(25)── Ethnography ──────────────────

John Brewer

Sociologists understand the term 'organization' in very broad terms to mean any structure by which social life and behaviour are managed. The term is more narrowly understood to mean formal organizations with a bureaucratic structure. Some formal organizations process 'clients', like schools and police stations, where the principal purpose is the management of people and their needs. Others are involved with the management of work, such as factories, where the principal purpose is the work itself. In the first case the people employed within the formal organizations still experience it as work, but this work involves the management of people in non-work settings. In the second, people are still being managed, but it is the employees themselves whose behaviour is being organized in the work setting. The reason for this clarification is to limit the boundaries of this chapter. Ethnography has contributed significantly to research on organizations in both the loose and strict meaning, but this chapter will focus on ethnographies that have been done on work in formal organizations and only on ethnographies done in people-processing organizations where the research focused on how employees experience it as work.

DESCRIPTION OF THE METHOD ──────────────────────────────

Ethnography is a style of research rather than a single method and uses a variety of techniques to collect data. This style of research can be defined as:

> the study of people in naturally occurring settings or 'fields' by means of methods which capture their social meanings and ordinary activities, involving the researcher participating directly in the setting, if not also the activities, in order to collect data in a systematic manner but without meaning being imposed on them externally (Brewer, 2000: 10; for other explications of ethnography see: Atkinson et al., 2001; Burgess, 1984; Davies, 1999; Fetterman, 1998; Hammersley and Atkinson, 1995).

The methods used must therefore permit access to people's social meanings and activities and involve close association and familiarity with the social setting. This does not necessarily mean actual participation in the setting, so ethnography's repertoire of techniques includes in-depth interviews (see King, Chapter 2, this volume), discourse analysis (see Dick, Chapter 7, this volume), personal documents and vignettes (on vignettes see Barter and Renold, 1999) alongside participant observation (see Waddington, Chapter 13, this volume). Visual methods, like video, photography and film (see Pink, 2001) and the Internet (Hine, 2000) are now also joining the list. These methods are also used in non-ethnographic research and what distinguishes their application in ethnography is that they are employed to meet the objectives

that distinguish it as a style of research – the exploration of the social meanings of people in the setting by close involvement in the field. One other feature of these methods when used in ethnographic research is that they are not employed in isolation from each other. Ethnography routinely builds in triangulation of method because it involves the use of multiple methods of data collection.

One further complication is that there is an interpolation of method and methodology in ethnography. As well as presupposing certain methods of data collection, ethnography is closely associated with a particular philosophical framework that validates its practice. This framework is called naturalism (also the humanistic, hermeneutic or interpretative paradigms). Naturalism is an orientation concerned with the study of social life in natural settings as they occur independently of experimental manipulation. It is premised on the view that the central aim of the social sciences is to understand people's actions and their experiences of the world, and the ways in which their motivated actions arise from and reflect back on these experiences. Once this is the central aim, knowledge of the social world is acquired from intimate familiarity with it and in capturing the voices of people who inhabit it, something ethnography is suitably equipped to achieve.

APPLICATIONS OF METHOD TO ORGANIZATIONAL RESEARCH

Ethnographies of work in organizations have a central place in the genre (see Smith, 2001 for listings of this work). It is useful to order this research into three categories: a focus on occupational careers and identities as mechanisms by which organizations maintain themselves; managerial control in organizations; and practical reasoning in bureaucratic and formal organizational settings. In first establishing ethnography in sociology, the Chicago School used it to illustrate the processes by which social life reproduced itself (on the School's use of ethnography see Deegan, 2001) and their preoccupation with work derived from an interest to show how specific social institutions maintained themselves through workers' careers and identities (on which see Barley, 1989). 'Natural histories' of various occupations were undertaken by means of ethnographic research, often with a focus on the unusual occupations found on the margins of urban industrial society. This trait has survived into the contemporary period where the intent remains to capture the experience of workers in organizations whose perspective and identity result in the maintenance of the particular social institution. Everett Hughes gave a name to this focus when he called it 'dirty work' (1964; also see Hughes, 1958) and ethnographers have toiled as nightclub hostesses (Allison, 1994), train locomotive repairers (Gamst, 1980), police officers (Brewer, 1991; Holdaway, 1983), prison warders (Jacobs and Retsky, 1975), lorry drivers (Hollowell, 1968), assembly line workers (Chinoy, 1955), machine operators (Burawoy, 1979), massage parlour trainees (Chapkis, 1997), and many more besides.

