Ethnography and participant observation

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Down the canyon, smoke from meat fires drifted through the cedar and mesquite trees, and if I squinted my eyes in the sun's setting, I could almost pretend that Spanish soldiers in silver chest armour and bladed helmets or a long dead race of hunters were encamped on those hill sides. Or maybe ever old compatriots in buttered brown wending their way in and out of history - gallant, their cannon-nipped colors unfurled in the rolling smoke, the fatal light in their faces a reminder that the contest is never quite over, the field never quite ours.

James Lee Burke (1993: 344) captures, in the final paragraph of In the Electric Mist with the Confederate Dead, all the important things about ethnography. We need to use all our senses: smell, sight, hearing, taste and touch (Stoller, 1989; Wafer, 1991). Smells drift, we have to squint to see in the rolling (i.e., turbid) smoke. Our fieldsite may be among hunter gatherers, or Spanish soldiers. Our contest is never quite over, their field is never quite ours. And, of course, if we were all able to write as well as Burke the social sciences would be much richer. Writing well is particularly important in qualitative research, at all stages from planning to publication. So too is reading: reading wisely and widely throughout the process (Delamont, 2002).

This chapter shows how participant observation is actually done. The processes of conducting observational research will be explained and illustrated with examples from ethnographies. There are three sections. The first section explores what is meant by 'ethnography', 'fieldwork' and 'participant observation', locates these three terms in relation to the wider term 'qualitative research' and clarifies their place in anthropology and sociology. In the second part, ways in which ethnography is done in anthropology and sociology will be contrasted using four fictional researchers. Third, the processes of conducting ethnographic research are explained with three subsections on watching, recording and reflection. The reflection section will, by definition, include material on how ethnographic research is written and how it is read. The 'watching' section explores the cycle from access negotiations through to exit from the field.

Two bodies of scholarship will be drawn on - anthropology and sociology. Some coverage of the history of the method in each discipline ensures that the pioneers in anthropology such as Boas and Malinowski, Zora Neale Hurston and Camilla Wedgwood on the one hand, and in sociology of the women and men of Chicago on the other, are recognized. From the anthropological side, examples come from the ethnography of Europe (Delamont, 1995). Research conducted on British social anthropologists (Delamont et al., 2000a) is also drawn upon. Interviews were conducted with lecturers and doctoral students in four universities: all universities: ('Kinglford', 'Soutersharn', 'Masonbridge' and 'Lachendan') and respondents are protected by pseudonyms. My own background is in anthropology, the British variety taught at Cambridge in the 1980s. I was taught by Edmund Leach (1984, 2001), one of Britain's greatest anthropologists, who introduced structuralism to the UK. After graduation I decided to use anthropological methods in the British educational system, and therefore faute de mieux I became a sociologist. In that era, doing fieldwork in the UK made a career; as an anthropologist almost impossible (on this point see Jackson, 1987, and Pink, 2000). The interview with Dr Hettick of Masonbridge (Delamont et al., 2000a: 75) reflects on this: 'My PhD was done in Britain, which was extremely unusual at that time in Britain ... most people feel who've worked in Britain that that kind of work was not particularly regarded by the British anthropological establishment as proper anthropology ... the
anthropology of Britain has always been extremely marginal.'

ETHNOGRAPHY, FIELDWORK AND PARTICIPANT OBSERVATION DEFINED

There are three closely related terms - ethnography, fieldwork, participant observation - all of which are part of a wider term, qualitative research. Qualitative research can include many different methods, such as many varieties of interview, documentary work, and the collection of personal constructs and mental maps, as well as observation. The vast majority of the qualitative studies conducted in the past twenty years in disciplines other than anthropology have been based on interviews of one sort or another rather than proper ethnography, as Atkinson (1997), Atkinson and Silverman (1997), Delamont (1997) and Delamont et al. (2000b) have pointed out. This chapter is about proper ethnography, that is, participant observation done during fieldwork.

Participant observation, ethnography and fieldwork are all used interchangeably in the literature, and are therefore synonymous: they can all mean spending long periods watching people, coupled with talking to them about what they are doing, thinking and saying, designed to see how they understand their world. I use ethnography as the most inclusive term, with participant observation and fieldwork being useful descriptions of the data-collection technique and the location of data collection. Fieldwork is the data-collection phase of the research process, especially when researchers leave the university and go out into the world. So a person who sent out postal questionnaires would probably not talk of fieldwork, whereas an investigator doing participant observation, or ethnographic interviewing, in a factory, a hospital, a school or a village in Portugal would do so. The term can cover collecting quantitative data (for example a census) if these data are collected 'in the field', especially during a period of ethnographic observation. We can therefore define the term for this chapter as follows: Fieldwork is the term used in qualitative research to cover the data-collection phase when the investigators leave their desks and go out 'into the field'. 'The field' is metaphorical: it is not a real field, but a setting or a population.

