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## Research design

### Problems, cases, and samples

The conduct of ethnography can seem deceptively simple, by contrast with the pursuit of quantitative research. It may appear to require only that one ‘act naturally’, putting aside any methodological rules and constraints. Perhaps for this reason, in the past, new ethnographers, especially in anthropology, were often given little or no research advice before they set out on their fieldwork. Heald writes:

I had no fieldwork training, it wasn’t done in those days. We had fieldwork seminars where someone recommended a particular HB pencil and someone else told us that notebooks six inches by three were a good idea as they’d fit into your pockets. In the field it was largely as though one was treading in the footsteps of one’s ancestors, trying to learn the techniques by remote control through their books, and then finding out that it was all much more complicated.

(Heald, quoted in Okely 2012:6)

Shaffir’s (2009:215) experience in sociology was similar, he writes that ‘while field research was the *modus operandi* by which information was to be gathered, I was left to my own devices as to how this was best accomplished.’ Here we have a model of learning ethnography that does not even amount to apprenticeship. Rather, each new researcher must discover for her or himself what is required in order to produce an ethnographic study.

At the other end of the spectrum, in many countries today ‘research training’ is a major enterprise in which key skills and essential knowledge are identified and must be inculcated before novices enter the field (Okely 2012:6–7). Where the first approach leaves people frustrated and uncertain, if not panic-stricken, the second can turn learning how to do research into a chore – and, even more importantly, produce a distorted picture of methodology-as-technique (Hammersley 2011a:ch1). Against this, it must be emphasized that all social research requires the exercise of judgment-in-context – it is never a matter of merely following methodological rules; nor can all problems be anticipated, with the best strategies for dealing with them predetermined. This is especially true of ethnography.

However, the fact that the course of ethnographic work cannot be preordained does not eliminate the need for pre-fieldwork preparation. Nor does it mean that the researcher's behaviour in the field can be haphazard, merely adjusting to events by taking 'the line of least resistance'. Indeed, we shall argue that research design is crucial to ethnography, but that it must be a reflexive and iterative process, operating throughout every stage of a project.

Of course, like other researchers, ethnographers usually have to produce research proposals at the beginning of a study; whether for prospective supervisors, funding bodies, and/or ethics committees. While the flexible character of ethnographic research design means that no detailed and fixed plan can be provided, it *is* possible to formulate one that lays out how the research is envisaged, albeit with varying levels of detail and confidence. This will need to cover what issues and interests motivate the research, why they are important, what questions might be addressed, what case or cases would be relevant, what data could be used and how these may be accessed, what strategies can be employed to analyse these data, and how the research findings would probably be disseminated, and to what audiences. Not only will such an outline usually be necessary for the various external audiences just mentioned, but it will also be of value in clarifying for the researcher some of the issues that will need attention during the course of the inquiry (see Maxwell 2013). Of course, in formulating a research proposal for external audiences, it may be necessary to exaggerate somewhat the likelihood that the research will follow the particular path laid out, given the currently prevalent myth that research can be planned and scheduled from the start. But, for both prudential and ethical reasons, this should not amount to fundamental deception regarding the intended character of the research.

In this chapter, we will look at key aspects of ethnographic research design, though we will leave most discussion of the ethical issues to Chapter 11.

### **Foreshadowed problems**

Research always begins with some question, issue, concern, or interest, that has arisen from reading the research literature or from some other source, including professional or personal experience. As Malinowski (1922:8–9) emphasized, contrary to overly inductivist conceptions of research, such 'foreshadowed problems' are not necessarily a source of bias, and indeed are essential to the process of inquiry. At the same time, they will need to be revisited and probably reformulated over the course of the investigation.

Sometimes the starting point for research is a well-developed theory from which a set of hypotheses can be derived. While it is rare for ethnographic enquiry to follow this pattern, there is at least one classic participant observation study with this character: Festinger and his colleagues tested cognitive dissonance theory by investigating the response of members of an apocalyptic religious group to the fact that the world did not end on the day predicted by their leader (Festinger et al. 1956). More recently, a somewhat similar kind of approach is occasionally found where ethnographers work within a particular 'theoretical framework' (see, for instance, Burawoy 1998; Wacquant 2002, 2009; Salzinger and Gowan 2018). However, what is usually involved here is not the testing of a set of hypotheses, but rather the *use* of particular theoretical concepts to structure the research.

Most ethnographic research has been concerned with producing descriptions and explanations of particular phenomena, or with *developing* theories or explanations, rather than with testing existing hypotheses or even with applying a prior theoretical framework. A number of authors, most notably advocates of grounded theorizing, have pointed to the advantages of developing theory through systematic empirical investigation rather than by relying on

‘armchair theorizing’.<sup>1</sup> Nevertheless, as Strauss (1970) himself showed, considerable progress can sometimes be made, even before fieldwork begins, in clarifying and developing research ideas, by making use of previous studies. To illustrate this, he examined Davis’s (1961a, 1963) research on ‘the management of strained interaction by the visibly handicapped’:

Davis’s theory is about (1) *strained* (2) *sociable* interaction (3) in *face-to-face* contact between (4) *two persons*, one of whom has a (5) *visible handicap* and the other of whom is (6) *normal* (no visible handicap) [. . .] The [italicized] terms in the above sentence begin to suggest what is explicitly or implicitly omitted from Davis’s theoretical formulation. The theory is concerned with the visibly (physically) handicapped, not with people whose handicaps are not immediately visible, if at all, to other interactants. The theory is concerned with interaction between two people (not with more than two) [. . .] The interaction occurs in situations termed ‘sociable’; that is, the relations between interactants are neither impersonal nor intimate. Sociable also means interaction prolonged enough to permit more than a fleeting exchange but not so prolonged that close familiarity ensues.

(Strauss 1970:47–8)

Strauss goes on to show that, by varying these different elements of the theory, new research questions can be generated.

How much previous research and theorizing there has been that is relevant to any particular area of investigation will, of course, vary. Indeed, the absence of existing knowledge about a phenomenon or process itself represents a useful starting point for research, and this is a very common rationale provided for ethnographic studies. Thus, Sobh and Belk (2011) present their investigation of the home life of middle-class women in Qatar as opening up a new field which complements both investigation of the symbolic meanings of domestic spaces in the West and the study of such spaces in traditional Islamic societies. A similar rationale was provided by Mac Giollabhuí et al. (2016) for their investigation of undercover police work. They write that:

There is marked imbalance in our academic understanding of the police, which is tilted heavily towards the organization and culture of uniformed officers. We still know very little about the values, attitudes and informal rules that apply in the world of covert policing. [. . .] In our research, we set out to redress this imbalance by conducting the first comprehensive ethnographic field study of covert police investigation in the UK.

(Mac Giollabhuí et al. 2016:642)

A closely related rationale can stem from what are regarded as erroneous or misleading ideas about some topic in the research literature, or in the mass media, as in the case of Chege’s research on ‘beach boys’ on the Kenyan coast:

Paucity of academic reflections on the subject was complemented by a tendency to simplify the phenomenon. This is exemplified in an article by a Kenyan academic, who noted that ‘Female tourists are also coming to Kenya to meet with the local beach boys

1 See Glaser and Strauss’s (1967) *The Discovery of Grounded Theory*. For helpful secondary accounts of grounded theorizing, see Dey 2004 and Pidgeon and Henwood 2004. See also Bryant and Charmaz 2007.

and promoting male prostitution’ (Omondi, 2003: 5). Prostitution being a morally charged word and the view that this was but another form of prostitution, and that the Kenyan men involved were being lured into it by female tourists, left room for me to engage with the complexities of the phenomenon in question.

