete aspects of ethnographic research, advocating and illustrating the reflexive point of view.

In writing this book we have had two rather different audiences in mind. On the one hand, there are practitioners of ethnography, of whatever degree of experience, student and professional. It is to them that our argument about the fruitfulness of thinking about ethnography in terms of reflexivity, rather than the more common framework of naturalism, has been primarily directed. At the same time, we have tried to write a book that is accessible to those with little or no knowledge of ethnographic techniques, though such readers might be well advised to begin at Chapter 2 and read the first chapter later (for example after Chapter 9). In this way the methodological questions tackled in Chapter 1 can be placed in their proper context.

What is ethnography?

Over the last few years there has been a growth of interest in ethnography among researchers in many different fields, both theoretical and practical. This stems largely from a disillusionment with the quantitative methods that have for long held the dominant position in most of the social sciences. However, it is in the nature of opposition movements that their cohesion is more negative than positive: everyone agrees, more or less, on what must be opposed, but there is less agreement on the nature of the alternative. Thus, across the numerous fields in which ethnography, or something very like it, has come to be proposed, one finds considerable diversity in prescription and practice. There is disagreement as to whether ethnography's distinctive feature is the elicitation of cultural knowledge (Spradley 1980), the detailed investigation of patterns of social interaction (Gumperz 1981), or holistic analysis of societies (Lutz 1981). Sometimes ethnography is portrayed as essentially descriptive, or perhaps as a form of story-telling (Walker 1981); occasionally, by contrast, great emphasis is laid on the development and testing of theory (Glaser and Strauss 1967; Denzin 1978).

As will become clear later, for us ethnography (or participant observation, a cognate term) is simply one social research method, albeit a somewhat unusual one, drawing as it does on a wide range of sources of information. The ethnographer participates, overtly or covertly, in people's daily lives for an extended period of time, watching what happens, listening to what is said, asking questions; in fact collecting whatever data are available to throw light on the issues with which he or she is concerned.

→ In many respects ethnography is the most basic form of social research. Not only does it have a very long history (Wax 1971), but it also bears a close resemblance to the routine ways in which people make sense of the world in everyday life. Some commentators regard this as its basic strength, others see it as a fundamental weakness. Ethnography has sometimes been dismissed as quite inappropriate to social science, on the grounds that the data and findings it produces are 'subjective', mere idiosyncratic impressions that cannot provide a solid foundation for rigorous scientific analysis. Others argue that only through ethnography can the meanings that give form and content to social processes be understood. 'Artificial' methods such as experiments and survey interviews are rejected on the grounds that these are incapable of capturing the meaning of everyday human activities. Indeed, the very notion of a science of social life explaining human behaviour in causal terms may be rejected.

All social researchers feel the tension between conceptions of science modelled on the practices of natural science on the one hand, and ideas about the distinctiveness of the social world and the implications of this for how it should be studied on the other. Often this tension is presented as a choice between two conflicting paradigms (Wilson 1971; Johnson 1975; Schwartz and Jacobs 1979). While the names given to these paradigms often differ, there is considerable overlap in content among the various accounts. Following much precedent we shall call these paradigms 'positivism' and 'naturalism'; the former privileging quantitative methods, the latter promoting ethnography as the central, if not the only legitimate, social research method.

In our view, statements about paradigms are best viewed as attempts to reconstruct the logic-in-use (Kaplan 1964) of social

research. From this point of view, and especially as regards ethnography, neither positivism nor naturalism is entirely satisfactory. Indeed, in our view they share a fundamental misconception: they both maintain a sharp distinction between social science and its object. We shall try to show that once one recognizes the reflexive character of social research, that it is part of the world it studies, many of the issues thrown up by the dispute over positivism become easier to resolve, and the specific contribution to be made by ethnography emerges more clearly.

Positivism and naturalism

We shall begin by examining positivism and naturalism and their implications for ethnography. It should perhaps be pointed out, however, that while the ideas we group together under these headings have a certain affinity, we do not pretend that social scientists can be divided straightforwardly into two groups on this basis. Indeed, even those whose work we cite as exemplifying one or another feature of the two perspectives by no means always adhere to the perspective *in toto*. Rather than produce straightforward descriptions of the methodological views of particular groups of social scientists, we have sought to capture two influential trends in thinking about the nature of social science in general, and of ethnography in particular. We shall use these throughout the book as benchmarks to fix our own position.

Positivism has a long history in philosophy, but it reached its apogee in the 'logical positivism' of the 1930s and 1940s (Kolakowski 1972). This movement had a considerable influence upon social scientists, notably in promoting the status of experimental and survey research and the quantitative forms of analysis associated with them. Where earlier, in both sociology and social psychology, qualitative and quantitative techniques had generally been used side by side, often by the same researcher, there was now a tendency for distinct methodological traditions to be formed and for those legitimated by positivism to become dominant. In these disciplines the distinction between qualitative and quantitative methods gradually metamorphosed into an epistemological chasm.