Participant observation is used to cover a mixture of observation and interviewing. In the field the researchers' aim is to understand how the cultures they are studying 'work', that is, to grasp what the world looks like to the people who live in the fishing village, the boarding school or the mining community. The researchers need to discover what 'their' people believe, what they do at work and in their leisure time; what makes them laugh, cry and rage; who they love, hate and fear; and how they choose their friends and endure their relations. This is done by living with the people being studied, watching them work and play, thinking carefully about what is seen, interpreting it and talking to the actors to check the emerging interpretations. The term 'participant observation does not usually mean real participation: researchers do not usually catch fish, teach classes or dig coal, rather they watch these things being done, and 'help' occasionally. It is important to participate enough to be able to write feelingly about the nature of the work: its pains and pleasures, smells and sounds, physical and mental stresses. However, the researcher cannot actually spend the whole time fishing, teaching or digging coal, because that would prevent both studying other members of the social world and, perhaps more vitally, time spent writing the fieldnotes, thinking about the fieldwork, writing down those thoughts, and systematically testing the initial insights in the setting. So 'participant' does not mean doing what those being observed do, but interacting with them while they do it. The researcher may do the same things, but that is not a requirement.

In traditional anthropological fieldwork, researchers go to a distant location, possibly in an underdeveloped country the other side of the world, and the fieldwork may last two years or more. In sociological research, the field is more usually visited on a daily basis with researchers returning to their home at night. These are the two main types of fieldwork, which we can gloss as total immersion and partial immersion. In anthropology and some varieties of sociology, researchers have traditionally moved to live at the fieldsite: in the fishing village, in the boarding school, on the housing estate next to the coal mine. In such cases researchers are totally immersed in the culture under study, twenty-four hours a day. Most observational research in sociology and education and the applied disciplines is based on a more partial immersion: researchers eat, sleep and relax at home but spend a large chunk of the twenty-four-hour period in the factory, the hospital or the school. The biggest difference between these is probably the amount of intellectual and emotional support available from academic supervisors or colleagues. In total immersion fieldwork researchers may be very isolated, very lonely, and lose their way. Contact is likely to be by letter, often with a long time-lag
between despatch and any advice arriving from home. In both total and partial immersion fieldwork, being fully engaged in another culture is a
\textit{sine qua non}. When the research is done, the result is an ethnography: a theorized account of the culture studied with ethnographic methods.

\section*{ETHNOGRAPHY IN ANTHROPOLOGY AND SOCIOLOGY}

Ethnography has a long history in both social and cultural anthropology and in sociology. Anthropologists still like to claim that they have the exclusive custody of the real, true ethnography (Delamont et al., 2000a) and rely on their use of the method to distinguish themselves rhetorically from other social scientists. As a PhD student at Southershams, Louisa Montoya, told us: ‘The qualitative methods used in anthropology are specific to this discipline.’ Hirsch and Gellner (2001) feel able to state that while other disciplines may or claim to do ethnographic fieldwork, the term ‘ethnography’ to refer to a monograph ‘is confined to anthropological circles’ (p.1). In the face of such claims, arguments that there is a convergence between anthropology and other disciplines seem unimpressive. It is true that anthropologists have used ethnography as their main method, and that no other technique (experiment, quasi-experiment, survey, observation with pre-specified schedule, questionnaire-based interviews, life history collection, archival or other documentary scrutiny, or narrative analysis) has ever rivalled it. Anthropologists have used some other techniques as substitutes to living in a culture full time, but only as substitutes (Faubion, 2001; Macdonald, 2001). In the USA, Boas is seen as the pioneer of fieldwork, inspiring disciples including Ruth Benedict and Margaret Mead (see Behar and Gordon, 1995). Zora Neale Hurston’s work prefigures much contemporary debate (Hernandez, 1995). In the UK, Malinowski is usually credited with inventing fieldwork, and his disciples included Audrey Richards and Camilla Wedgewood (Leach, 1984; Lukachaus, 1986).

Sociology has used ethnography as long as anthropology, that is, since the 1890s, but it has often been unfashionable, a minority pursuit. Ethnography has never had the status, and sole domination, in sociology that it had, and has, in anthropology. Ethnography and other qualitative methods were pioneered at Chicago, alongside survey and statistical techniques. Chicago sociology was robustly empirical, and this empiricism spread to other centres of sociology in the USA (Deegan, 1988, 2001). In Chicago, which was the most important sociology department in the world from 1892 until 1935, and still has massive influence, ethnographic methods fell out of fashion in the early 1960s (Platt, 1995; LeCompte, 1998).

Since the mid-1970s there has been a rebirth of qualitative methods in sociology and a rapid growth in their popularity in education, geography, nursing studies and other social sciences (see Atkinson et al., 2001). Journals, textbooks, handbooks and conferences have all developed. This growth has not, however, united sociological and anthropological uses of the method (Delamont and Atkinson, 1995). In the past decade there have been several apocalyptic statements that traditional ethnography is dead. That is simply wrong, but in this chapter there is no space to rehearse the arguments, which are covered elsewhere (Delamont et al., 2000b; Atkinson et al., 2003). Traditional ethnography continues as strongly as ever.