(Chege 2015:466–7)

Or it may simply be that previous work has omitted one or more important aspects of what is being investigated. For example, in introducing a study of nursing homes in the United States, Rodriquez writes that:

There have been a handful of exemplary ethnographies of nursing homes that have given voice to nursing assistants and sought to highlight the ways that they manage the strains of their work within [. . .] bureaucratic and hierarchical structures [. . .]. As important as these studies have been, they have had a tendency to overlook all the other nursing home care workers. These individuals include dietary aides, activities assistants, rehabilitation aides, social workers, and maintenance staff. Little is known about these workers, but all of them are important to a functioning nursing home. This book integrates their experiences to provide a fuller portrait of nursing homes as workplaces.

(Rodriquez 2014:5)

Along similar lines Mount (2018:60) writes that ‘ethnographers have amassed a large body of literature on exotic dance and strip clubs. However, this literature contains little information about strip club managers [the focus of her investigation] and the work they perform “behind the curtain”.’

Alternatively, the stimulus for a study may be a surprising fact or set of facts. Thus, Measor (1983) noted that not only did girls tend to fare worse than boys in science examinations in the UK, but that the gap was even greater in the case of Nuffield science, a course emphasizing discovery learning. She set out to discover why this was, through participant observation in Nuffield science lessons and by interviewing both boys and girls about their attitudes to these lessons.

Social events themselves may sometimes stimulate research, providing an opportunity to explore some unusual occurrence or to test an explanatory idea. An example is the Challenger explosion, which prompted Vaughan’s (2016:xi) ‘historical ethnography’ of NASA:

On January 28, 1986, I was one of the millions who heard the news and then watched, riveted, as, over and over on TV, NASA’s Space Shuttle *Challenger* rose from the launch pad, then turned, and at the words ‘*Challenger*, go at throttle up’ exploded, pieces raining down into the ocean. [. . .] In an urgent teleconference on the eve of the Challenger launch, the engineers who worked closely on the Solid Rocket Boosters had protested against launching, given the unprecedented cold overnight temperatures predicted for Florida, but managers overrode their protests and proceeded, with disastrous consequences. More startling still, [an] investigation revealed the shuttle had been flying with damage to the O-rings since the first shuttle mission in 1981. [. . .] I began this research with the hypothesis that the Challenger accident was not caused by amorally calculating individual managers, as the report suggested, but was an example of organizational misconduct.

Another example of a study prompted by an unanticipated public event would be Gould-Wartofsky’s (2015) investigation of an anti-capitalist movement in New York. The aftermath

of natural disasters can also be studied to explore what is revealed about the societies in which they occurred: for example, Simpson (2013) investigated the political conflicts that followed an earthquake in Gujarat. A very different example of a study of a particular event is Chen's (2018) research on 'Burning Man', a temporary city created annually in the Black Rock Desert of north-west Nevada, an experimental community designed to facilitate both artistic self-expression and communal cooperation. Similarly, organizational innovations can serve as 'natural experiments', revealing processes that go on under the surface of social life, or showing what happens when the limiting factors constraining normality in a particular context are breached. At such times, social phenomena that are otherwise taken for granted may become visibly problematic for the participants, and thus for the observer. Such 'critical' events or incidents can be a productive focus for research (Sewell 2005; Berezin 2012; Dooremalen 2017).

Sometimes unanticipated events occur during the course of a study (see Meinert and Kapferer 2015), leading to its reorientation. For example, Pieke (1995) recounts how, five months into his fieldwork in Beijing, he witnessed the emergence of the 1989 Chinese People's Movement. Even in its initial stages, this clearly represented an important research opportunity to understand the dynamics of Chinese society and the nature of the Communist Party's rule there.

In addition, ethnographers occasionally focus on scheduled events of various kinds. Thus, Delgado and Cruz (2014) argue for 'multi-event ethnography', in which there is a focus on networks of events that play a crucial role in the development of the field being investigated, illustrating their argument from research on two international meetings concerned with the negotiation of legal instruments to regulate biodiversity exploitation. And, occasionally, ethnographers have themselves organized events for the purposes of research, as with Goldstein's (2017:185) 'staged encounter': a 'seminar' with market vendors in the Bolivian city of Cochabamba about the insecurity they face as a result of criminal activity. This was designed to establish 'a space in which the aspirational expressions of an imagined policing could emerge' (p200).

Even chance personal encounters can provide motive and opportunity for research. Sanders (2009) reports how, finding himself 'in San Francisco for a week with few obligations', he 'looked up the listings in Yellow Pages under "Museums"' and eventually found himself 'climbing the dingy stairs leading to the Tattoo Art Museum'. He was fascinated, got a tattoo himself, and ended up carrying out ethnographic work on various aspects of tattooing. Meanwhile, Henslin came to do research on the homeless as a result of meeting someone for whom the problem of homelessness had become a consuming passion:

When [he] found out that I was a sociologist and that I was writing a textbook on social problems, he asked me to collaborate on a book about the homeless. He felt that my background might provide an organizing framework that would help sort out his many experiences and observations into a unified whole. During our attempt at collaboration, he kept insisting that as a sociologist I owed it to myself to gain first-hand experience with the homeless. Although I found that idea somewhat appealing, because of my heavy involvement in writing projects I did not care to pursue the possibility. As he constantly brought up the topic, however, I must admit that he touched a sensitive spot, rubbing in more than a little sociological guilt. After all, I was an instructor in social problems, and I did not really know about the homeless [. . .] With the continued onslaught, I became more open to the idea. (Or perhaps I should say that I eventually

wore down.) When he invited me on an expense-paid trip to Washington, DC, and promised that I would see sights hitherto unbeknownst to me – such as homeless people sleeping on the sidewalks in full view of the White House – firing my imagination, he had pierced my armor through. With the allure of such an intriguing juxtaposition of power and powerlessness, of wealth and poverty, how could I resist such an offer?  
(Henslin 1990:52)

By contrast, Currer (1992:4–5) began her research on Pathan mothers in Britain as a result of her own experience as an English mother in Peshawar, Pakistan. Her research questions arose initially from what she saw as the parallels between her former position and that of the people she chose to study, and from her sympathy for them. Research may also be stimulated by personal interest in some form of activity, though Rossing and Scott (2016) note that studying ‘something you love’ may ‘take the fun out of it’. Of course, research interest may equally arise from conflict and negative feelings. Van Maanen (1991:33) reports that his long career investigating police culture began in part because he had been ‘subject to what I regarded as more than my fair share of police attention and hence viewed the police with a little loathing, some fear, and considerable curiosity’.

It is also common for research to be stimulated by previous experience in temporary or permanent jobs. Everett Hughes, an influential practitioner and teacher of ethnography, encouraged his students in this, resulting, for example, in Becker’s (1963) study of jazz musicians and Davis’s (1959) account of taxi-driving. A more recent example is the work of Leigh (2014) who, as a social worker in the field of child protection, decided to carry out research on this form of professional work, comparing its practice in a UK setting with the rather different character it took in Belgium.

Up to now, we have assumed that ethnographers are relatively free to decide for themselves what to investigate, but of course it is increasingly true that they must select topics for study, at least in part, according to what is most likely to gain funding, or what is in line with the strategic priorities of the institutions in which they study or work. Indeed, they may be specifically recruited to investigate a particular issue; though, even here, there is usually some leeway for re-making rather than simply ‘taking’ the research focus as defined by funders or research directors.

Whatever the origins of the inquiry, there is always a need to work the initial research problem up into a worthwhile and viable form. In this, previous academic research on the topic is particularly important, since the aim of research is not simply to satisfy one’s own curiosity but also to contribute to a developing body of knowledge about social phenomena. To some degree, this task of formulating research questions is carried out over the course of the whole project, but reflection and reading in the early stages are advisable, as well as recurrent attempts to outline the focus – as currently conceived. While there are no hard-and-fast rules for deciding how far initial ideas can be clarified and elaborated before the collection of data begins, exploring these ideas and their implications with the help of whatever secondary literature is available is wise. Relevant resources here are not only research monographs and journal articles but also official reports, journalistic exposés, autobiographies, diaries, and even novels or short stories (see Chapter 6). It may also be possible to examine data relating to previous studies available in archives (Corti 2011, 2016).