Today, the term 'positivism' is used in a confusing variety of ways. Indeed, in the last ten years, among social scientists, it has become little more than a term of abuse. For present purposes the major tenets of positivism can be outlined as follows (for more detailed discussions see Keat and Urry 1975; Giddens 1979; and Cohen 1980):

1. *Physical science, conceived in terms of the logic of the experiment, is the model for social research.* While positivists do not claim that all the methods of the physical sciences are the same, they do argue that these share a common logic. This is the logic of the experiment where quantitatively measured variables are manipulated in order to identify the relationships among them. This logic, it is argued, is the defining feature of science.
2. *Universal laws.* Positivism has come to adopt a characteristic conception of explanation, usually termed the 'covering law' model. Here events are explained in deductive fashion by appeal to universal laws that posit regular relationships between variables held to obtain across all circumstances. However, it is the statistical version of this model, whereby the relationships have only a high probability of applying across all circumstances, that has generally been adopted by social scientists and this has encouraged great concern with sampling procedures, especially in survey research. Given this model, a premium is placed on the generalizability of findings.
3. *Neutral observation language.* Finally, epistemological and/or ontological priority is given to phenomena that are directly observable; any appeal to intangibles runs the risk of being dismissed as metaphysical nonsense. Scientific theories must be founded upon - tested by appeal to - descriptions that simply correspond to the state of the world, involving no theoretical assumptions and thus being beyond doubt. This foundation could be sense data, as in traditional empiricism, or as with later versions, the realm of the 'publicly observable': the movement of physical objects, such as mercury in a thermometer, which can be easily agreed upon by all observers. Because observation in the social world is rarely as straightforward as reading a thermometer, this concern with a theoretically neutral

observation language has led to great emphasis being given to the standardization of procedures of observation. This is intended to facilitate the achievement of measurements that are stable across observers. If measurement is reliable in this sense, it is argued, it provides a sound, theoretically neutral base upon which to build.

Central to positivism, then, is a certain conception of scientific method, modelled on the natural sciences, and in particular on physics (Toulmin 1972). Method here is concerned with the testing of theories. A sharp distinction is drawn between the context of discovery and the context of justification (Reichenbach 1938 and 1951). It is the procedures employed in the latter that are held to mark science off from commonsense, the aim being to replace the latter with a body of scientific knowledge.

The most important feature of scientific theories is that they are open to, and subjected to, test: they can be confirmed, or at least falsified. This process of testing involves comparing what the theory says should occur under certain circumstances with what actually does occur; in short comparing it with 'the facts' (Goode and Hatt 1952). These facts are collected by means of methods that, like the facts they collect, are regarded as theory-neutral; otherwise, it is assumed, they could not provide a test of the theory. In particular, every attempt is made to eliminate the effects of the observer by developing an explicit, standardized set of experimental or interview procedures. This allows replication by others so that an assessment of the reliability of the findings can be made (Moser and Kalton 1971). In survey research, for example, the behaviour of interviewers is specified down to the wording of questions and the order in which they are asked. In experiments the behaviour of the experimenter and the instructions he or she gives to subjects are closely specified. If it can be ensured that each experimental subject or survey respondent in a study and its replications is faced with the same set of stimuli, then, it is argued, their responses will be commensurable. Where such explicit and standardized procedures are not employed, as in participant observation, then, so the argument goes, it is impossible to know how to interpret the responses since one has no idea what they are responses to. Such research, it is claimed, can do no

more than speculate about causal relationships since no basis for testing hypotheses is available.

In reaction against mounting positivist criticism over the last forty years, ethnographers have developed an alternative view of the proper nature of social research, often termed 'naturalism' (Blumer 1969; Lofland 1967; Matza 1969; Denzin 1971; Schatzman and Strauss 1973; Guba 1978; but see also Williams 1976).

Naturalism proposes that, as far as possible, the social world should be studied in its 'natural' state, undisturbed by the researcher. Hence, 'natural', not 'artificial' settings like experiments or formal interviews, should be the primary source of data. Furthermore, the research must be carried out in ways that are sensitive to the nature of the setting. A key element of naturalism is the demand that the social researcher adopt an attitude of 'respect' or 'appreciation' toward the social world. In Matza's words, naturalism is 'the philosophical view that remains true to the nature of the phenomenon under study' (1964:5). This is counterposed to the positivists' primary and prior commitment to a conception of scientific method reconstructed from the experience of natural scientists:

'Reality exists in the empirical world and not in the methods used to study that world; it is to be discovered in the examination of that world. . . . Methods are mere instruments designed to identify and analyze the obdurate character of the empirical world, and as such their value exists only in their suitability in enabling this task to be done. In this fundamental sense the procedures employed in each part of the act of scientific inquiry should and must be assessed in terms of whether they respect the nature of the empirical world under study - whether what they signify or imply to be the nature of the empirical world is actually the case.'

(Blumer 1969:27-8)

A first requirement of social research according to this view, then, is fidelity to the phenomena under study, not to any particular set of methodological principles, however strongly supported by philosophical arguments. Moreover, social phenomena are regarded as quite distinct in character from

natural phenomena. Here naturalism draws on a wide range of philosophical and sociological ideas: symbolic interactionism, phenomenology, hermeneutics, linguistic philosophy, and ethnomethodology. From very different starting points these various traditions argue that the social world cannot be understood in terms of causal relationships or by the subsumption of social events under universal laws. This is because human actions are based upon, or infused by, social meanings: intentions, motives, attitudes, and beliefs. Thus, for example, at the heart of symbolic interactionism is a rejection of the stimulus-response model of human behaviour which is built into the methodological arguments of positivism. In the view of interactionists, people interpret stimuli, and these interpretations, continually under revision as events unfold, shape their actions. The same physical stimulus can mean different things to different people and, indeed, to the same person at different times.