\section*{ILLUSTRATING ETHNOGRAPHY IN THE TWO DISCIPLINES}

To illustrate sociological versus anthropological ethnography, we can contrast four fictional researchers, starting with Rachel Verinder, an anthropologist, and Lucilla Majoribanks, a sociologist, both doing PhDs at Boarbridge. Doctoral students are used for these fictional episodes because, in practice, the bulk of the ethnographic research done in both disciplines is actually conducted by doctoral students or junior scholars, because senior ones can rarely get time or money to conduct sustained fieldwork themselves. These two fictional characters are based on empirical research on young scholars, the published autobiographical reports on fieldwork, and experience watching young colleagues over thirty years.

Imagine Rachel Verinder has graduated in social anthropology, and has been offered a scholarship to do a PhD at Boarbridge University. She and her thesis supervisor, Dr Selma Goby, decide that Rachel should do research on Galicia, a region in north-western Spain, and the Galician separatist movement. Rachel did A-level Spanish and has spent several holidays there since, and she is interested in Atlantic maritime cultures, the lives of women in fishing communities and regionalist movements.
in Europe. For nine months Rachel stays in Boarbridge, improving her knowledge of social anthropology, especially the anthropology of Europe and maritime societies, reading about Galicia and other regions of Spain, and learning the Galician language. Then she packs appropriate clothes and equipment for collecting data, such as a digital camera, mindless recorder, and lots of notebooks and pens, and sets off from Boarbridge for the ferry to Santander. When she lands in Spain she heads for Galicia, and searches out a fishing village where she will live for the next year or more. She has to try several villages before she finds one that has a bus service to the nearest town, has a family who are prepared to rent her a room, and has fishing boats still working. Rachel’s research is similar to that done by Canadian (Rosenberg, 1988), American (Reed-Danahey, 1996), Australian (Just, 2000) and British (Goddard, 1996) anthropologists in Europe over the past thirty years.

Lucilla Maioribanks starts at the same time and decides with her supervisor, Dr Henry Centen, that she will do an ethnography of students training to be laboratory technicians at a further education (vocational) college. There is a course at a college, ‘Midhurst’, in Boarbridge, so Lucilla can live ‘at home’. She spends about six months reviewing the literature on vocational education, on laboratory technicians, on science and technology education and on qualitative methods. Dr Centen insists that Lucilla writes a draft of her methods chapter, a review of the literature, and a 2000-word paper on her fore-shadowed problems, that is, the ideas she expects to develop during the observation. Once her ideas are clear, Lucilla writes to the Principal of Midhurst College, to ask for access both to the institution and to the specific course. He agrees, and despatches her to the staff who teach it; they agree, and she is able to start her data collection with a fresh cohort of students in September, a year after she began to be a student. (Outwith the USA clearance from Human Subject Committees is needed only for medical research.) This will produce a sociological ethnography of an educational institution like those done by American (Raissiguet, 1994; Vali, 1986), Australian (Walker, 1988) and British (Gleson and Mardle, 1980; Piseke, 1993) investigators.

Meanwhile in Galicia, Rachel starts her fieldwork. The most important part is living in the village, and watching what goes on. When it is not feasible to join in, she will watch what she is allowed to. Once the villagers have got used to her being around, watching is supplemented with talking. Rachel talks informally to everyone possible, does formal interviews with people, collecting their family trees and hearing their life stories, plus gathering folk tales and songs, listening to gossip, jokes and legends. A fieldworker is likely to draw maps of the village, of the insides of houses, of the graveyard, diagrams of the seating plans at weddings or funerals, the layout of fishing boats and anything else that has a spatial angle. Rachel will count the number of residents in the village, count the fishing boats, measure the sizes of fields, orchards and pastures, count cows, sheep and pigs, estimate the size of the fishing catch, work out how many tourists come, how many people get the bus each day, how many cars, taxis, motor scooters and even bicycles there are, how many pupils in the school and so on.

It will be important to hear who speaks Galician and who does not, and when Galician and Castilian (Spanish) are used. If Rachel is allowed on a fishing boat she will go, if not she will find out why women are not allowed to sail on them. The lives of the women will probably be easier for her to observe than those of men. If there is separatist political activity, Rachel will try to attend any meetings, meet the activists and discover what is motivating them. Apart from what she can see, and what she can learn by listening and asking, there may also be documents. Rachel might spend days in the provincial capital working on municipal archival material, or in the cathedral or ecclesiastical records, or both. If the Galician regionalist movement has produced newsletters, pamphlets or books, these will all be read. Rachel might get the schoolchildren to write her something, or ask to read letters sent home by villagers living abroad.

While Rachel is in Galicia, Lucilla is doing her ethnography of the students in the vocational college, in Boarbridge. Lucilla can intersperse her data collection with teaching undergraduates, going to seminars, seeing her supervisor every week and using the library. She is only in the field for short periods of some days, does not have to learn a foreign language or eat strange food. She goes to the college nearly every day, sits in the lectures and workshops, writing pages and pages of fieldnotes. She interviews the lecturers, formally and informally, and she hangs out with the students, sometimes going out with them socially. She talks to them in their break, and she interviews them formally too.