### **The development of research problems**

So, a key aim in the pre-fieldwork phase, and one of the tasks in the early stages of data collection too, is to turn foreshadowed problems into a set of questions to which answers could

be given, whether these are to consist of narrative descriptions of unusual or typical sequences of events, idiographic or generalized accounts of the perspectives and practices of particular people, or more abstract theoretical formulations. Sometimes, in this process of formulation and reformulation, the original problems are transformed, or even completely abandoned in favour of others. To take an extreme example, one study began from an interest in the organization of a housing association, how it had been established and had changed in character over time, then shifted to focus on the conflicting stories that members of the association told about its history, before being finally transformed into an investigation of the ‘confused’ talk of people suffering from dementia (Shakespeare 1997). Stuart (2018) provides an example of a less dramatic and therefore more common sort of change, initially setting out to investigate how sellers of goods on Skid Row informally maintain social order, and particularly the economic aspects of this, but later giving primary attention to the role of the police and the way in which people on the street anticipated and thereby sought to avoid arrest.

Change in research problems can derive from several sources. We have already noted that new, more important or more appealing, possibilities can arise during the course of investigation. Furthermore, Alvesson and Sandberg (2013) have argued that researchers should set out to challenge ideas derived from the literature, rather than simply looking for gaps that need to be filled. Another reason why research questions may change is that, in the course of inquiry, it may be discovered that the original formulation was founded on mistaken assumptions. Or there could be external factors that bring about change. The latter is what happened in the case of Shakespeare (1997), just mentioned, where an alteration in the circumstances of the researcher – recruitment to a new job – gave rise to new interests and opened up new possibilities. At the same time, the new research question was linked to the earlier phase of investigation: given that all of us may sometimes talk in apparently confused ways, she came to wonder what is distinctive about the talk of those suffering from dementia?

Another reason for change is that it may be concluded that the initial problem selected is not open to effective investigation, given the current state of knowledge or the time and resources available, or because of other barriers. What are viable research problems depends on the resources available to the researcher at the time; and what are relevant here are not just external resources but also personal ones such as background knowledge and skills, emotional capacity for dealing with particular types of issue, social characteristics and past experience. This importance of feasibility in the selection of research problems arises across the whole field of human inquiry. Medawar comments, in relation to natural science:

Good scientists study the more important problems they think they can solve. It is, after all, their professional business to solve problems, not merely to grapple with them. The spectacle of a scientist locked in combat with the forces of ignorance is not an inspiring one if, in the outcome, the scientist is routed. That is why some of the most important biological problems have not yet appeared on the agenda of practical research.

(Medawar 1967:7)

Recurrently, researchers rediscover the truth of the old adage that finding the right question to ask is at least as important, and sometimes more difficult, than answering it (Merton 1959).

Much of the effort that goes into the initial stages of ethnographic work is concerned, then, with formulating and reformulating research problems in ways that make them more fruitful and/or more amenable to investigation. Problems vary in their degree of abstractness. Some, especially those deriving from practical or political concerns, will

be 'topical' (Lofland 1976), focusing on types of people and situations readily identified in everyday language. Others have a more 'generic' cast. Here the researcher is asking questions such as 'Of what abstract sociologically conceived class of situation is this particular one an instance?' and 'What are the general features of this kind of situation?' This distinction between topical and generic research problems is closely related to that between substantive and formal analyses outlined by Glaser and Strauss in their account of grounded theorising:

By substantive theory, we mean that developed for a substantive, or empirical, area of sociological inquiry, such as patient care, race relations, professional education, delinquency, or research organizations. By formal theory, we mean that developed for a formal, or conceptual, area of sociological inquiry, such as stigma, deviant behavior, formal organization, socialization, status incongruity, authority and power, reward systems, or social mobility.

(Glaser and Strauss 1967:32)

In ethnographic research there is usually a constant interplay between the topical and the generic, or the substantive and the formal. One may, for instance, begin with some formal analytic notion and seek to extend or refine its range of application in the context of a new substantive application. An example is the way in which the labelling theory of deviance was extended to the study of social problems (Holstein and Miller 1993; Weinberg 2009). Labelling theorists had argued that rather than focusing, as criminology had previously done, entirely on the causes or processes that lead people to engage in crime or deviance, there should also be attention to how particular kinds of activity come to be defined as criminal or deviant, including differences between societies in this; to the effects of labelling and punishment, notably whether these dissuade or encourage future deviance, and to how particular types of people are more likely to be prosecuted by the police and convicted by legal processes, and what the effects of this are. Similarly, the study of social problems had previously been preoccupied with how such problems arise and who is responsible: why there is poverty among some groups, what are the causes of drug addiction or environmental pollution, etc. The extension of labelling theory to this field involved a shift to the investigation of how particular phenomena come to be defined as social problems requiring action, how efforts are made to move problems up the public agenda or to resist this, how issues may be reframed in new terms (for example, the reformulation of 'prostitution' as a type of 'sex work'). It also opened up investigation of the effects of particular outcomes or activities coming to be defined as social problems. Subsequently, the implications of these generic ideas have been explored in relation to a wide range of topics, and in the process have themselves been refined and developed.

Just as one can formulate research problems by moving from the formal to the substantive, so it is possible to move from the substantive to the formal or generic. This can be illustrated in part from a research project in which one of us was involved (Atkinson 1981). It was concerned with the investigation of 'industrial training units' designed to ease the transition from school to working life for 'slow learners'. The research began with foreshadowed problems that were primarily substantive or topical in origin. In an exploratory orientation, the research team started the fieldwork phase with general interests of this sort: How is the day-to-day work of the unit organized? How are the students selected and evaluated? What sort of work do they do, and what sort of work are they being prepared for? At the same time, it became



apparent that there was a need to reformulate these ideas in terms that were more general than their local manifestations in this project. A research memorandum was produced that indicated some of the more generic or formal categories that might prove fruitful, and how these could be linked back to more topical issues. Among the categories mentioned was the notion of ‘gatekeepers’:

By gatekeepers I mean actors with control over key sources and avenues of opportunity. Such gatekeepers exercise control at and during key phases of the youngster’s status passage(s). Such gatekeepers’ functions would actually be carried out by different personnel in the different organizational settings [. . .]

The identification of the general class of ‘gatekeepers’ would then allow us to go on to ask some pertinent questions of a general nature. For instance: What resources do gatekeepers have at their disposal? What perceptions and expectations do gatekeepers have of ‘clients’? Are these perceptions mutually compatible or are there systematic differences of opinion? Do gatekeepers believe that their expectations of clients are met or not? Do they have an implicit (or even an explicit) model of the ‘ideal client’?

What is the information-state of gatekeepers? For example, what sort of model of the labour market are they operating with? What views of working life do they bring to bear? How accurate are their assessments of the state of local labour markets?

What sort of routines and strategies do gatekeepers employ? For instance, what criteria (formal and informal) are used to assess and categorize ‘clients’? What bureaucratic routines are used (if any)? What record-keeping procedures are used, and how are such data interpreted in practice?

(Atkinson 1981)

As this illustrates, one must beware of over-simplifying the distinction between topical and generic levels of analysis, and it is rare for there to be progress in a uni-directional way from one to the other: there will normally be a constant shuttling back and forth between the two analytic modes. Particular substantive issues may suggest affinities with some formal concept that will, in turn, indicate substantive issues as deserving new or further attention, and so on.