This kind of research often only incidentally addresses the organizational setting within which the work takes place, but this focus is the main attention of ethnographies that address control within organizations. The well-known Hawthorne studies in the 1920s established a tradition of ethnographic research that blended with developments in human relations management theory to focus on informal social interaction in the workplace. The research pointed to the existence of an informal organization alongside the formal one and showed how the pace of work and job satisfaction are regulated by informal sets of norms and rules (classic studies include Roy, 1952, 1953, 1954). Besides the obvious impact in revising our

understanding of bureaucracy (for classic studies on which see Blau, 1955; Gouldner, 1954; Jacobs, 1969), this ethnographic research sensitized us to the role of informal social organization in coping with boredom (Roy, 1960), the problems inherent in coercive control (Burawoy, 1979) and the dynamics of worker resistance (Beynon, 1975). Some of this ethnographic work later went in a neo-Marxist direction with Braverman's study of deskilling (1974) and Willis's ethnography of dead-end work (1977).

A third category of ethnographic organizational research addressed the practical reasoning skills of people coping at the bottom of bureaucracies. Ethnographies conducted within the framework of ethnomethodology (on which see Pollner and Emerson, 2001) focused on the way workers understood the bureaucracy's formal rules and invoked them informally in accounts of how they achieved the organization's goals (see Bittner, 1964; for an application to prisons see Weider, 1974). This often focused on the work of professionals and semi-professionals, such as doctors (Becker et al., 1961), nurses (Chambliss, 1996) or psychotherapists (Schwartz, 1976), the thrust of which was to show how complex jobs are ordinary in that they involve practical reasoning skills. Other ethnographies had the opposite effect and alerted us to the tacit knowledge possessed by workers and only by means of which the pressure of work at the bottom of the bureaucracy could be accomplished. They uncovered the operation of discretion, decision making and the complexity of knowledge required for routine jobs (for examples see Brewer, 1991; Finlay, 1988; Juravich, 1985; Paules, 1991).

These strands of ethnographic research on work persuaded qualitative sociologists to reconceptualize their notion of organization. It is not just that bureaucracy has dysfunctions or that alternative sets of informal norms exist, ethnography enables qualitative sociologists to see bureaucracy differently. Organizations are symbolic social institutions entirely rooted in people's practices for reproducing them. There is a recursive relationship between the formal and informal organizational structure and rules in which its formal character is seen to be the result of the ad hoc negotiation processes and practical reasoning of its workers. Workers often try to follow the formal rules but have to engage informally in practical reasoning and ad hoc practices to operationalize them when the formal rules are incapable of meeting the job at hand, such that fulfilment of the organization's formal goals requires informal organizational rules, tacit knowledge and discretion. However, workers have to make it look as if the formal rules were followed as part of the organization's coercive control, so engage in further ad hoc practices to ensure the paperwork conforms to procedures. Organizations have no reality other than that given them by people who reproduce the appearance of formal structure in their informal practices and lay reasoning. This reformulation is the culmination of ethnography's long-standing application to the study of work and owes all to ethnography's special approach as a method: its focus on the naturally occurring activities and social meanings of workers employed in real life organizations, captured in their own words and understood in their own terms.

ETHNOGRAPHIC RESEARCH IN POLICE ORGANIZATIONS

It is instructive to highlight some features of the practice of ethnography by reference to my own study of one police organization, the Royal Ulster Constabulary (RUC). The research was conducted in 1987 and I employed Kathleen Magee, a young, female Catholic as the

ESRC-funded research assistant. This fact is interesting for two reasons. It was an early example in British sociology of multiple researcher ethnography, something that was more common at the time in social anthropology; secondly, it gave a high profile to gender and sectarianism within the organization, both highly controversial issues for the RUC (the results are discussed in Brewer, 1990, 1991; Magee, 1991). It is impossible here to accent all features of the research design but some points are worth consideration.