One difference between Rachel and Lucilla’s fieldwork is the focus of the research. Lucilla and Dr Centen try to keep very tightly focused on a pre-specified topic: the occupational socialization and student culture of the trainee laboratory technicians (see Colley and Atkinson, 1994). To gather data on the catering staff, or the fine art
appreciation course, or the people taking academic sociology at night school, would be a distraction, a different project, a diversion. If Lucilla drifted, Dr Centum would force her back to 'her' topic. In contrast, Rachel is more likely to settle on a tight focus only on her return to Boarbridge. She has to collect more, because once 'home' she is not, realistically, able to go back. Lucilla, in contrast, can return to consult documents or re-interview participants; and indeed much of what she may need is not 'in the field', but in libraries and offices elsewhere in Boarbridge.

As Rachel goes on living in the village she will find that she has been told different things by different people and she will set out to find out why. As her Galician improves, she will spot that people were lying to her, and find out why they did so. It is common, for example, for villagers to think that outsiders are tax inspectors, CIA agents, spies or otherwise undesirable. As her understanding of the people and the place deepens, Rachel will be able to come up with more and more questions to ask: to cross-check her ideas, to corroborate earlier information, to test her developing hypotheses. She also makes friends and gets involved.

As Lucilla spends more time in Midhurst College of Further Education she discovers that two of the lecturers bitterly resent her presence. One is openly rude, the other keeps suggesting she does not come to his classes because they are 'routine', 'boring' or 'the lads will be using bad language'. This man has terrible discipline problems, which make watching his classes embarrassing. The rude man dislikes the changes in English vocational education that have occurred over the past twenty years. He yearns for the 'old days' when vocational education was dominated by young male apprentices who came in on day release to learn trades. Both these men refuse to be interviewed, and will not talk to her in the common room or dining hall. They see her as the university's spy, or as an informer working for the Principal. The other staff are friendly, and not at all interested in what Lucilla is doing. The students are puzzled by her research: it is not scientific. They are going to get jobs in laboratories in proper subjects like electrical engineering and computer science. Overwhelmingly male, they are tolerant of Lucilla, but suggest she is too scruffy to get a husband and ought to dress more smartly.

At the end of her year or more, Rachel packs up her staff and returns to Boarbridge. There she sorts out her data and picks a central theme with which to organize them. Using anthropological theory, and organizing her data around that theme, she writes a PhD thesis. Once that is done, she starts to publish articles about 'her' village, and to write a book. That takes a considerable time, so that if Rachel spent 2000 in her village, the book would probably appear in 2005 or so. Jane Cowan, for example, did her main fieldwork in Sohos in the 1983–5 period, got her PhD in 1988, and published her book in 1990. Rachel's career after her PhD should include a lectureship and return to Galicia periodically over the next ten years or so. She might do her next piece of research on Galicians in some other region of Spain (e.g. Catalonia) or working in another country such as Switzerland (Buechler and Buechler, 1981, 1987). She might study a separatist movement in another country.

At the end of her year Lucilla, too, withdraws from Midhurst, although she might still do interviews with the trainees or staff, or meet the trainees when they go out together. She could have decided to re-interview them as a formal follow-up stage of the project. All these are easy because she and her informants are all in Boarbridge. Lucilla too needs to organize her data (although she should have been doing this all along, transcribing interviews, and putting her fieldnotes and interview transcripts into AtlasTi as she collected them). Using a sociological theory, she writes her PhD, and then begins to publish papers and ideally a book. This too takes several years. Perhaps she too gets a lectureship, but she is much less likely to revisit Midhurst, or re-study trainee laboratory technicians or even FE: her career would be enhanced by doing her next project on a different topic, perhaps a team of biologists working on mosquito control or stem cells.

If Rachel Verinder is going to be a successful researcher she has to go to her Galician fishing village and come home again to Boarbridge with her data. She is a failure if she hates the staple diet so much that she comes home after a week, saying the villagers are barbarians. However, she is also a failure if she becomes more Galician than the Galicians. If Rachel stops writing her diary, collecting and recording data, and thinking like an anthropologist, and becomes a leading light of the Galician separatist movement, then she has 'gone native'. If Dr Goby visits and finds Rachel leading the protest march on Madrid, or planting a bomb in the police post, or organizing a school boycott, she would have every reason to accuse her of 'going native'. For Lucilla 'to go native' is possible, but more complicated. If she marries one of the students or staff, if she abandons the PhD to train as an FE lecturer, if she decides to retrain as a laboratory technician herself, or becomes a research or officer for the technicians' trade union, then she has gone
native. This does sometimes happen with people doing fieldwork in health settings, who abandon sociology to train as a doctor or a nurse, but it is not common in the sociology of education or of science.

Rachel and Lucilla’s research and careers look distinct, and are not hard to classify as anthropology and sociology. Rachel will belong to the Association of Social Anthropologists and the American Anthropological Association, publish in the Journal of the Royal Anthropological Institute and American Ethnologist. Lucilla will belong to the British Sociological Association and the American Sociological Association, and publish in Sociology and Qualitative Studies in Education. They could be friends, even fiancées or lovers, but they are in different disciplines. They use different theories, and theorists, and have no reason to read or cite each other’s work. Because their topics are so separate, this is not surprising. However, the gulf is not due to empirical topic, but to disciplinary identity grounded in theoretical differences. Lucilla will never read or cite the anthropology of education either (Atkinson and Delamont, 1980, 1990; Delamont and Atkinson, 1995).