On this argument, using standardized methods in no way ensures the commensurability of the data produced. In fact quite the reverse occurs. Interpretations of the same set of experimental instructions or interview questions will undoubtedly vary among people and across occasions. According to naturalism, in order to understand people's behaviour we must use an approach that gives us access to the meanings that guide that behaviour. Fortunately, the capacities we have developed as social actors can give us such access. As participant observers we can learn the culture or subculture of the people we are studying. We can come to interpret the world in the same way as they do.

The need to learn the culture of those we are studying is most obvious in the case of societies other than our own. Here, not only may we not know why people do what they do, often we do not even know what they are doing. We are in much the same position as (Schutz's) (1964) stranger. Schutz notes that in the weeks and months following the immigrant's arrival in the host society, what he or she previously took for granted as knowledge about that society turns out to be unreliable if not obviously false. In addition, areas of ignorance previously of no importance come to take on great significance, overcoming them being necessary for the pursuit of important goals, perhaps even for the

stranger's very survival in the new environment. In the process of learning how to participate in social situations in the new society, the stranger gradually acquires an inside knowledge of it, which supplants his or her previous 'external' knowledge. Schutz argues that by virtue of being forced to come to understand the culture of the host society in this way, the stranger acquires a certain objectivity not available to culture members. The latter live inside the culture, quite unable to see it as anything but a reflection of 'how the world is'. They are not conscious of the fundamental assumptions, many of which are distinctive to that culture, that shape their vision.

As Schutz points out, the experience of the stranger is not restricted to those moving to live in a different society. Movement among groups within a single society can produce the same effects, though generally in a milder form. According to the naturalist account, the value of ethnography as a social research method is founded upon the existence of such variations in cultural patterns across and within societies, and their significance for understanding social processes. Ethnography exploits the capacity that any social actor possesses for learning new cultures, and the objectivity to which this process gives rise. Even where he or she is researching a familiar group or setting, the participant observer is required to treat it as 'anthropologically strange' in an effort to make explicit the assumptions he or she takes for granted as a culture member. In this way the culture is turned into an object available for study. Through marginality, in perspective and perhaps also in social position, naturalism proposes that it is possible to construct an account of the culture under investigation that captures it as external to, and independent of, the researcher; in other words, as a natural phenomenon. In fact, the description of cultures becomes the primary goal. The search for universal laws is rejected in favour of detailed descriptions of the concrete experience of life within a particular culture and of the social rules or patterns that constitute it. Attempts to go beyond this, to explain particular cultural forms, are discouraged. As Denzin (1971:168) notes 'the naturalist resists schemes or models which over-simplify the complexity of everyday life'.

To understand the world
 we must first understand
 the culture we are in

In short, then, naturalism presents ethnography as the pre-eminent if not exclusive social research method. This is because any account of human behaviour requires that we understand the social meanings that inform it. People interpret stimuli in terms of such meanings, they do not respond merely to the physical environment. Such understanding requires that we learn the culture of those we are studying. This cannot be done by following standardized procedures; it is a natural process analogous to the experience of any stranger learning the culture of a group. The task becomes cultural description; anything more is rejected as imposing the researcher's own arbitrary and simplistic categories on a complex reality. The centrality of meaning also has the consequence that people's behaviour can only be understood in context. For this reason 'natural' settings must be investigated: we cannot understand the social world by studying artificial simulations of it in experiments or interviews. To restrict the investigation of social behaviour to such settings is to discover only how people behave in experimental and interview situations.

Problems with naturalism

The origins of the contrasting positions on the nature of social research we have outlined can be traced back as far as differences in view between Plato and Aristotle (von Wright 1971; Levy 1981). However, it is only in the last fifty years that these ideas have generated distinct research traditions within some social science disciplines. Nineteenth-century investigators, such as Mayhew (1861), LePlay (1879), and Booth (1902-3), treated quantitative and qualitative techniques as complementary. Even the sociologists of the Chicago School, often represented as thoroughly interactionist in outlook and arch-exponents of participant observation, employed both 'case-study' and 'statistical' methods. While there were recurrent debates among them regarding the relative advantages and uses of the two sorts of technique, there was general agreement on the value of both (Harvey 1982). It was only later, with the rapid development of statistical methods and the growing influence of positivist philosophy, that survey research came to be regarded by some of its practitioners as a self-sufficient methodological tradition. (In

social psychology the process started rather earlier and the experiment became even more dominant.)

Within sociology, naturalism emerged as a reaction against the development of the survey research tradition, the construction of an alternative paradigm designed to protect ethnography and other qualitative techniques from the positivist critique. The pioneer in the 1940s and 1950s was Herbert Blumer (Blumer 1969), while in the 1960s the trend was strengthened by the renaissance of interpretive sociology. Even in anthropology, where ethnography has always been the staple research method, a similar, if milder, tendency to the establishment of distinct research traditions can be found (Pelto and Pelto 1978). In social psychology it is only relatively recently that the dominance of the experiment has been seriously threatened (Harré and Secord 1972; Cronbach 1975; Rosnow 1981).

Undoubtedly, many of the naturalists' criticisms of positivism are well-founded. Indeed, the force of some of them has been recognized by experimentalists and survey researchers themselves. The serious problems involved in drawing inferences from responses under experimental conditions, or from what is said in interviews, to what people do in everyday life, have come to be listed under the heading of 'ecological validity' (Brunswik 1956; Bracht and Glass 1968). In most of the physical sciences the generalizability of findings across time and space presents few problems. Chemical substances, for example, do not usually behave differently inside and outside the walls of laboratories. However, this seems to be a serious problem in the study of human behaviour. As a little reflection on everyday life makes clear, people do behave, and are expected to behave, differently according to context (Deutscher 1973).