The research was overt, thus access was negotiated and permission obtained from the gatekeeper, the Chief Constable. Hornsby-Smith (1993: 53) makes a useful distinction between 'open' and 'closed' access, the latter involving fields where controls are likely to be imposed and barriers erected. Anticipating the RUC to exemplify the latter, it was essential beforehand that attention be given to what the gatekeeper thought sensitive, so the research was presented to him in such a way that permission might be granted. This strategy involved an important ethical compromise, for the interests of the gatekeeper were allowed to affect the conduct of the research, although the ethical problems around covert ethnography are just as great (see Bulmer, 1982). But there are different levels of gatekeeping and once in the field informal gatekeepers tried to restrict the access given on their behalf by the head. The permission of the chief constable was a disadvantage in the field because it raised doubts in the minds of people lower down in the organization about why the management had agreed to the research (for similar experiences in police organizations see Fox and Lund, 1974). Retrenchment from below in organizational research is as much a problem as limitations from above.

The selection of cases and planning for the possibility of empirical generalizations to other police organizations also needed careful thought before entry into the field (see also Hartley, Chapter 26 this volume). Stake (1998: 88–9) identifies three kinds of case study. The intrinsic case studies address one instance (perhaps the only instance) of the phenomenon; collective case studies focus on several instances of the same phenomenon to identify common characteristics; while instrumental case studies focus on the phenomenon because it facilitates understanding of something else. Collective cases permit empirical generalizations; instrumental cases theoretical inferences. Empirical generalizations involve application of the data to a wider population and there are two ways this can be done ethnographically. It is possible to design the project as a series of parallel ethnographic studies with different cases or with the same case in different fields, perhaps using multiple researchers (for example see Brewer, Lockhart and Rodgers, 1997), or to design the single project in the mould of similar ones in different fields so that comparisons can be made across them and a body of cumulative knowledge built up. This option was adopted in the study of the RUC. The project was designed deliberately to follow the pattern of ethnographic studies of police organizations in socially homogeneous societies without communal conflict so as to add to this cumulative knowledge the dimension of studying a police organization in a divided society. This allowed us to explore the impact of civil unrest in routine police work.

Effective sampling of cases is critical to the aspiration to engage in empirical generalizations. To sample means to select the case or cases for study from the basic unit of study when it is impossible to cover all instances of that unit. In some cases it is possible to cover all instances of the unit and sampling is unnecessary – this is possible when the unit of study is a specific organization interesting in its own right. But where there are many instances or where the ambition is to engage in empirical generalizations, sampling becomes necessary. The RUC research involved what Glaser and Strauss (1967) call 'theoretical sampling', in which an

optimal case is selected as the fieldwork site where the processes being explored can be expected to happen. With our intent to study the way in which police work was affected by communal conflict, in order to link up with those studies on routine police work in police organizations operating in socially homogeneous settings, we needed to select a site where routine policing took place. 'Easton' was selected as a police station purposely because it was in an area of Belfast where routine policing was possible. Sampling of cases was not the only consideration, for we needed to sample by time and event (on sampling in ethnography see Burgess, 1984: 61ff). The time frame spent in the field and the events and people encountered in the organization needed to be representative of the organization: too little time and the events and people encountered can be abnormal and unusual. We asked police officers to complete time budget diaries to determine our sampling of the time to spend in the field and we initially restricted the fieldworker's contact to a few hours a shift once a week, gradually building up to a full shift, including nights, twice a week for a whole year. This was done not only to ensure a representative cross section of people and events in the organization but also to facilitate the development of a fieldwork role for the researcher in which rapport could be established.