To illustrate the differences further, we can contrast another pair of ethnographers studying urban areas. Imagine that Rachel Verduer starts her PhD alongside a fellow student, Franklin Blake, who is passionately committed to urban anthropology. When Rachel sets off for her Galician village, Franklin packs his bags and heads for Munich, using one of four strategies to find a manageable project. He could choose a neighbourhood, live in it, and treat it as an urban village, like Press (1979) in Seville. He could choose one set of people, such as migrants from a particular place, and make them his focus as Griffio (1985) did in Lyon, and Kenny (1960) did in Madrid. He could choose an institution or organization, such as a hospital or a factory, and treat it as a microcosm of the city. This is frequently done by urban anthropologists in America, such as the contributors to the collections edited by Messerschmidt (1982) and Baranowsky and his colleagues (1991). Alternatively, Franklin could choose one category of people, such as members of a trade union, or tour guides, or priests, or practitioners of acupuncture, or antique dealers, and focus on them (e.g. Sheehan, 1993, on Dublin intellectuals). McKevitt’s (1991a, 1991b) work in San Giovanni Rotondo focused on devotees of Padre Pio, and McDonough (1986) focused on elite families in Barcelona.

Munich has migrants from Greece, Italy, Vietnam, Turkey, all the nations of the former Yugoslavia, and Germans who have moved in from rural areas or other cities in Germany. Franklin could therefore choose all sorts of projects for his PhD in Munich, before settling down to use the same range of methods, or a selection of them, as Rachel uses in her fishing village. He could even focus on Bavarian separatists to parallel her Galician nationalists. The fieldwork contexts will be mere varied in a big city, depending on the project. McDonough (1986: xi) describes doing fieldwork at the opera:

families invited me into another experience of the opera house in the aristocratic loges of the first balcony. The first performance I saw was Montserrat Caballé singing L’Africaine in her home theatre. The magnificence of that performance highlighted the dualities of my role as participant observer—half attentive to the stage and half attentive to the dramas around me.

Compare that fieldwork setting with Belmonte’s (1989: 275) Naples, in a district he calls Fontana del Re, not at all like the opera house in Barcelona:

the district was notorious in Naples as a dangerous zone, a den of thieves, roughnecks, and prostitutes ... a disproportionate number of the inhabitants earned their living collecting cardboard and junk.

Let us assume that Franklin settles on studying the tensions among guest workers from the former Yugoslavia, and how previously shared identifiers such as Serbo-Croat are unavailing since 1989, a study to parallel Danforth’s (1995, 2000) on Greeks and Macedonians in Melbourne.

We can contrast Franklin with a sociologist, Garrett Monmouth, doing an ethnography in Boarbridge itself. He could have decided to focus on one neighbourhood, or one set of people, or one institution or organization. As Boarbridge is a historic city, Garrett might study the tourist industry, or museums, or heritage (Dicks, 2000). If he were more interested in generic urban issues he could focus on perhaps the young homeless (Hall, 2003), body builders using gyms (Monaghan, 1999) or bouncers on the door of clubs and pubs. However, let us imagine he has decided to focus on refugees from the wars in Yugoslavia, and also discovers the disintegration of the old ‘Yugoslav’ identity and the Serbo-Croat language.

The contrast between Franklin’s anthropology and Garrett’s sociology is just as stark as between Rachel and Lucilla. Franklin is away abroad, working in two other languages, German and Serbo-Croat (or rather Serbian and Croatian). He will not see his supervisor for nine months, merely exchanging letters or e-mails. As
Dr Teague of Southershams said to us: 'I don't feel that one should be poking one's nose into a student's fieldwork ... it can be awkward, embarrassing and annoying for a research student.' Similarly Dr Drummock of Kingfords said, 'The student has got to be independent enough to form their own judgements'. Students told us, 'You're not allowed to visit the department when you're in the field. Because you're supposed to be in the field' (Beulah Wyston, Southershams). Franklin is expected to collect a great deal of data and focus on his theoretical theme once 'home' in Boarbridge. Garrett will see his supervisor regularly throughout the fieldwork, and will be told repeatedly by his supervisor to focus on one aspect of the refugees in Boarbridge and not drift off target. Because they have both done PhDs on Serbsians and/or Croatsians in exile, the two men might attend the same seminars or conferences, and even compare findings, but Franklin is likely to go to the same meetings as Rachel, and Garrett to the same ones as Lucilla.

One other clear difference between Rachel and Franklin as anthropologists and Lucilla and Garrett as sociologists will have been their pre-fieldwork training, especially their exposure to methods training. Sociologists in the UK have been much more enthusiastic about compulsory training in research methods, including qualitative techniques, than British anthropologists have. As Dr Trelithick of Southershams told us: 'Participant observation is not: I would say, a research method which can be taught in the classroom and then applied in the field. ... It's something you can only learn by doing it.' Dr Fustjan of Kingfords concurred: 'All this business of training I think is largely spurious. It is something that is learnt by the experience of doing it. It's rather like teaching music. You cannot teach people how to play without a piano. It's only by playing they can learn. I think fieldwork is like that.'