One aspect of the problem of ecological validity - the effects of researchers and the procedures they use on the responses of the people studied - has been subjected to considerable investigation (Orne 1962; Rosenthal 1966; Hyman 1954; Sudman 1974; Schuman 1982). Similarly, recognition of the difficulties involved in interpreting the meaning of people's responses has led to calls for the extension of pilot work of a broadly ethnographic kind in surveys and of 'unstructured' debriefing interviews on experiments. There have even been

being met
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calls for participant observation to be used as a supplement or complement to these methods (Crowle 1976).

Of course, those working in the experimental and survey traditions do not usually draw the same conclusions from naturalist criticisms as would the naturalist. They are understandably reluctant to abandon experiments and surveys in favour of exclusive reliance on ethnography. Even less are they inclined to accept naturalism's rejection of causal explanation, and in our view they are quite correct in this. While many of them have yet to realize the full implications of what is valid in naturalism, they are certainly wise not to embrace it *in toto*.

Naturalists are right to point to the dangers of drawing inferences from what people say and do in research settings to what they do in everyday settings, but the problem of ecological validity is more subtle than they suppose. Not only are 'artificial' settings by no means *necessarily* ecologically unrepresentative in relevant respects, but also the results of research carried out in 'natural' settings may be ecologically invalid too. Owing to the influence that a participant observer may have on the setting studied, and/or the effects of temporal cycles within the setting (Ball 1983), the conclusions he or she draws from the data are by no means necessarily valid for that setting at other times. For similar reasons, findings produced by participant observation in one setting may not be true for other settings of 'the same type'.

At a deeper level, the very notion of 'natural' and 'artificial' settings is misleading. Even to make this distinction is to take the positivists' rhetoric for reality, to treat them as if they really had succeeded in becoming Martians, viewing society from outside (Davis 1973). 'Artificial' settings set up by researchers are still part of society. Indeed, the real force of the naturalist critique of experiments and survey interviews is precisely that they are social occasions subject to all those processes of symbolic interpretation and social interaction to be found elsewhere in society, and which threaten constantly to undermine positivist attempts to manipulate variables.

This ambivalence on the part of naturalism over the nature of 'artificial' settings is a symptom of a general problem. It reflects a conflict between the account of social research it presents,

bordering on a naive realism, and its view of social actors, derived from symbolic interactionism and other forms of interpretive sociology.

Where positivism stresses hypothesis-testing, and in particular the role of 'crucial experiments', naturalism portrays research as a process of exploration. There is a strong parallel here with the views of some early natural scientists:

'In the early days of science, it was believed that the truth lay all around us . . . was there for the taking . . . waiting, like a crop of corn, only to be harvested and gathered in. The truth would make itself known to us if only we would *observe* nature with that wide-eyed and innocent perceptiveness that mankind is thought to have possessed in those Arcadian days before the Fall . . . before our senses became dulled by prejudice and sin. Thus the truth is there for the taking only if we can part the veil of prejudice and preconception and *observe things as they really are*. . . .'

(Medawar 1979:70)

Rather than importing methods from the physical sciences, naturalism argues, we must adopt an approach that respects the nature of the social world, which allows it to reveal its nature to us. This argument sometimes takes on a political dimension because the objects under investigation in social research are people who have their own views about the world, views that through their actions also shape the character of that world. Interactionist theory notes how some powerful groups are able to impose their 'definitions of reality' on others, and this analysis is applied to social research itself, the conclusion being drawn that science should not assist in the oppression of people in this way. In response to this the research task comes to be defined as understanding the perspectives of social actors, and in particular those of 'underdogs' (Becker 1967; Gouldner 1968).

While forming a useful antidote to positivism's preoccupation with hypothesis-testing, this inductivist methodology is fundamentally misconceived. After all, how can we discover the nature of the social world without employing some method? Indeed, is not discovering the nature of social phenomena precisely the goal of social science? While some methods may be more structured and selective than others, all research, however

exploratory, involves selection and interpretation. Even in a very small-scale setting we could not begin to describe everything, and any description we do produce is inevitably based on inferences. Thus, for example, when setting out to describe a culture, we operate on the basis of the assumption that there are such things as cultures, and have some ideas about what they are like; and we select out for analysis the aspects of what is observed that we judge to be 'cultural'. While there may be nothing wrong with such cultural description, the kind of empiricist methodology enshrined in naturalism renders the theory implicit and thus systematically discourages its development and testing.

One of the most significant assumptions built into naturalism is that all perspectives and cultures are rational. Understanding a culture becomes the first requirement and any attempt to explain it in terms of material interests or ideological distortion is regarded as incompatible with such understanding. Here the quite different issues of intelligibility and validity are confused. Views do not have to be true to be intelligible, though of necessity we do assume in all science that the truth is intelligible. Here, naturalism takes over the common, but erroneous, view that only false beliefs can be explained sociologically, and in this case the outcome is a thoroughgoing relativism. While the usual consequence of relativism – the erosion of any possibility of knowledge – is avoided, the cost is nevertheless very high: social research is limited to cultural description. Anything more would imply that the cultures under study were false, being the product of social causation rather than of cultures members actively constructing reality.