Ethnographers are viewed differently as a relationship is built up and trust developed. This bond of trust is premised on the same qualities people bring to all social relationships – honesty, friendliness, reciprocity, openness, communication and confidence building. Trust is rarely instantaneous and normally builds slowly. The RUC fieldwork shows that it is also sometimes not a one-shot process: trust continually needs to be worked at and reassurances given. Over a 12-month period in the field, a fieldworker's persistent inquisitiveness is bound to become something of an irritant, and van Maanan (1982: 111) warns that ethnographers must not expect to be liked by everyone. But leaving aside moments of irritation, most informants in the organization became confident enough of Magee's presence to express what were widely held fears about the research, mostly by humour but once by anger. Toward the end of a long and tiring night shift, when news was coming through of the murder of another member of the RUC, one policeman in particular decided to put the fieldworker through a gruelling test of trust that was something like a rite of passage that she needed to pass before she could be trusted (outlined in Brewer, 1991: 21–4 and discussed further in Brewer, 2000: 86–7).

Getting people to talk to you when trust has not been established is difficult; it is so even when this bond has been established. The problem can be compounded where people in the organization are suspicious of the management's motives in permitting access. People can be reluctant to talk to ethnographers and avoid one-to-one contact (see Westley, 1970 for his experiences in a police organization in the USA). In this case it is necessary to hang around long enough to force people to talk. With respect to the RUC, we used those naturally occurring moments when sensitive topics came up in conversation naturally or could be artfully manufactured to appear as if casual by use of props. We used as props artefacts like events seen on the television the night before or as they appeared on screen in the television room of the station, things read about in the daily newspapers and relayed by computer as they happened in police stations elsewhere. Our experience shows that recording data when people do talk needs to be handled sensitively too. The ethnographer's conventional notepad can be obtrusive, yet when the time in the field is extensive it is impossible to do without this aid. To recall events in detail in the evening or when in private is difficult and results in general impressions. Sometimes a tape recorder or video camera can be used to record data but these

are even more obtrusive. If note taking is the main form of recording data, one way of allaying fears is by taking notes as unobtrusively as possible. This can be achieved by reducing the visibility of the pad and the physical activity of note taking, occasionally foregoing it when the situation seems appropriate, and by emphasizing that the notebooks are not secret. In the RUC research, the fieldworker was instructed to consider certain spaces in the station as private (the recreation and television rooms) where note taking was not done *at the time* (but left to later), and to leave the notebook around the station so that people could read it and thus know it was not secret. We occasionally reiterated this point by showing respondents extracts of the data. However, irrespective of what occasion the ethnographer decides to record the data, writing up the field notes from the notebook in a more legible form is essential. The sooner this is done after recording the data the better. In our research, writing up of notes was done before the next venture into the field so that points of clarification at the next visit could be identified and new issues addressed. This was essential given the involvement of two ethnographers in the project.

The data collected was voluminous, containing over half a dozen large box files of typed notes. With this bulk, computer-assisted analysis packages for managing and organizing the data are very useful (on which see Fielding and Lee, 1998), although these packages were in their infancy at the time and were not used. This bulk also meant that field notes had to be carefully maintained. While notes are a running description of events, people and conversations, we kept a note of the time, date, location and identities of the people involved, and of other circumstances (Burgess, 1982: 192 calls these 'methodological field notes'). We recorded notes of many conversations and identified whether they were verbatim or précis. Records of what is seen and heard (called 'substantive field notes') were kept separate from our interpretation of it (called 'analytic field notes'). Analysis was not a separate process from fieldwork and the initial tentative interpretations occurring while in the field were recorded but kept separate from the data. We also kept a diary separate from the field notes in which I asked the fieldworker to record her impressions, feelings and emotions, reflecting on such things as the developing relationships in the field, the emotional costs and problems in the field and other exigencies that affected the research. This was used later as the basis of the reflexivity that contextualized the results. Finally, we made duplicate copies of the notes once they were written up and we kept them in different places for security reasons.