### Selecting settings and cases

There is another aspect of research design that plays a significant role in shaping the way in which research problems are developed in ethnography, though it is also important in its own terms: the nature of the setting, or settings, chosen for investigation. Sometimes the setting comes first – an opportunity arises to investigate an interesting situation or group of people, and foreshadowed problems spring from the nature of the setting. This is true, for example, in the case of studying ‘critical events’ or ‘natural experiments’, and when ethnographers are specifically employed to investigate particular settings, or where they are carrying out ethnography in their own communities or places of work.

In the case of contract research, what is to be studied, and perhaps even to some extent how this is to be done, is determined by the contract. An interesting example is a study carried out by Viegas (2009), which was designed to support a formal indigenous land claim by the Tupinambá people living in Brazil. One requirement was that the focus of inquiry be on the *whole* of the people concerned and the land they were claiming, whereas her previous research had (in the manner of much anthropology) studied a small group of these people

in one area in depth. In addition, in the new study there were external constraints on the research design:

the fieldwork [. . .] was ‘official’, which meant that (1) a government decree had to be published, giving the exact time that the anthropologist would arrive and leave the region of fieldwork, and (2) until the research was handed in, the anthropologist was not to visit the area or carry out fieldwork without the knowledge and permission of FUNAI [the department of the Brazilian Ministry of Justice that deals with indigenous affairs], who would send a person to accompany the anthropologist during any visit he or she might choose to make.

(Viegas 2009:149)

This restricted both the frequency and the length of the visits to the field that could be made by the anthropologist, and the presence of an official ‘chaperone’ no doubt shaped the character of the data that could be collected too. Above all, though, the focus and indeed specifications of the research were determined by what was laid down as required to support a land claim. Duck (2015) provides an example of the reverse sequence. He was asked to collect evidence about mitigating circumstances for a federal death penalty case in the United States, one which involved a neighbourhood drug dealer living in an impoverished, predominantly black area. He did this, but then went on to carry out a 10-year ethnographic study of that area.

Even where a setting is selected on the basis of foreshadowed problems, the nature of the setting may still subsequently shape the development of research questions. This arises because, as we noted earlier, in ethnographic research the development of these is rarely completed before fieldwork begins; indeed, it may extend not only through the data collection process but even into the stage of writing up. It is often found that some of the questions into which the foreshadowed problems have been developed are not open to investigation in the setting selected, or the data may throw doubt on some of the assumptions on which they rely, or on their importance. The researcher may be faced with the choice of either dropping the initial questions from the investigation or re-starting the research in a setting where they *can* be studied, if that is possible. While, on occasion, the importance of a problem may lead to the latter decision, generally researchers stay where they are and tailor their research questions to what can be investigated there. After all, more questions are always generated than can be tackled in a single study, so that some selection is inevitable. Moreover, not only does moving to another setting involve further delay, and perhaps renewed problems of access, but there is also no guarantee that the new setting will turn out to be suitable when more is known about it. Everett Hughes is reported to have remarked, only half jokingly, that the researcher should select the research problem for which the setting chosen is the ideal site!

It is also worth remembering that, often, the ethnographer is not able to specify in very precise terms the type of setting required to investigate a particular set of questions. Nor does he or she have an entirely free hand in where a study will be carried out. Initially, at best, what can be done is to identify the *sort* of location that would be most appropriate for investigation of the research problem, as currently formulated. And, when a type of setting has been determined, it is advisable (if possible) to ‘case’ possible research sites with a view to assessing their suitability, the feasibility of carrying out research there, and how access might best be accomplished should they be selected. This involves collecting and subjecting to preliminary analysis any documentary evidence available about the setting, interviewing anyone who can be easily contacted who has experience or knowledge of it, and perhaps making brief visits

there, covertly or overtly. It may be possible to combine this with carrying out pilot research, trying out some of the methods that one plans to use; though sometimes it is better to employ a setting for this purpose that one knows will *not* be employed in the main data collection.<sup>2</sup>

‘Casing the joint’ in this fashion will not only provide information about one or more potential settings in which the research could be carried out, but also feed into the development and refinement of the research questions. It may be discovered that what had been assumed to be a homogeneous category of sites must be broken down into a number of subtypes that have significantly different characteristics. For example, in her study of strip clubs in upstate New York, Mount came to recognize that the clubs fell into two broad types, varying significantly in the role of the management, the typical clientele, and in the attitudes of the women working there. She writes that:

Hustle clubs are characterized by a high turnover of dancers, large volumes of customers from diverse social classes, and few “regular” customers [in addition] management is detached from dancers, managing conflict between [them] only if it becomes a major issue [whereas] social clubs have fewer customers, a relatively steady group of dancers, and many “regular” customers, most of whom are middle-aged, working-class men [. . .]. Management in social clubs have personal contact and negotiations with dancers [. . .].  
(Mount 2018:68)

Furthermore, differences between sites in which similar activities occur can offer opportunities for a deeper understanding of those activities. For example, in her study of prostitution in Mumbai, Shah (2014:3) studied three distinct types of setting where

women who have migrated to [Mumbai] from impoverished rural areas within India negotiate selling sexual services [. . .]: brothels, streets, and public day wage labour markets [. . .], known as *nakas*, where [by contrast with the other two contexts] sexual commerce is solicited discreetly, or alongside other income-generating activities.  
(Shah 2014:3)

The role of pragmatic considerations must not be under-estimated in the choice of a setting. While by no means absent in hypothesis-testing research, such matters are likely to play an especially important role in ethnographic work. This is because here the criteria specifying suitability are usually much less determinate: there are often many settings that *could* be relevant in investigating a foreshadowed problem. Furthermore, the data collection process is usually very intensive, and this has implications for what is and is not going to be viable. As a result, contacts with personnel that promise to ease access, the scale of the travel and/or accommodation costs likely to be involved, and the availability of documentary information, etc. are often major considerations in narrowing down the selection of where to carry out the investigation. Sampson and Thomas illustrate this with reference to a distinctive consideration arising in their ethnographic work on board ships. They write:

In the course of our research we deliberately selected a high proportion of ships with long sea passages and few port calls. This was the result of our finding that seafarers tend to have little free time in port and some, for example chief officers, have many

2 For an illuminating discussion of the role of pilot research in qualitative research generally, see Sampson 2004.

demands made on their time in port and therefore become very fatigued. Seafarers with time 'off' generally go ashore and may be unavailable to researchers who may find that research opportunities are limited aboard ships engaged in what are termed short-sea trades, where port calls are very frequent and close together. Our choice of ship was thus driven by our assessment of the optimum conditions for data collection [ . . . ].

(Sampson and Thomas 2003:170)

Sometimes, the search for an appropriate setting can take unpredictable turns, as Campbell's account of his research in Greece in the 1950s illustrates. He set out to study one of the villages in a mountain region north-east of Jannina. However, he found the populations of the villages much depleted as a result of civil war, and that his English background led to suspicions that he was a spy. A fortuitous event transformed his research plans. Sarakatsan transhumant shepherds lived on the hills above the village, and relations between them and the villagers were uneasy:

Our own contacts with them had not gone beyond formal greetings when one day in the heat of summer a young shepherd-boy returning from school had stopped at the village spring to drink, and was there set upon by larger village boys [ . . . ]. At this point, the anthropologist's wife entered indignantly to rescue the victim. This small adventure had its consequences. We received an invitation to visit a Sarakatsan encampment and the relationship prospered. When some weeks afterwards the time arrived for the Sarakatsani to take their flocks and families down to the plains of Thesprotia for the winter, one family sent us a peremptory message. We were to accompany them and they would build us a hut.