All four fictional characters could struggle with the issues raised by the 'rhetorical turn' and by postmodernism. In the past fifteen years both anthropology and sociology have been through some turmoil about the ways in which data are analysed and texts written, but the debates are contained within each discipline.

In the next section the general lessons that all four would have learned from their fieldwork are distilled into some general precepts.

ETHNOGRAPHY FROM START TO FINISH

Ethnography is hard work: physically, emotionally and mentally exhausting. The research does not proceed in a straight line, but in a series of loops, because each step leads the researcher to reflect upon, and even revisit, earlier steps. For example, if an observation in a biochemistry lab included one postdoctoral fellow selling the researcher that the senior professor was having an affair with another postdoctoral fellow and allocated her the 'best' projects, that would open up research issues: 'How do projects get allocated?', 'How does gossip in the lab work?', 'What literature is there on women in science?', and so on. Such questions might be dead ends, but a researcher would need to re-read all the fieldnotes and interview transcripts, do a literature search, plan to raise all three topics in future fieldwork, and perhaps revisit any past fieldsites to raise those issues. That example comes from Kevles's (1998) historical reconstruction of the Baltimore case. If he had learned of such an accusation while doing an ethnography he could have followed it up in other labs, other research groups, but because it came up in a history he had no opportunity to pursue the topic further as an ethnographer would do. Because ethnography proceeds in loops, the foreshadowed problems are revisited, the access negotiations reveal key features of the setting used when analysing the data, the analysis returns the scholar to the fieldnotes, and so on ad infinitum.

During fieldwork it is vital to sample the setting in a systematic way. A good study focuses on different types of participants, includes observations made at all times of the day (and, perhaps the night), and in all the possible locations. It is not always possible for a researcher to watch the opposite sex, people of very different ages, or push into all the possible settings, but it is necessary to plan to observe systematically wherever possible. In a fishing village the researcher should not only observe fishing, but also net mending, the sales of the catch, boat repair, women's everyday lives, the days of men too old to go out to sea any more, and the experiences of children. Observations should not only encompass the sea, but also the church, the vegetable garden, the school and the fish market. Thinking through all the possible places to observe, the times to observe, the people to watch, and discovering whether or not it is possible and productive to do so is a central task of good fieldwork (Spradley, 1979).

The beginning, the middle and the end of fieldwork can all be problematic. Autobiographical accounts by anthropologists and sociologists suggest many researchers experience culture shock when conducting their fieldwork; this is rare when researchers gather their data by post while among
colleagues. Much of the methods literature also focuses on using the initial culture shock (Geer, 1964) as a particularly fruitful time for insightful data collection. Once accepted by actors at a fieldwork site, researchers have to guard against 'going native': abandoning the researcher perspective and adopting the views of the actors in the setting. Some researchers are reluctant to leave the field even when no further useful insights or data are being gained because actors in the fieldsite have become friends, and the fieldwork comfortable.

Fieldwork in an unfamiliar culture is both harder and easier than fieldwork in one's own familiar society. In an unfamiliar culture everything is strange, and so it is unlikely that the researcher will forget to convey that strangeness. In contrast, researchers studying their own society often fail to report, or to make anthropologically strange, many aspects of the setting. So the French researcher who goes to live in a Brazilian favela will have a more frightening, bewildering and confusing experience, but the data will be unfamiliar to her readers in France, and even exotic. The same researcher studying everyday life in France is likely to feel safe, clear-minded and coherent, but to have to work very hard to produce an 'interesting' account of the fieldsite, unless, of course, she becomes embroiled in witchcraft (Favret-Saada, 1980).

One of the biggest problems is that informants often want to help researchers, by showing and telling what they think investigators want to see and hear. Equally, informants may systematically hide things, and tell lies, to protect themselves, their secrets or their privacy. Researchers who prefer fieldwork to the quicker method of interviewing hope to get beyond the informants' impression management, even though sometimes they discover that their informants initially believed them to be spies, tax collectors or loose women (Kenna, 1992).

**Foreshadowed problem(s)**

Central to good ethnography is an intellectually thoughtful set of foreshadowed problems: ideas that will guide the access negotiations, the initial fieldwork, the early writing of the out-of-the-field diary. These come from reading, from colleagues and mentors, from the core of the discipline. The ethnography is only as good as the ideas the researcher deploys. At the time of writing I am planning an ethnography of opera tourism in Central Europe. The foreshadowed problems include, for example, ideas that people who pay for holidays focused on opera in Prague

and Budapest will also attend opera in the UK, will prefer 'traditional' production values, classic core repertoire works, and appreciate relatively low seat prices. That is, they also go to, for example, Opera North productions in Leeds. Prefer the Hebrew slaves in Nabucco to look like illustrations from a Ladybird book of the Bible rather than Jews in Dachau, want to see Tosca rather than Der Fliegende Hase or Die Fledermaus, and believe that the two London opera houses, Royal Opera House (ROH) and English National Opera (ENO), cost too much money, as do hotels in London. All these ideas, gleaned from, for example, the correspondence pages of the opera magazines and the brochures of the specialist tour companies, could be blown out of the water by the first period of fieldwork. That would be splendid: it is marvellous when the foreshadowed problems turn out to have been wrong.