The final issue to be highlighted here concerns the handling of the ethnographer's identity in the field. It is a myth to see ethnographers as people without personal identity, historical location and personality who would all produce the same findings in the same setting. Because gender is perhaps the primary identity for most people, feminist ethnographers were amongst the first to deconstruct ethnographic practice and identify the ways in which identity influenced fieldwork relations (for an excellent overview see Warren, 1988). Attention has been given to the special problems of female ethnographers in obtaining entrée, the problems around establishing rapport and trust, and sexual politics in the field. Van Maanan (1981: 480) once argued that researchers on the police had to be male in order to be able to participate fully in masculine occupational cultures, although this is no guarantee (for the difficulties of a male researcher in establishing rapport in the police see Warren and Rasmussen, 1977: 358). However, while female ethnographers have discussed their treatment as sex objects, their gender ensured they were seen as a light relief from the demands of the job, seen as less threatening than males (Hunt, 1984), and treated as 'acceptable incompetents' (Lofland, 1971: 100), resulting in informants giving them more time and taking more care to explain (for

example see: Easterday et al., 1977; Hunt, 1984). The downside is that young female ethnographers can be subject to sexual hustling, fraternity and paternalistic attitudes from male respondents, and treated as gofers, mascots, or surrogate daughters. Although some of these roles may be useful in establishing rapport with men, female ethnographers can receive the unwanted sexual attention of male informants. Magee, for example, was asked for a date by several policemen, and it was only after some time spent in the field when her presence became routine that we were sure she was being talked to as a person rather than a sex object. Nonetheless her experience shows that female ethnographers should not risk over-personalized interaction and should be on guard for the sexual hustle disguised as research cooperation. Yet her identity proved a distinct advantage in another way, in that it pushed onto the research agenda issues normally glossed by the organization – gender and religion. In some settings gender is not the primary identity, although there is very little methodological debate about other biographical features. As a Catholic, Magee's religion was assumed by us to be problematic and we first tried to conceal it, which reflected our naivety in under-estimating the skill the Northern Irish have in telling identity from various subtle cues (for a discussion of how we managed the effect of her religion on fieldwork see Brewer, 1991: 24–7). Instances like this reinforce the importance of ethnographers being reflexive when writing up the results but also of ensuring that fieldwork is sufficiently prolonged and intensive so that relationships of trust can be built up in the field.

ASSESSMENT OF THE METHOD

The interpolation of method and methodology that characterizes ethnography has proved problematic. Within naturalism, ethnography was privileged as the principal method and weaknesses overlooked in exaggerated claims for its efficacy, while critics of naturalism as a theory of knowledge rejected ethnography more or less out of hand. This has led to two sorts of criticisms of ethnography. The natural science critique condemns ethnography for failing to meet the canons of natural science methods as applied to social life (for a modern example see Goldthorpe, 2000). Some principles it offends have to do with the role of the researcher. The natural science model of research for example, does not permit the researcher to become a variable in the experiment yet ethnographers are not detached from the research but are themselves part of the study or by their obtrusive presence come to influence the field. If participant observation is used in data collection, ethnography can involve introspection, or what Adler and Adler (1998: 97–8) call auto-observation, whereby the researcher's own experiences and attitude changes while sharing the field become part of the data. Another principle ethnography offends concerns methods of data collection. Methods that are unstructured, flexible and open-ended can appear to involve unsystematic data collection, in which the absence of structure prevents an assessment of the data because differences that emerge can be attributed to variations in the way they were collected. The rationale behind the highly structured methods of the natural sciences is to minimize extraneous variations in order to isolate 'real' differences in the data. This is why methods within natural science models of social research are designed to eliminate both the effects of the researcher and of the tool used to collect the data. Ethnography also breaches dearly held principles about the nature of data. The natural science model of social research seeks to describe and measure social phenomena by assigning numbers to the phenomena. Ethnography also describes and

measures, but it does so by means of extracts of natural language and deals with quality and meaning (see Bryman, 1988). As Dey indicates (1993: 12), meanings may seem shifty, unreliable, elusive and ethereal.