(Campbell 1992:152)

This example also illustrates how occasionally researchers find that they have effectively been chosen to research a setting by one or more of the people involved in it, though usually with rather more strings attached than in this case. In such circumstances, the ethnographer must balance the ease of initial access offered against the desirability of the site in other respects, as well as against any problems that such direct sponsorship might cause.

Generally, ethnographers have studied only a single setting, or a very small number of them. Sometimes, the focus has even been on single individuals, as with Harper's (1987, 2018) study of Willie, a mechanic in a remote area of New York State who fixed cars, agricultural equipment, and other kinds of machinery. However, recognition of the effects of globalization, in linking places that are geographically remote from one another, has led to increasing emphasis on multi-site ethnographies (Marcus 1995; Hannerz 2003; Nadai and Maeder 2005), and on the need for an approach that takes account of the highly mobile lives of some people in the contemporary world. An example of multi-site ethnography is Burawoy et al.'s (2000) *Global Ethnography*, in which the researchers investigated how people in different localities adapt to and resist the pressures of globalization, focusing on very different settings in several countries. Meanwhile, Trouille and Tavory (2016) point to the value of shadowing participants (accompanying them as they move through the various contexts in which they participate), illustrating this by reference to a study of informal networks among immigrant Latino men who met to socialize and play soccer in a Los Angeles park.<sup>3</sup> In the

3 Shadowing can use other means of transport than walking – see Wegerif 2019.

case of online ethnography (discussed in more detail in Chapter 7), it is particularly easy to study multiple sites, though it is not always possible to identify individuals participating across different ones.

Another reason for studying several sites, or for shadowing participants, relates to a concern with identifying what is and is not typical, and also with using cases that contrast in relevant respects in order to develop theoretical ideas. Thus, Henslin decided to carry out a study that covered various parts of the United States, and found a relatively cheap and convenient way of doing this:

I heard about a ‘fly-anywhere-we-fly-as-often-as-you-want-for-21-days’ sales gimmick from Eastern Airlines. I found their offer was legitimate, that for \$750 I could pack in as many cities as I could stand – actually more than I could stand as it turned out [. . .] It was the method itself, participant observation, that became the key for making this research affordable. Obviously, the homeless spend very little money, which dovetailed perfectly with my situation and desires. I was able to stay in the shelters at no financial cost. (The shelters, however, exacted a tremendous cost in terms of upsetting my basic orientational complacencies.) In addition to a free bed and a shower, the shelters usually provided morning and evening meals. Although those meals were not always edible, I was able to count on the noon meal being of quality, and that was already included in the price of my airline ticket [. . .] I primarily focused on major cities in the Western part of the United States, later adding cities in other areas during subsequent travels. My purpose was to obtain as good a ‘geographical spread’ as I could.

(Henslin 1990:55)

There is, perhaps, a need to express caution about multi-site strategies: the more sites are investigated (assuming a fixed level of resources) or the more ‘mobile’ the research, the less possible it will be to study any one site in depth. Hammoudi comments that: ‘*Do not linger* seems to be the motto or, if you insist, *hang out* for a moment—and remember, everybody you happen to cross paths with is on the move. So, move on!’ (2009:25; emphases in original). He goes on to illustrate the need for careful, in-depth investigation of particular places. The key point is that there is a trade-off between breadth and depth of investigation (see also Harper 2018:97–8, 110). Of course, where a team of researchers investigates different settings, as in the case of Burawoy and his students, this is less of a problem than in that of a lone ethnographer, such as Henslin.

### Settings and cases

It is important not to confuse the choice of settings with the selection of *cases* for study, since they may be different. There can be a tendency to overlook the distinction because settings are sometimes treated as objects that simply exist in their own right. Thus, Chicago School sociologists in the 1920s and ’30s studied various parts of their city, treating them as ‘natural areas’ produced by the evolution of modern society (see Rock 1979:92). In other sociological contexts, similar appeals have been made to models of relatively self-contained groups or ‘communities’. In the past, the anthropological tradition tended to lay stress on the investigation of small-scale ‘face-to-face’ societies and local collectivities (such as ‘the village’). This, and the cognate tradition of ‘community studies’, has often rested on a *Gemeinschaft*-like view of local society, emphasizing its internal stability and its relative discreteness and distinctive culture. The more recent concept of ‘communities of practice’ (see Lave and Wenger 1991),

which was originally developed through an anthropological study of Liberian tailors, may also encourage this tendency. However, it is necessary to remember that, in an important sense, settings are not naturally occurring phenomena: they are constituted and maintained through ongoing cultural definition and social strategies. Furthermore, their boundaries are not fixed but shift across occasions, to one degree or another, through processes of redefinition and negotiation – so they are, at best, fuzzy (Nadai and Maeder 2005). There is also the fact that, given the way in which mobile digital technology is now integrated into the lives of many people today, the settings in which people interact are ‘networked’ with others, intermittently if not continuously.

There is another reason, too, why it can be misleading to talk of ‘studying a setting’. This is that it is not possible to give an exhaustive account of any locale. In producing descriptions, we always rely on criteria of selection and inference. There is an important sense, then, in which, even in the most descriptively oriented study, the case investigated is not isomorphic with the setting in which it is located. Settings are contexts in which many phenomena occur that might be studied from any number of angles; whereas a case is a set of phenomena viewed from one particular angle, this constituted by a set of research questions. Thus, in any investigation, some features of the setting will be given no attention at all, while parts of its wider context are; and even those phenomena that are the major focus may be looked at in ways that by no means exhaust their characteristics.

It is also worth noting that cases can vary considerably in scale. At one extreme, the case examined may be a piece of interaction that is quite local and short in duration, as for example with Lake et al.’s (2015:64) ‘account of one nurse with one patient for a few moments of her day’, the data relating to a 10–15 minute interaction between a nurse and a man with diabetes whose foot had become severely infected. At the other extreme, the case investigated may involve many settings and take place over an extended period of time, perhaps even years, as for example with Vaughan’s (1996, 2004) study of NASA.

Moreover, any setting may contain several cases relevant to a study. Thus, for example, in an investigation of the effects of various kinds of external assessment on secondary school teaching, it was particular courses taught within the schools that constituted the cases under investigation, rather than the schools themselves (Scarth and Hammersley 1988). Conversely, a case may not be contained within the boundaries of a setting, so that it may be necessary to go outside to collect information on important aspects of it, as Ocejo (2014:ix) found researching a bar in New York. He writes:

I studied Milano’s over the next couple of years. I hung out at the bar often and got to know its regular customers and bartenders well. I also met dozens of patrons whom I never saw again [ . . . ]. But the more I focused on the people and activities inside the bar, the more I was drawn towards its outside urban context. I identified three types of customer: older men who lived on the nearby Bowery, the city’s most notorious skid-row district, and had been coming to the bar for many years; a group of younger regular customers, many of whom had moved to or often visited the neighborhood early on in its gentrification; and even younger visitors, in their twenties and thirties, who had either recently moved to or lived outside the neighborhood and visited the bar infrequently.

Jacobs (1974) found much the same in studying gangs among male prisoners: exploring their links with the outside was essential in order to understand how gangs within the prison came to be formed and how they continued to recruit new members.

While it may seem innocent enough, then, the naturalistic conception of studying fields and settings may discourage the systematic and explicit selection of aspects of a setting for study, as well as movement outside of it to follow up promising theoretical leads. It can also result in neglect of the processes by which settings are established and maintained, and of communicational and other links among settings, including the ways in which the ‘actual’ and the ‘virtual’ can be interlaced. As this makes clear, the process of identifying and defining the case(s) under study must proceed side by side with refinement of the research problem and development of the analysis.