This is one difference between team research and an individual project. In a team effort it is necessary to discuss the foreshadowed problems, and share them. The main difference between team ethnographies that ended up with a joint project, such as Strauss et al. (1964), and those that produce an unintegrated collection of individual accounts (e.g. Stuke and Easley, 1978) is the pooling of the hypotheses before, during and after the fieldwork. The ORACLE project on primary to secondary transfer (Delamont and Galton, 1986) involved coordinating the fieldwork of seven observers, most of them untrained.

In Galton and Delamont (1985) we discussed this as follows:

> The timetable of the ethnographic research allowed us to use the study of the 9-13 schools, in September 1977, as a pilot study for 1978, when the pupils transferred into the 12-18 and 11-14 schools. Thus by 1978 we had a full set of ideas from the 1977 study which we could use as 'foreshadowed problems' or 'sanitizing concepts' in 1978. Sara Delamont and Maurice Galton were involved in both years, and others only worked in one year, but some of the 1977 lessons were carried forward to 1978.

This was an unusual feature of a school ethnography. We had a chance to think for a year between the Ashburton phase of the research and the Bridgehampton and Coakhorne phase. Our account continues:

> We never believed that ethnographers enter the field open-minded. In the 1977 study of the two 9–13 schools we had a short list of 'foreshadowed problems' derived from our reading of other school studies. These were of two kinds: some vaguely 'theoretical' ideas we had derived from the literature, and some 'common sense' ideas derived more from our members' knowledge.

Among the more 'theoretical' ideas we were interested
in utilizing Basil Bernstein's (1971, 1974) ideas on classification and framing and visible and invisible pedagogies, the beginnings of labelling, and the notion of coping strategies. More concretely, we asked all observers to look carefully at pupils' 'adjustment' to their new schools, sibling comparisons, staffroom discussions of pupils, bullying and the schools' responses to it, and to compare 'theory' and 'practice' in such areas as curriculum balance, pupil groupings, allocation of teachers to classes and so on. For example, in Local Authority A we had found that allocation of children to bands at Guy Manning School was more closely related to the primary school attended than ability or heads' reports, and so we asked the observers in Local Authorities B and C to examine band allocation, class allocation and so forth.

This is a fairly typical list of foreshadowed problems. Some are theoretical at a high level of generalization (visible pedagogy), some are middle-range concepts (coping strategy), some are concrete issues (how are children allocated to classes?). The big problem with all team research is that each observer is bound to have her own agenda, and they may not all be equally explicit for the researcher herself, or in the fieldnotes or in the out-of-the-field diary and analytic memos. As we commented on the ORACLE project: "How far the observers took any notice of these "foreshadowed" problems is, in retrospect, unclear — because of the diverse nature of the observers.'

Access and initial encounters

Having done the reading and thinking to develop foreshadowed problems, the good ethnographer negotiates access to one or more fieldsites, making careful notes of all the interactions. Access can be by letters and formal interviews if the fieldsite is a formal organization or a private space such as a family. If the fieldsite is a public place, then access may be a process of hanging out and informal chats. There are many autobiographical accounts of access negotiations (see Delamont, 2002: ch. 6) by sociologists and anthropologists, and the topic is covered in all the textbooks. There are three golden rules. First, every aspect of the processes needs to be meticulously recorded, because vital features of the setting are made visible during the access stages. Second, failed access attempts are "data", just as successful ones are. Third, the harder it is to gain access, the more likely the work will be rewarding once 'inside' and vice versa: very often deceptively easy access leads to barrier-strewn fieldwork.

Geertz (1964) wrote the classic paper on initial encounters, and its insights have not been bemdered (see also Atkinson et al., 2003). Given the importance of fighting familiarity (Delamont and Atkinson, 1995; Delamont, 2002). The initial encounter is the best time to see the genuinely unfamiliar and to force oneself to search for the vantage point from which the familiar can be made strange.

Data collection

The biggest problem novices find when preparing for ethnographic fieldwork is that the methods books are not explicit enough about what to observe, how to observe and what to write down. It is very hard to describe in words how to observe. Wolcott (1981) has produced an excellent attempt comparing his methods in his anthropological research (in an African beer hall) and his educational projects in the USA. Essentially an ethnographer observes everything she can, writes the most detailed fieldnotes she can, takes time to expand, elaborate and reflect upon them outside the field and/or as soon as time permits, constantly pester those being observed to explain what they are doing and why, and sweeps up any documents, pictures or ephemera available. The aim is to produce, in Geertz's (1973) classic formulation, a thick description, of the setting and the actors in it, sufficiently rich to enable a reader to live in that setting without unwittingly violating its basic tenets. So if it is absolutely forbidden for women to set foot on fishing boats, or for Turkish children in Munich (Yalta-Heckmann, 1994) to have fireworks on New Year's Eve, that should be crystal-clear from the eventual publications, which will have been drawn from the observations and interrogations.