This is a paradoxical conclusion. While culture members freely and legitimately engage in checking claims against facts and frequently employ causal explanations to account for one another's behaviour, the social scientist is debarred from this on the grounds that it would 'distort reality'. Naturalism's escape from relativism is secured, then, by applying quite different theories to the way in which social researchers on the one hand and culture members on the other make sense of the social world. The restriction placed on social research limiting it to cultural description serves to keep these two theories apart, to prevent them from coming into collision.

What we have here, in effect, is the retention of the distinction between science and commonsense that lies at the heart of positivism. While naturalism conceptualizes both science and commonsense in very different ways and reverses their status and power, the distinction nevertheless remains. It is a distinction that has little to recommend it, however. It is the social scientists' equivalent of that between professional and lay knowledge adopted by many occupations. Of course, the question of who is judged a professional and who a layperson is relative to a particular occupation, but the contrast between science and commonsense, like other devices used by professionals to highlight their expertise against a background of lay ignorance, obscures that relativity. Indeed, the distinction between science and commonsense, whether used by positivism or naturalism, tends to imply that science is quite separate from society and that scientists, *qua* scientists, are quite different from other people.

Reflexivity

The distinction between science and common-sense, between the activities of the researcher and those of the researched, lies at the heart of both positivism and naturalism. It is this that leads to their joint obsession with eliminating the effects of the researcher on the data. For one, the solution is the standardization of research procedures; for the other it is direct experience of the social world, in extreme form the requirement that ethnographers 'surrender' themselves to the cultures they wish to study (Wolff 1964; Jules-Rosette 1978). Both positions assume that it is possible, in principle at least, to isolate a body of data uncontaminated by the researcher, either by turning him or her into an automaton or by making him or her a neutral vessel of cultural experience. However, searches for empirical bedrock of this kind are futile; all data involve theoretical assumptions (Hanson 1958).

The first and most important step towards a resolution of the problems raised by positivism and naturalism is to recognize the reflexive character of social research: that is, to recognize that we are part of the social world we study (Gouldner 1970; Borhek and Curtis 1975; and Hammersley 1982b). This is not a matter

of methodological commitment, it is an existential fact. There is no way in which we can escape the social world in order to study it; nor, fortunately, is that necessary. We cannot avoid relying on 'common-sense' knowledge nor, often, can we avoid having an effect on the social phenomena we study. There is, though, as little justification for rejecting all common-sense knowledge out of hand as there is for treating it as all 'valid in its own terms': we have no external, absolutely conclusive standard by which to judge it. Rather, we must work with what knowledge we have, while recognizing that it may be erroneous and subjecting it to systematic inquiry where doubt seems justified. Similarly, instead of treating reactivity merely as a source of bias, we can exploit it. How people respond to the presence of the researcher may be as informative as how they react to other situations.

However distinctive the purposes of social science may be, the methods it employs are merely refinements or developments of those used in everyday life. This is obvious in the case of ethnography, and perhaps also in the historian's use of documents (Barzun and Graff 1970), but it is equally true of other methods. As a 'structured conversation', the interview is by no means unique to social research. While the journalistic interview, the social work interview, the market research interview, and the social science interview each carry distinctive features, they are clearly overlapping varieties of a single interactional format. The same applies, if perhaps less obviously, to the experiment. While few people apart from scientists use laboratory experiments, the general device of experimentation is widespread. As Medawar (1979:69) explains, 'in the original Baconian sense, an experiment is a contrived, as opposed to a natural experience or happening - is a consequence of "trying things out"'. Experiments are questions put to the world: 'what would happen if . . .?' Such experimentation is common in everyday life and the 'genuine' laboratory experiment is simply a refinement of this. Furthermore, experimentation is founded upon the more basic principle of the testing of hypotheses through comparison of cases. It is predictions that are tested, but these need not even relate to future events, let alone to those that are open to manipulation by the researcher. They are predictions only in the sense that they antecede the researcher's knowledge

of their truth (Reilly 1970).

We are arguing, then, that the testing of hypotheses is by no means restricted to science. And, indeed, the role of hypothesis testing has been noted in a variety of areas, including perception (Gregory 1970) and language learning (Chomsky 1968). It even plays a major role in the process that naturalism places at the very heart of social research: understanding the actions of others. In observing people's behaviour we derive hypotheses from our cultural knowledge to describe and explain their actions, and we test these out against further information. Thus, for example, if we know something about school classrooms we can guess that a pupil raising his or her hand may be indicating that he or she is offering to answer a teacher's question, volunteering to do some chore, or owning up to some misdemeanour. To find out which of these applies, or whether some other description is more appropriate, we have to investigate the context in which the action occurs; that is, we have to generate possible meanings from the culture for surrounding or other apparently relevant actions. Having done that, we must then compare the possible meanings for each action and decide which form the most plausible underlying pattern. Thus, to take a simple example, if the teacher has just asked a question, we might conclude that the pupil is offering to provide the answer. If, however, the teacher chooses someone else to answer who successfully provides an answer and yet the original pupil keeps his or her hand up, we might suspect that the original intention had not been to answer the question but that he or she has something else to say. However, it may be that the pupil is dreaming and has not realized the question has been answered, or it may be that he or she thinks the answer provided was incorrect or has something to add to it. These alternative hypotheses can, of course, be tested by further observations and perhaps also by asking the pupil involved.