### Cases and generalization

One of the limitations often raised in connection with ethnographic work is that because, usually, only a small number of cases are studied, perhaps just one, the ‘representativeness’ of the findings is always in doubt. This is a common criticism, coming, for example, from those whose model of social research is the social survey (Small 2009). In response, many ethnographers, and other qualitative researchers, have denied that their aim is generalization. Lincoln and Guba (1985:110) famously declared that ‘the only generalization is: there is no generalization’, while (along similar lines) Abu-Lughod (2006:475) has challenged anthropologists’ and others’ use of the concept of culture on the grounds that it involves reification, proposing instead that anthropology should carry out ‘ethnographies of the particular’.

However, in practice, most of the time ethnographers *do* engage in generalization, implicitly if not explicitly (Hammersley 1992:ch5). Occasionally, ethnographic research is concerned with a case that has *intrinsic* interest, so that generalization is not the primary concern. This is true in some evaluation studies, where the target is the operation of a program or policy in a particular situation (Patton 2015). It also holds where the setting to be investigated is unique, widely known, and/or powerful, as in Crewe’s (2005, 2015, 2016) studies of the UK Parliament. However, generally speaking, ethnographers study cases in settings that do not have these characteristics. Furthermore, they usually anonymize them, which must mean that their aim is to produce general knowledge.

So, generalization of some sort is usually part of the goal of ethnographic research. Occasionally, a relatively large number of cases is studied, thereby possibly providing a substantial basis for generalization to a population. Thus, Strong (2001) investigated a thousand cases of paediatric consultation in three hospitals, two in the UK and one in the US. But this is unusual, so that the question of the validity of any generalization produced often arises more sharply. At the same time, various strategies can be employed to deal with the problem, more or less adequately (Small 2009; Maxwell and Chmiel 2014; Emmel 2013; Rapley 2014). And it should be remembered that no type of research deals perfectly with issues of generalizability: social surveys are hampered by high levels of non-response, and experiments face challenges regarding their ecological validity. Critics of ethnography for its ‘lack of generalizability’ often seem to forget this.

How the problem of generalization should be dealt with in ethnography, as elsewhere, depends upon the aim. Is it to generalize to a finite population of cases, actually existing or possible in the future? Or, rather, to develop and/or test a theory, which necessarily assumes a potentially infinite domain of relevant cases that cannot be sampled in a way that ensures representativeness, even in principle? Both are legitimate.

Schofield (1990) has outlined some of the strategies that can be employed in qualitative research where the aim is generalization to a finite population. In the process, she draws important distinctions between generalizing to an existing population (for example, seeking

to identify what is representative of that population), studying what seems likely to become typical in the future (for instance by focusing on cases that are the leading edge of a current change), and investigating what *could* become typical (for example, investigating new developments that might spread). At the same time, she notes the complexities of such generalization (in particular, what is typical on one dimension may not be typical on relevant others). She also highlights the role of multi-site studies in providing a basis for this kind of generalization.

It is worth emphasizing that it may be possible to assess the typicality of the case or cases investigated by comparing their relevant characteristics with information about the target population, if this is available in official statistics or from other studies. Thus, in his investigation of religious intermarriage in Northern Ireland, Lee sought to check the representativeness of his snowball sample of couples by comparing some of their characteristics with a special tabulation of the census data. This revealed that his sample ‘showed a sharp bias towards young, recently married couples, mostly without children and with relatively high levels of educational attainment’ (Lee 1992:133). While he was not able to correct this sampling bias, because of the problem of gaining access to couples whose position was so delicate in the Northern Ireland situation, he could allow for it in his analysis.

It may even sometimes be possible to carry out a small-scale survey on a sample of the population to gather information in order to assess the typicality of the cases being studied ethnographically. Thus, in his investigation of students at Rutgers University, Moffatt used a survey to assess the extent to which they had a vocational orientation, and he was also able to compare the results with those from a national study (Moffatt 1989:331). Another possibility is to combine in-depth investigation of a small number of cases with more superficial checks on other cases. For example, in his study of law enforcement agencies, Skolnick (1966) concentrated on one city, but he made a brief investigation of agencies in another city to check the likely generalizability of his findings. On a smaller scale, McDermott (2018) compared situations systematically to detect factors that allowed or prompted the expression of racial prejudice. None of these strategies can usually provide absolutely conclusive evidence, but they can give some indication of how likely it is that the findings will be true of the population: this is analogous to what has been referred to as naturalistic or *moderatum* generalization (Payne and Williams 2005; see also Larsson 2009).

Where the concern is theory development and testing, the strategic selection of cases is central. This can take a variety of forms (Emmel 2013). One is what Glaser and Strauss (1967) called ‘theoretical sampling’. The primary concern of these authors was the generation and elaboration of theory, and they argued that the selection of cases should be designed to produce as many interrelated categories, and properties of categories, as possible, and also to facilitate the emergence of relations amongst categories. They recommended two complementary strategies: minimizing the differences between cases to highlight basic properties of a particular category; and then subsequently maximizing the differences between cases in order to increase the density of the properties relating to core categories, to integrate categories, and to delimit the scope of the theory. As an illustration, they cite their research on the awareness contexts surrounding patients dying in hospital:

Visits to the various medical services were scheduled as follows: I wished first to look at services that minimized patient awareness (and so first looked at a premature baby service and then a neurosurgical service where patients were frequently comatose). I wished next to look at dying in a situation where expectancy of staff and often of patients was great and dying was quick, so I observed on an Intensive Care Unit.



Then I wished to observe on a service where staff expectations of terminality were great but where the patient's might or might not be, and where dying tended to be slow. So I looked next at a cancer service. I wished then to look at conditions where death was unexpected and rapid, and so looked at an emergency service. While we were looking at some different types of services, we also observed the above types of services at other types of hospitals. So our scheduling of types of service was directed by a general conceptual scheme – which included hypotheses about awareness, expectedness and rate of dying – as well as by a developing conceptual structure including matters not at first envisioned. Sometimes we returned to services after the initial two or three or four weeks of continuous observation, in order to check upon items which needed checking or had been missed in the initial period.

(Glaser and Strauss 1967:59)

Strategic selection of cases of this kind can also be employed in *testing* theoretical ideas. Here the aim is to select cases for investigation that subject theories to a relatively severe test. An example is the sequence of studies of UK schools by Hargreaves, Lacey, and Ball (Hargreaves 1967; Lacey 1970; Ball 1981; see also Abraham 1989, 1995). They argue that the way in which schools differentiate students on academic and behavioural grounds – via streaming, tracking, banding, or setting – polarizes them into pro- and anti-school subcultures. These subcultures, in turn, shape students' behaviour inside and outside school, and affect their levels of academic achievement. This theory was initially developed and tested in examples of three types of secondary school. Moreover, in the case of one of these – the grammar school – because the students entering it had been strongly committed to school values at their junior schools, variables at the heart of competing explanations for the process of polarization – such as attitude towards school, aspects of home background, etc. – were partially controlled (Lacey 1976). And, in his study of Beachside Comprehensive, Ball examines the effects of a shift from banding to mixed-ability grouping within a single school (some factors thereby remaining constant), this change representing a weakening of differentiation. What we have in this sequence of studies is an illustration of the systematic use of comparative method to produce general, theoretical conclusions, in part focusing on 'crucial' or 'critical' cases.<sup>4</sup>

It is worth emphasizing that the appropriate strategy to adopt in selecting cases may vary over the course of a single piece of research. In the early phases, which relevant cases are chosen for investigation may not matter greatly. Later on, it can acquire considerable importance. This means that initial decisions may have to be revised. Klatch reports how in her research on women involved in right-wing political organizations, she began with 'a neat fourfold table comparing four organizations: two Old Right groups and two New Right groups; two "religious" and two "secular organizations"'. However, she soon faced problems. In particular, she discovered that:

the chosen organizations for my original design did not in fact divide along secular versus religious lines [. . .]. Furthermore, I noticed a general pattern developing between the 'homemaker' type of woman active in many religious/pro-family groups [. . .] and the 'professional' type of women active in the more secular conservative groups [. . .].