Recording

The most important thing researchers have to do is record what they see, usually in fieldnotes but sometimes on tape or film, because anything not recorded is lost. Once recorded, data are safe, although the real work of the research comes in analysing data, interpreting them, and writing them up into accounts for a wider readership. Fieldwork that is never written up is wasted. Reading the autobiographical accounts of both sociologists and anthropologists, it is clear that many ethnographers keep fieldnotes and other kinds of more reflexive records such as an 'out-of-the-field diary' in which theoretical ideas can be rehearsed. In the past decade, scholars have been more prepared to discuss and reflect on how
field-notes are, and can be, written (Spruck, 1990; Emerson et al., 1995, 2001). There is no point in observing things and leaving them unrecorded, or in leaving the scribbled notes unelaborated. If interviews are recorded, there is no point in leaving them untranscribed. If photographs are taken, they must be developed, printed and labelled (cf. Pink, Chapter 25, this volume). Film or video, likewise, must have a commentary made while the images are fresh (cf. Pink, ibid.). In anthropological fieldwork, the researcher may need to store data in tamper-proof boxes: in all kinds, it is sensible to create duplicates and back-ups as soon as practically possible. The importance of writing, still the most vital form of record-keeping, is explored in Delamont (2002: Chs 4, 8 and 12).

Reflecting

Central to ethnography is the constant and tiring process of reflecting. Reflexivity is the most important characteristic of fieldwork, and of analysis. Hammersley and Atkinson (1995) explore the concept thoroughly. Reflexivity is the way that qualitative researchers strive for reliability and validity, and the development and training of one’s reflexive skills and empathies is the keystone of what Coffey (1999) calls The Ethnographic Self. Constant exercise of reflexivity should inform all the stages from the fore-shadowed problems through the data collection to the eventual writing up (Ellis and Bochner, 1996). One vital stage of the fieldwork, where reflexivity needs to be exercised, is the exit from the fieldsite, which is too often neglected.

Many methods books spend thousands of words on access and initial encounters, while ignoring leaving. Yet the disengagement from the field is just as important as the entry and engagement. Fine (1983), Altheide (1980), Mawson et al. (1980), Wulff (2000) and Delamont (2002) all address leaving. A good basic principle is that once the field-site feels like home it is time to leave: fieldwork should be uncomfortable. Once it is feeling familiar, it is time to move on.

Analysis

The analytic strategies available to ethnographers have not changed in principle for a century (Strauss, 1987). However, the development of software packages that handle text has changed the practices of analysis and made it much more a matter of public discussion. Useful ways into the literature on software packages and practices are reported in Weitzman and Miles (1994); Fielding (2001) and Fielding and Lee (2002). Coffey and Atkinson (1996, 2001) address the epistemology of the new technologies. Bryman and Taylor (2003) are entirely devoted to analysis, with several chapters on the analysis of qualitative data. A greater self-consciousness about analysis is inextricably linked to the debates on writing ethnography.

Writing

The ways in which ethnographic data are written have been controversial for the past fifteen years, and the issues are complicated by the vogue for postmodernism. One of the major issues associated with postmodern standpoints concerns the so-called crisis of representation that was especially prominent among social and cultural anthropologists in the 1980s, and permeated other social sciences too in the subsequent years. This supposed crisis threatened the taken-for-granted foundations of social inquiry. The crisis of representation was centred on the appropriate modes of cultural representation or reconstruction. It was, and is, widely recognized that the ‘ethnography’ has for many decades referred to both the conduct of fieldwork in all its aspects, and the written product of that research such as the monograph. The scholar is thus recognizably engaged in a double process of engagement with the field. First, she or he is engaged in a protracted series of transactions and explorations with informants, in and of themselves, these engagements are far from innocent. The cultures and social realities reported in the course of fieldwork are dependent on the active explorations, and the joint negotiations, that the investigator undertakes in conjunction with her or his hosts and informants. Secondly, there are further acts of interpretation when the scholar acts as author. The discoveries of disciplines like sociology and anthropology are not the revelations of an independent social reality, but are fictions – in the sense that they are created and crafted products.

This general perspective was articulated and widely publicized in Writing Culture (Clifford and Marcus, 1986). A number of authors explored the textual conventions of cultural anthropology, and through their textual explorations raised more profound questions concerning the ethnographic project. Their reflections included the recognition that ethnographic representation is grounded in conventional modes of representation. These include textual devices and
CONCLUSION

The chapter started in roiling smoke, and stressed that the contest is never over. It must end in that same smoke. The field is never entirely ours. Ethnographic research is exhausting and fascinating and it is a great privilege to be allowed into other people's social worlds. Despite exaggerated claims that classic methods are outmoded, and classic textual forms extinct, there is plenty of scope for proper ethnography to be done. There are Spanish soldiers, hunter gatherers and Cajun detectives still waiting in the smoke for us to live among them and capture their worlds using ethnographic methods.

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REFERENCES


ETHNOGRAPHY AND PARTICIPANT OBSERVATION