The moral to be drawn is that all social research takes the form of participant observation: it involves participating in the social world, in whatever role, and reflecting on the products of that participation. Irrespective of the method employed, it is not fundamentally different from other forms of practical everyday activity, though of course it is closer in character to some than to others. As participants in the social world we are still able, at

least in anticipation or retrospect, to observe our activities 'from the outside' as objects in the world. Indeed, it is this capacity that allows us to co-ordinate our actions. While there are differences in purpose and perhaps also in refinement of method, science does not employ cognitive equipment of an essentially different kind from that available to non-scientists.

The fact of reflexivity has some important methodological implications, it seems to us. For one thing, it makes implausible attempts to found social research upon epistemological foundations independent of common-sense knowledge. As Rescher (1978:20) notes, the search for 'absolutely certain, indefeasible, crystalline truths, totally beyond the possibility of invalidation . . . represents one of the great quixotic quests of modern philosophy'. This is a view that corresponds closely to the 'critical commonsensism' of Peirce (Reilly 1970; Almeder 1980).

The same argument counts against efforts to set up alternative social research paradigms founded upon contrasting epistemological or ontological assumptions. It leads us to view social science as sharing much in common with natural science while yet treating both as merely the advance guard of common-sense knowledge. If paradigms play an important role in science, their character is almost certainly less all-encompassing than Kuhn and some of those who have taken up his ideas often suggest (Keat and Urry 1975). Moreover, differences in view about the nature of social research are merely reconstructions of its logic; they are hypotheses subject to evaluation against the evidence currently available and against further evidence that will become available in the future. As hypotheses, they must not be treated as matters of ultimate commitment even if, for the purposes of practical scientific work, they are taken as true until further notice.

Reflexivity has implications for the practice of social research too. Rather than engaging in futile attempts to eliminate the effects of the researcher, we should set about understanding them; a point that Schuman has recently made in relation to social surveys.

'The basic position I will take is simple: artifacts are in the mind of the beholder. Barring one or two exceptions, the

problems that occur in surveys are opportunities for understanding once we take them seriously as facts of life. Let us distinguish here between the simple survey and the scientific survey. . . . The simple approach to survey research takes responses literally, ignores interviewers as sources of influence, and treats sampling as unproblematic. A person who proceeds in this way is quite likely to trip and fall right on his artifact. The scientific survey, on the other hand, treats survey research as a search for meaning, and ambiguities of language and of interviewing, discrepancies between attitudes and behaviour, even problems of non-response, provide an important part of the data, rather than being ignored or simply regarded as obstacles to efficient research.'

(Schuman 1982:23)

In short, 'what is an artifact if treated naively reflects a fact of life if taken seriously' (1982:24). In order to understand the effects of the research and of research procedures, we need to compare data in which the level and direction of reactivity varies. Once we abandon the idea that the social character of research can be standardized out or avoided by becoming a 'fly on the wall' or a 'full participant', the role of the researcher as active participant in the research process becomes clear. He or she is the research instrument *par excellence*. The fact that behaviour and attitudes are often not stable across contexts and that the researcher may play an important part in shaping the context becomes central to the analysis. Indeed, it is exploited for all it is worth. Data are not taken at face value, but treated as a field of inferences in which hypothetical patterns can be identified and their validity tested out. Different research strategies are explored and their effects compared with a view to drawing theoretical conclusions. What is involved here is the adoption of an experimentalist mentality in the general sense outlined earlier. Theories are made explicit and full advantage taken of any opportunities to test their limits and to assess alternatives. Such a view contrasts sharply with the image of social research projected by naturalism, though it is much closer to other models of ethnographic research such as 'grounded theorizing' (Glaser and Strauss 1967), 'analytic induction' (Cressey 1950; Denzin 1978), and the strategy model

to be found alongside naturalism in the work of Schatzman and Strauss (1973).

The third and final conclusion we wish to draw from reflexivity is that the theories we develop to explain the behaviour of the people we study should also, where relevant, be applied to our own activities as researchers and should aid the development of research strategies. The first step required for this – the collection of data about the research process – has been under way since Whyte added his methodological appendix to *Streetcorner Society* in 1955 (Whyte 1981). Recently, though, this trend has become stronger, and a large number of research biographies have appeared in the last ten or twelve years. The subsequent stage of applying existing theories or developing new ones to make sense of this data has, however, hardly begun in a systematic way (though see, for example, Berreman 1962 and Martin 1981). We shall try to illustrate the potentialities of this at various points throughout this book.

Having argued that social research shares much more in common with other kinds of social activity than is customarily assumed, it is clearly also important for us to say where we think its distinctiveness lies. We must ask what the purpose of social research is, or, as Garfinkel (1981:vii) puts it, 'if social science is the answer, what is the question?'

We have seen that positivism and naturalism address this issue in very different ways. For positivism it is the discovery of universal laws, or at least explanations of particular phenomena framed in terms of universal laws, that is the goal. For naturalism, on the other hand, the only legitimate task is cultural description.

In our view the development and testing of theory is the distinctive function of social theory; it is this that marks it off from journalism and literature, even though it shares much in common with these other pursuits (Strong 1982). Moreover, the idea of relationships between variables that, given certain conditions, hold across all circumstances seems essential to the very idea of theory (Willer 1967). Quantification, as an aid to precision, goes along with this too; though this is not to excuse the indiscriminate quantification that positivism has sometimes encouraged.