The other set of criticisms constitutes what can be called the postmodern critique. This attacks the exaggerated claims made by some ethnographers who fail to recognize its weaknesses in the light of postmodern deconstruction of science as an intellectual enterprise. In this respect, all knowledge is relative, so there are no guarantees as to the worth of the activities of researchers or the truthfulness of their statements. This 'moment' in the development of ethnography is referred to by postmodern critics as the 'double crisis' (Denzin and Lincoln, 1998: 21–2; for greater detail see Brewer, 2000: 38–54). The first is the crisis of representation. This challenges the claim that ethnography can produce universally valid knowledge by accurately capturing the nature of the social world 'as it is' – a view described as 'naïve realism' (for this critique in anthropology see Clifford, 1988; Clifford and Marcuse, 1986; in sociology see Atkinson, 1990; Atkinson and Hammersley, 1998; Denzin, 1997; Hammersley, 1990, 1992; Hammersley and Atkinson, 1995; van Maanen, 1988). All accounts are constructions and the whole issue of which account more accurately represents social reality is meaningless (see Denzin, 1992).

The second is the crisis of legitimation. In as much as ethnographic descriptions are partial, selective, even autobiographical in that they are tied to the particular ethnographer and the contingencies under which the data were collected, the traditional criteria for evaluating ethnography become problematic, as terms like 'validity', 'reliability' and 'generalizability' lose their authority to legitimate the data. 'Validity' refers to the extent to which the data accurately reflect the phenomena under study (also sometimes called 'internal validity'), 'reliability' the extent to which measurements of it are consistent, and 'generalizability' the applicability of the data to other like cases (also sometimes called 'external validity'). The postmodern critique challenges that there is an objective and knowable 'real' world that can be accurately described and this undermines all evaluative criteria.

These crises have implications for how we should understand ethnographic accounts: ethnography does not neutrally represent the social world (but, then in this view, nor does anything else). There are implications for the claims ethnographers are able to make about their account: ethnography is no longer a privileged description of the social world from the inside (once called 'thick description' in order to emphasize its richness and depth). There are also implications for the written text, which attempts to represent in writing the reality of the 'field', for ethnographers should no longer make foolish authority claims in order to validate the account as an accurate representation of reality but be 'reflexive'. That is, reflect on the contingencies that bore upon and helped to 'create' the data as a partial account. Thick descriptions, therefore, do not represent 'reality as it is' because such descriptions are selective from the various competing versions of reality that could have been produced and end up presenting a partial picture: if ethnographers see themselves as cameras 'telling it like it is', the picture is blurred because there is more than one image on the lens.

CONCLUSION

So whither ethnography? Ethnography is not left in the postmodern state of complete scepticism and relativism in which 'anything goes'. Some ethnographers have rescued it from

the worst excesses of postmodernism while still accepting some of the more valid criticisms of naïve realism. As Seale argues, quality in qualitative research is possible (1999: 17), and a number of sets of guidelines exist by which the practice of ethnography is codified and can be made rigorous (Brewer, 1994; Hammersley, 1990, 1992; Silverman, 1989; Stanley, 1990). What one might call 'post postmodern ethnography', advocates the possibility and desirability of systematic ethnography and remains rooted in weaker versions of realism. Martyn Hammersley's account of subtle realism (1990: 61, 73ff, 1992), for example, makes it clear that he believes in independent truth claims that can be judged by their correspondence to an independent reality. 'Post postmodern ethnography' contends that while no knowledge is certain, there are phenomena that exist independent of us as researchers and knowledge claims about them can be judged reasonably accurately in terms of their likely truth. This shares with naïve realism the idea that research investigates independently knowable phenomena but breaks with it in denying that we have direct access to these phenomena. It shares with anti-realism recognition that all knowledge is based on assumptions and human constructions, but rejects that we have to abandon the idea of truth itself. This is the best ethnography can claim but it is more than enough.

FURTHER READING

For a general introduction to the method for beginners see Brewer (2000). This mounts a strong defence of ethnography against various contemporary critics. For a view of the method's limitations and potential by a leading quantitative researcher see Goldthorpe (2000). The handbook collated by Atkinson, Coffey, Delamont, Lofland and Lofland (2001) contains chapters on various aspects of the history, methodology and practice of the method written by some of the world's leading ethnographers.

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