4 For further discussion of the process of theory development and testing involved in this sequence of studies, see Hammersley 1985. For a discussion of many of the issues in using case studies in comparative analysis, see Gomm et al. 2000; Gerring 2007a and b. We discuss comparative method further in Chapter 8.

The final design continued to rely on in-depth interviews, participant observation, and a textual analysis of right-wing literature, but I broadened the sample to include a much wider range of conservative groups in order to increase the variation among the female activists, thereby gaining a better understanding of the broader divisions within the Right.

(Klatch 1988:75)

This example reinforces the point that research design in ethnography, both as it relates to the selection of cases for study, and in other respects too, is a continual process. As a result, the match between research problems and cases selected must be frequently monitored.

### **Sampling within the case**

Selecting cases for investigation is not the only form of sampling involved in ethnography. Equally important, often, is sampling *within* cases. At least this is true where cases are not so small that they can be subjected to exhaustive investigation, as for example in Lake et al.'s (2015) study of a brief incident or perhaps in Strong's (2001) study of paediatric consultations. Usually, decisions must be made about where to observe and when, who to talk to and what to ask, as well as about what to record and how. In this process, we are not only deciding what is and is not relevant to the case under study but also sampling from the data available with a view to generalizing from part to whole. Very often this sampling is not the result of conscious deliberation – indeed, in the early stages it usually cannot be, both because of the still emerging sense of what case(s) are being investigated, and why, *and* because information is still being gathered about the setting(s) in which they occur. However, it is important to make any criteria employed as explicit and as systematic as possible, with the aim of ensuring that, at the end of the process, data about the case have been adequately sampled, and to indicate any inadvertent sampling that has taken place which could lead to false generalization within the case. There are three major dimensions along which sampling within cases occurs: time, people, and contexts.

#### ***Time***

In thinking about time we need to recognize, first of all, variation in the scheduling of research activity, which has at least three aspects: how long is spent in the field overall, how frequently sites are visited, and how long each visit lasts. Much ethnographic work today, by contrast with the norm in anthropology in the past, does not involve presence and participation in a single setting day-in-and-day-out for one or more years, but instead more specific and episodic bouts of data collection, sometimes over a relatively short period. Murphy illustrates one pattern from his study of elderly men who met up regularly in a corner donut shop:

For more than four years, I hung out at Steve's Donut Shop about three or four times per week for roughly one hour during the main phase of data collection. Fieldwork occurring outside of the donut shop was less regularly patterned but occurred with roughly the same frequency and duration.

(Murphy 2017:115)

The most extreme contrast as regards overall length of time in the field is between traditional anthropological fieldwork and what has been referred to as 'blitzkrieg', 'focused', or

‘rapid’ ethnography (Millen 2000; Knoblauch 2005; Wall 2015). Meanwhile, with respect to frequency and intensity of visits, Jeffrey and Troman (2004:538) have distinguished three different modes: ‘compressed’ (‘a short period of intense ethnographic research in which researchers inhabit a research site almost permanently for anything from a few days to a month’); ‘selective intermittent’ (where there are strategic short bouts of fieldwork over a lengthy period); and ‘recurrent’ (this involves bouts of fieldwork aimed at systematic coverage of some phase of activity).

The last of these modes indicates how the scheduling of the data collection must be tailored to how time structures, and is structured by, the constellation of activities taking place in a setting (Dalsgaard and Nielsen 2015). This *in situ* structuring can be complex, as Zerubavel has shown in the case of hospitals:

The list of sociological aspects of temporality which can be discussed within the context of hospital life is almost endless: the temporal structure of patients’ hospital careers; the relations between time and space; deadlines and strategies of beating the schedule; the temporal relations among the various hospital units; the impact of organizational time on hospital personnel’s life outside the hospital; and so on.

(Zerubavel 1979:xxi)

While time may seem a dimension of obvious importance in social life, it has often been neglected. Yet the scheduling of research activity must take account of temporal structures, given that attitudes and activities vary over time in ways that are highly significant for social theory (Dawson 2014; Dalsgaard and Nielsen 2015; Elliott et al. 2017). Indeed, not attending to these can lead to false conclusions. Berlak et al. (1975) provided an example of this many years ago from their research on English ‘progressive’ primary schools in the early 1970s. They note that much of the journalistic and educational literature about these schools gave the impression that children simply decided for themselves what they would do, when, and for how long. However, through intensive investigation – in Jeffrey and Troman’s terms, the ‘condensed’ mode – they discovered that the reality was somewhat different:

we observed thirty children on a Wednesday morning who, after a brief discussion with the teacher, went about their work individually: some began to work on ‘maths’, others to study spelling or to write original stories in much the way [that the literature describes]. We observed no teacher behavior on that morning which appeared to direct the children to what they were to do. It appeared that the children were pursuing their own interests. However, during the following days, we observed events and patterns which appeared to account for the behavior observed on that Wednesday morning. On the following Monday morning we observed Mr Thomas set work minimums in each subject for the week [. . .]. On the following Friday morning we saw him collect the children’s work ‘diaries’ where each child had recorded in detail the work he [or she] had completed during the week. Over the weekend, Mr Thomas and, as we were to later discover, sometimes the head, checked each record book and wrote comments in the diaries such as ‘good’, ‘more maths’, or the ominous ‘see me’.

(Berlak et al. 1975:218)

Franch and de Souza (2015) have provided a more recent analysis of the temporal contours of life in schools, in this case focusing on a high school in Brazil. Meanwhile, Tjora (2016)

has noted the significance of the temporal cycles characteristic of rock music festivals. In both cases there are clear implications for the time-scheduling of research in such situations.

We can think hypothetically about what would be required in order to avoid similar errors to that highlighted by the Berlaks in studying, say, the accident and emergency department of an urban general hospital. Any intensive study here would almost certainly reveal different patterns of work and activity according to the time of day or night, and according to the day of the week. The nature of the referrals and emergency presentations would vary too. Saturday nights would probably be characterized by very different rates and patterns of admission from Monday nights, and so on. Study of temporal patterns in this department would also relate to changing shifts of nursing staff, rotations among junior doctors, and so forth. Similar temporal complexities would also be found in many other settings: in factories, bars, cafés, prisons, and care homes, for example.

It should be apparent, therefore, that any attempt to ensure that all relevant types of people, actions, attitudes, and events are represented will have to be based on adequate coverage of temporal variation. Of course, it is generally impossible to conduct fieldwork round the clock, and some degree of time sampling must usually be attempted. It may be necessary to employ several of Jeffrey and Troman's different modes, taking account of what is known about temporal patterns in the field, and indeed thereby enabling further ones to be discovered.

Over and above the issue of establishing adequate coverage, the researcher will probably identify particularly salient periods and junctures: the change-over between shifts, for instance, might prove crucial in the organization of work and the sharing of information in some settings. Such crucial times should then come in for particular attention. Similar considerations to those outlined above will also apply to larger-scale temporal dimensions, such as seasonal or annual cycles, and patterns of recruitment of new cohorts; although overall constraints of time and resource will obviously prove limiting here.

There can also be 'critical incidents' (accidents, the outbreak of conflict, the arrival of a significant visitor, etc) that are not pre-scheduled, so that whether or not the ethnographer happens to be in the setting when they occur is happenstance. If they are witnessed, it may be possible to study them directly; but, if not, reliance has to be placed upon retrospective accounts from informants. In her study of social workers on a child protection team, Leigh (2014) was not present when one of her colleagues was suspended, but (contrary to official instructions) he rang her up to discuss what had happened.