Nevertheless, in several important respects the positivist

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model is misleading and naturalism ^{instructive}. For one thing, we would want to insist that the mere establishment of a relationship among variables, while providing a basis for prediction, does not constitute a theory. A theory must include reference to mechanisms or processes by which the relationship among the variables identified is generated. Moreover, such reference must be more than mere speculation, the existence and operation of these ^{such processes} 'intervening variables' must be described (Keat and Urry 1975). Equally, while formalized theories are the goal, we must not allow this to blind us to the value of more informal theories, or to regard theorizing as restricted to social scientists. As we noted earlier, there is no clear-cut distinction to be drawn between theory and fact, nor is common-sense knowledge limited to one end of the theoretical-empirical continuum (Kaplan 1964).

However, it is positivism's conception of the research process, of how the goal of theory is to be achieved, that is most deficient. Reliance is placed upon the hypothetico-deductive method in which, as we saw, all the emphasis is given to the testing of theory. Indeed, where theory comes from, or how it is developed, are regarded as unimportant. What is required is that its truth or falsity be ascertained in the most rigorous manner possible.

There has been some disagreement over whether theories can be proved valid, and in fact it seems clear that they cannot: there is always the possibility that new facts will appear in the future that will disprove them. The most elegant attempt to resolve this problem is the work of Karl Popper who argues that while theories can never be proven true, they can be falsified since only one contradictory example is required for this (Popper 1972; Magee 1972). On this view the defining feature of science is the attempt to falsify theories. Science proceeds, according to Popper, through the progressive elimination of error.

However, this view makes it very difficult to understand how natural science has been so successful in furthering our understanding of the physical world. As Rescher points out, the idea that the elimination of falsehood results in scientific progress only holds if we assume that there are a limited number of hypotheses to test:

'Once we grant (as Popper time and again insists) that any hypotheses we may actually entertain are but a few fish drawn from an infinite ocean - are only isolated instances of those infinitely many available hypotheses we have not even entertained, none of which are *prima facie* less meritorious than those we have - then the whole idea of seeking truth by elimination of error becomes pointless. If infinitely many distinct roads issue from the present spot, there is no reason to think that, by eliminating one or two (or *n*) of these, we come one jot closer to finding the one that leads to the desired destination.'

(Rescher 1978:53-5)

Rescher's argument suggests that we cannot afford to ignore the context of discovery, and indeed he goes on to suggest that there are heuristic procedures available for developing theory.

However, the problem is not simply that an infinite number of hypotheses are available for testing. It seems likely that sociologists *do* make judgements as to the plausibility of particular hypotheses, but we cannot assume, as Peirce (see Rescher 1978) did in the case of the natural sciences, that these will be based on well-founded intuition. Much anthropological and sociological research has been concerned with the way in which beliefs are structured by social processes. Particular emphasis has been placed upon how different groups develop divergent perspectives on the same phenomena and stereotypes of one another. And, of course, if social research is itself part of the social world, we cannot assume that social scientists escape such processes unscathed. Now, there is no implication here that beliefs that are socially produced are necessarily wrong, but the sociology of knowledge does show that the origins of our beliefs, and the sources of the sense of certainty we attach to them, may be different to what we imagine. It also suggests that social scientists must take care not to become straitjacketed by the beliefs that are typical of the social circles in which they move.

The hypothetico-deductive method has also led to the idea that every study must be a test of a hypothesis, as Becker notes in his essay, *Life History and the Scientific Mosaic*:

'But perhaps the major reason for the relatively infrequent use

*to maintain a methodological neutrality
is not to maintain a methodological neutrality
"Heuristics" (1978:53-5)*

of [the life history] is that it does not produce the kind of "findings" that sociologists now expect research to produce. As sociology increasingly rigidifies and "professionalizes", more and more emphasis has come to be placed on what we may, for simplicity's sake, call the *single study*. I use the term to refer to research projects that are conceived of as self-sufficient and self-contained, which provide all the evidence one needs to accept or reject the conclusions they proffer, whose findings are to be used as another brick in the growing wall of science - a metaphor quite different than that of the mosaic.'

(Becker 1970:72)

What positivism neglects, then, is the process by which theory is generated and developed, a point made forcefully by Glaser and Strauss (1967) in their attack on 'verificationism'. They demonstrate the importance of the development of theory and the role of systematic comparison in that process. However, in our view, like naturalism, though to a lesser degree, 'grounded theorizing' represents an over-reaction to positivism. At various points in their argument Glaser and Strauss (1967) seem seriously to underrate the importance of testing, sometimes implying that 'grounded' theory, once developed, is more or less beyond doubt. Of course, they are correct to recognize that the emerging theory is usually subjected to testing, at least of a weak kind, in the process of development. But systematic and rigorous testing of the developed theory is nevertheless important even though it can never be absolutely conclusive.

In a similar way, Glaser and Strauss also tend to over-react in their rejection of the more descriptive forms of ethnography (see particularly Glaser 1978). Like positivists, they tend to overlook the variety of different functions that research can serve. Indeed, they too seem hooked on the single study model. Description of the perspectives of a particular category or group of people, or of patterns of interaction within a particular type of setting can be extremely valuable, not least because it may open up to challenge the preconceptions that social scientists bring to research. This is made more obvious in the case of 'exotic' societies but the argument also applies, in some ways more strongly, to the study of our own societies.