Up to now we have referred primarily to issues relating to fieldwork in organizations and the like. But it should also be apparent that similar temporal considerations might apply to fieldwork in less formally defined settings. The patterns of urban life, 'relations in public', variability in the use of public settings, and patterns of deviant activity, all have temporal dimensions: the seasons, the days of the week, and the time of day or night all play their part. Likewise, it may be important to pay some attention to special occasions, such as seasonal festivals and carnivals, ceremonies, and rituals (Kapferer 2010; Symons 2016) – though it is as important to include what is routine as it is to observe the extraordinary.

At the same time, we must remember that any attempt to schedule research activity in this manner relies upon assumed knowledge about the temporal organization of the setting, and its success will depend upon how accurate this is. For this reason, waiting, hanging around, passing the time with people, etc., are often essential if genuine understanding is to be gained, as well as for both prudential and ethical reasons concerned with building relationships (see Mannay and Morgan 2015; Palmer et al. 2018; see Chapter 4).

## *People*

No setting will usually prove socially homogeneous in all relevant respects, and the adequate representation of the people involved in a particular case will normally require some sampling (unless the whole population of relevant actors can be studied in sufficient depth). This problem was noted earlier by Rodriquez (2014) in relation to nursing homes: some categories of staff who work there had been neglected in earlier studies.

The sampling of persons may sometimes be undertaken in terms of fairly standard ‘face-sheet’ demographic criteria. That is, depending on the particular context, one may sample persons by reference to gender, ‘race’, ethnicity, age, occupation, educational qualifications, and so on. Thus, in selecting people to interview in her study of black middle-class neighbourhoods, Pattillo-McCoy sought to represent different age groups and segments of the middle class, though she was aware that upwardly mobile young adults were under-represented in her sample because they had moved out of the neighbourhood (Pattillo-McCoy 1999: 222). However, these face-sheet categories are of importance only as they relate to the emerging analysis, to rival theories, or to ensuring representation in terms of some larger population; they will usually need to be complemented by other categories of more specific analytic relevance. Such emergent categories may be either ‘member-identified categories’ or ‘observer-identified categories’ (Lofland 1976; Lofland et al. 2006).

The term ‘member-identified categories’ refers to typifications that are employed by members themselves – that is, they are ‘folk’ categories identifying types of people. These are important, not least because they are often the basis on which people act, either as regards themselves or in relation to others. They may relate to institutionalized roles or they could be more informal differentiations. For instance, in her study of a women’s prison, Giallombardo (1966) documents the following collection of labels that the prisoners use: snitchers, inmate cops, and lieutenants; squares, jive bitches; rap buddies, homeys; connects, boosters; pinner; penitentiary turnouts, lesbians, femmes, stud broads, tricks, commissary hustlers, chippies, kick partners, cherries, punks, and turnabouts. Such labels are applied on the basis of ‘the mode of response exhibited by the inmate to the prison situation and the quality of the inmates’ interaction with other inmates and staff’, including styles of sexual orientation (Giallombardo 1966:270).

By contrast, ‘observer-identified categories’ are types constructed by the researcher, designed to capture variation amongst people that is relevant to the investigation, for instance picking out significant differences in the trajectory by which people arrive in a particular situation, or differences in adaptation. Very often these categories build on, but also develop and elaborate, distinctions made by participants themselves, as with Snow and Anderson’s (1993:ch2) complex typology of homeless people. Similarly, in a study of Chinese car factories, Zhang (2015) identifies a central division between core workers with long-term contracts who are paid relatively well, and a host of temporary workers on lower salaries, who do not have access to benefits and can be hired or fired on a daily basis. Then, within these categories, she makes further differentiations, for example highlighting the position of student interns who are taken on as temporary workers, paid below the minimum wage, offered no benefits, and have little prospect of their internships turning into permanent employment.

Both member-identified and observer-identified categories can be used as a basis for sampling within the case, with the aim of trying to ensure that all the kinds of variation among people likely to be relevant to an investigation are covered adequately. Of course, this process is also inextricably linked with the development of analytical ideas and of strategies for the collection of data.

### Contexts

Taking account of variations in context is as important as sampling across time and people. It is important, however, not to mistake spaces for places. Radice (2011:13) argues that what the ethnographer initially identifies as a distinctive, bounded area may not be treated as such by the people to be found within it. Indeed, ‘senses of place are contingent, emerging at different scales and in relation to specific fields of social action.’ In other words, they are more fluid and complex than they are sometimes taken to be. This echoes what we said earlier about the social construction of settings.

Within any setting, people may distinguish between various kinds of context that require different sorts of behaviour. Sometimes the boundaries around these are quite clearly marked, perhaps even policed or vigorously defended. This reflects the fact that spaces may be territories that are defined as belonging to some group or category of person, and (equally important) as excluding certain others. Just as countries have borders that can be difficult to cross, on a smaller scale there are armed camps and gated communities that are committed to keeping people out and/or in.

Of course, territories and their boundaries can be less obvious than this, but equally significant for people’s lives (Low 2017). As Sobh and Belk (2011) show in their study of the home life of women in Qatar, how places are socially defined and treated is expressive of culture; it can tell us a great deal about what people value and what they fear, how they are seen, and how they see themselves. In other words, places may carry symbolic meanings that relate to particular identities or relationships, religious or political beliefs, differences in social status, and so on. Furthermore, the same place can be occupied by more than one group, perhaps at different times of the day, who regard it very differently (Chevalier 2015).

There can also be functional differentiation and relationships between contexts, as is highlighted by Goffman’s famous distinction between frontstage and backstage regions:

A back region or backstage may be defined as a place, relative to a given performance, where the impression fostered by the performance is knowingly contradicted as a matter of course. [. . .] It is here that the capacity of a performance to express something beyond itself may be painstakingly fabricated; it is here that illusions and impressions are openly constructed. [. . .] Here costumes and other parts of personal front may be adjusted and scrutinized for flaws. Here the team can run through its performance, checking for offending expressions when no audience is present to be affronted by them; here poor members of a team, who are expressively inept, can be schooled or dropped from the performance. Here the performer can relax; he can drop his front, forgo speaking his lines, and step out of character.

(Goffman 1959:114–15)

Goffman illustrates his argument by reference to a wide range of settings, from hotel restaurants to shipyards.

Also following Goffman (1963), we must remember that spaces and the physical structures that define them are props used in social dramas – in other words, they shape what people do, but they do not *determine* it. For example, while it is well known that in schools the behaviour of teachers often varies sharply between classrooms and staffrooms (Woods 1979; Hammersley 1980), ‘staffroom behaviour’ may nevertheless occur in other parts of a school where conditions are right, or even beyond its boundaries, such as in a local bar. Conversely, behaviour typical of the staffroom may not occur in that setting while visitors are there, or even while the head teacher is present.

If we are to avoid being led by contextual variability into false generalizations about attitudes and behaviour within a case, we must identify the contexts in terms of which people in the setting act, recognizing that these are social constructions not simply physical locations, and try to ensure that we sample across all those that are relevant to our focus of inquiry. One way of doing this, mentioned earlier, is through shadowing particular participants; observing them as they move, over time, between the different contexts that form part of their lives or their work (see McDonald 2005; Trouille and Tavory 2016; Wegerif 2019).

### **Conclusion**

Of course, researchers cannot always select those settings and cases for study judged likely to be most fruitful, or always sample within them appropriately. The cases we might wish to select may not be open to study, for one reason or another; and, even if they are, effective strategies for gaining access to the necessary data will need to be developed. Similarly, not all the people we wish to observe or talk to, nor all the contexts we wish to sample within a case, may be accessible – certainly not at the times we want them to be. The problem of gaining access to data is particularly serious in ethnography, since one is operating in settings where the researcher generally has little power, and people have pressing concerns of their own which often give them little reason to cooperate. It is to this problem of access that we turn in the next chapter.