There are many different layers or circles of cultural

knowledge within any society. Indeed, this is particularly true of modern industrial societies with their complex divisions of labour, multifarious life-styles, ethnic diversity, and deviant communities; and the subcultures and perspectives that maintain, and are generated by, these social divisions. This was, of course, one of the major rationales for Chicago School research. Drawing on the analogy of plant and animal ecology, they set out to document the very different patterns of life to be found in different parts of the city of Chicago, from the 'high society' of the so-called Gold Coast to slum ghettos such as Little Sicily. Later, the same kind of approach came to be applied to the cultures of different occupations and deviant groups, as well as even more diffuse 'social worlds' (Strauss 1978) such as those of art (Becker 1974), surfing (Irwin 1973), or racing (Scott 1968). Describing such 'worlds' tests assumptions and creates theory.

Ethnography as method

Once one begins to recognize the complexity of the scientific enterprise, the different functions that research can serve, and the failings of the single study model, one is in a better position to appreciate the contribution that ethnography can make to social science. It should be clear that we do not regard ethnography as an 'alternative paradigm' to experimental, survey, or documentary research. Rather it is simply one method with characteristic advantages and disadvantages, albeit one whose virtues have been seriously underestimated by many social researchers owing to the influence of positivism.

The value of ethnography is perhaps most obvious in relation to the development of theory. Its capacity to depict the activities and perspectives of actors in ways that challenge the dangerously misleading preconceptions that social scientists often bring to research has already been mentioned. Much like Schutz's (1964) stranger, it is difficult for an ethnographer to maintain such preconceptions in the face of extended first-hand contact with the people and settings concerned. Furthermore, while the initial response to such contact may be their replacement by other misconceptions, over time the ethnographer has the opportunity to check out his or her understanding of the

phenomena under study. Equally importantly, though, the depiction of perspectives and activities in a setting allows one to begin to develop theory in a way that provides much more evidence of the plausibility of different lines of analysis than is available to the 'armchair theorist', or even the survey researcher or experimentalist.

Also important here is the flexibility of ethnography. Since it does not entail extensive pre-fieldwork design, as social surveys and experiments generally do, the strategy and even direction of the research can be changed relatively easily, in line with changing assessments of what is required by the process of theory construction. As a result, ideas can be quickly tried out and, if promising, followed up. In this way ethnography allows theory development to be pursued in a highly effective and economical manner.

However, the contribution of ethnography is not limited to the phase of theory development. It can also be used to test theory. For example, cases that are crucial for a theory - those where it seems most likely to be proved false - may be examined through ethnography; though this is not always feasible for macro-social theory where the scale of the object under investigation often necessitates survey research. While the fact that, unlike in the experiment, variables cannot be physically manipulated hampers the evaluation of competing hypotheses, it does not rule it out. As we noted earlier, experimentation is itself founded upon the logic of comparison. Moreover, what is lost in terms of the control of variables may be compensated by reduced risk of ecological invalidity. Since it investigates social processes in everyday settings rather than in those set up for the purposes of research, the danger that the findings will apply only to the research situation is generally lessened. In addition, ethnography's use of multiple data sources is a great advantage here. This avoids the risks that stem from reliance on a single kind of data: the possibility that one's findings are method-dependent. The multi-stranded character of ethnography provides the basis for triangulation in which data of different kinds can be systematically compared (see Chapter 8). In our view this is the most effective manner in which reactivity and other threats to validity can be handled.

A good example of the way in which ethnography can be used

to test theory is provided by the work of Hargreaves (1967), Lacey (1970), and Ball (1981) on pupil orientations to school. They argue that the way in which schools differentiate pupils on academic and behavioural criteria, especially via streaming, tracking, and banding, polarizes them into pro- and anti-school subcultures. These subcultures, in turn, shape pupils' behaviour inside and outside school and affect their levels of academic achievement. This theory is tested in examples of three types of secondary school: secondary modern (Hargreaves), grammar school (Lacey), and comprehensive school (Ball). Moreover, in the case of the grammar school, because the pupils entering the school have been strongly committed to school values at their junior schools, a variable at the heart of competing explanations for the process of polarization - home background - is partially controlled. Similarly, in his study of Beachside Comprehensive, Ball examines the effects of a shift from banding to mixed ability grouping, representing a weakening of differentiation, showing that polarization is also weakened. Taken together these studies give us some confidence that the theory is well founded, though they do not provide absolutely conclusive proof. But then no method is able to do that.

Conclusion

We have examined two contrasting reconstructions of the logic of social research and their implications for ethnography. Neither positivism nor naturalism provides an adequate framework for social research. Both neglect its fundamental reflexivity, the fact that we are part of the social world we study, and that there is no escape from reliance on common-sense knowledge and on common-sense methods of investigation. All social research is founded on the human capacity for participant observation. We act in the social world and yet are able to reflect upon ourselves and our actions as objects in that world. By including our own role within the research focus and systematically exploiting our participation in the world under study as researchers, we can develop and test theory without placing reliance on futile appeals to empiricism, of either positivist or naturalist varieties.

Reconstructing our understanding of social research in line

with the implications of its reflexivity also throws light on the function of ethnography. Certainly there is little justification for the view that it represents an alternative paradigm to quantitative research. On the other hand, it has a much more powerful contribution to make to social science than positivism allows. The remainder of this book is devoted to spelling out the implications of reflexivity for ethnographic practice.

*Research design:
problems, cases,
and samples*

At first blush, the conduct of ethnography is deceptively simple: 'anyone can do it', apparently. Indeed, some authors have reported being given little more research advice than just that before they set out on their fieldwork. Nader, for example, relates how at one time this had become a tradition among North American anthropologists:

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

(Nader 1970:98)

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