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Version of attached file:

Published Version

Peer-review status of attached file:

Peer-reviewed

Citation for published item:

Crang, M. and Cook, I. (1995) 'Doing ethnographies.', Norwich: Geobooks.

Further information on publisher's website:

<http://www.gees.bham.ac.uk/research/>

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DOING ETHNOGRAPHIES



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Acknowledgements.

The authors would like to thank Myles Gould, Steve Hinchliffe, David Ley, Sarah Whatmore, Clare Madge, James Sidaway, two anonymous referees, and those others who have entered into more informal chat with us for their help, advice and encouragement. The co-author is responsible for any remaining flaws and inadequacies. If it had not been for his interference it would, of course, have been perfect. Various parts of the work used in this booklet were supported by research studentships from the ESRC and a teaching assistantship from the University of Kentucky Graduate School.

About the authors.

At the time of completing this booklet, Ian Cook was a lecturer at Lampeter having conducted PhD research at Bristol University into the links between the production of an exotic fruit in Jamaica and its retailing in British supermarkets. He had previously conducted MA research at the University of Kentucky into the biographies and everyday lives of four legally-blind people living in the city of 'Effingham'. Mike Crang was a lecturer at Durham having undertaken PhD research at Bristol into contemporary popular understandings of national and local heritages. He had previously attempted undergraduate research on the labour process in electronics firms in Malaysia. Both authors believed that they had benefited from the errors made in their previous work but neither had yet finished an error-free thesis. Both, however, remained optimistic that their theses would somehow get done within university and/or research council deadlines but neither expected them to resemble what they had proposed to do at the outset.



1. INTRODUCTION.

In recent years, an increasing number of researchers in human geography have drawn on qualitative methods in their work. The aim of this booklet is to give an introductory guide to the practice of those methods broadly referred to as "ethnographic", i.e. participant observation, interviewing, focus groups and, increasingly, video/photographic work. The basic purpose in using these methods is to understand parts of the world as they are experienced and understood in the everyday lives of people who actually 'live them out'. Yet, while there is an established literature dealing with the poetics and politics of *writing* such ethnographies (Atkinson 1990; Crang 1992; Gordon 1988; Marcus & Clifford 1986; Marcus & Cushman 1982; Spencer 1989), far less has emerged concerning the poetics and politics of *doing* them. Historically, relatively few researchers have, in their final monographs, included detailed discussions of how their methods 'worked' in the field. And, as a result, many first-time ethnographers have found that reading these works, along with the standard 'how to' manuals, can leave them ill-prepared for the losses of 'control' and surprising twists and turns which their work can subsequently take. Although some may be drawn to the more 'predictable' and 'controllable' results which quantitative methods often promise, our intention here is to argue that, with appropriate preparations, the inevitable contingencies of any ethnographic project can be productively incorporated and built upon from the very start.

This booklet has by no means been written as a menu of abstract concepts and methods to be learned and then applied in the field to answer tightly defined research questions. Rather, it is intended to serve as a guide to preparing for the sorts of issues and methods which have to be considered throughout an ethnographic project (with its inevitable constraints of time and money). In our experience, researchers have often been reluctant to do ethnographies because they fear that these somehow must either inevitably fail to get to the "nitty-gritty" of a problem, or involve methods which can only be used "properly" by rare, and unusually gifted, people. Our intention here is to argue that neither of these need be true. Drawing on both the techniques literature and our experiences of doing this type of work, we want to demystify this approach and thereby provide a positive foundation on which to build a 'doable' project.

In place of the conventional *read-then-do-then-write* sequence of 'doing research', we sketch out a different series of events where, in order for a

prospective researcher effectively to prepare for and to deal with her/his ethnography's contingent progress, reading, doing and writing should be thoroughly mixed up throughout its course. At the same time, we acknowledge that the relative emphasis placed on these different activities will change between devising a project and handing in the final manuscript. Thus we discuss, first, how subjectivities can be conceptualised, second, how these conceptualisations can be used to develop appropriate fieldwork strategies, third, what kinds of information can be constructed by using differing ethnographic 'methods' and, finally, how the consequent mass of information can begin to be analysed.



2. CONCEPTUALISING THE SUBJECT.

"I don't like the distinction between theory and ethnography. There is a saying, attributed to William James, that you can't pick up rocks in a field without a theory. Ethnography is not simply 'data collection'; it is rich in implicit theories of culture, society and the individual" (Agar 1980:23).

(i) INTRODUCTION.

In the 1970s, humanist geographers began to incorporate ethnographic methods into their research as a reaction to positivist geographers' general lack of concern with the complexities of different people's experiences of everyday social and cultural processes (e.g. Ley 1988; Rowles 1978a; Seamon 1979). They began to draw on sociological and anthropological traditions in which these experiences were not being treated as constellations of measurable variables but, rather, as localised, holistic cultures which could be made sense of only through in-depth observation, *in situ*. Here, readings of inter-war, 'Chicago School' ethnographies as well as more philosophical works in phenomenology and symbolic interactionism were particularly important in the rethinking of people's geographies (Jackson 1983, 1985, 1989). Everyday actions were seen as the result of individuals drawing on the structures of their 'culture', rather than these structures being seen as, somehow, existing 'outside' the mundane spheres of their everyday action and knowledge. Yet, while the general aims of this ethnographic approach have come

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to be considered quite favourably (especially within feminist and new cultural geographies), it has also been criticised because 1) it has invariably characterised its subjects as having a 'culture' which can be unproblematically 'read' by an apparently detached researcher, 2) these subjects have been treated as pure, transparent, and knowable carriers of uncontested cultural codes, 3) their 'cultures' have been seen as isolated, pure and homogeneous entities, and 4) in the face of the still-narrow 'Scientism' of mainstream academia, ethnographic researchers have had to fend off criticisms of the 'mere subjectivity' of their conclusions. Our intention in this section, then, is to argue that, in using ethnographic methods, it is an extremely good idea for the prospective researcher to incorporate social and cultural theories which will allow her/him to take these issues into account from the very start.

(ii) THE DETACHED RESEARCHER?

In traditional academic works, 'cultures' have been represented as independent both from the means by which the researcher gained access to and (mis)understood them, and from the ways in which they were produced, reproduced and transformed in the histories and day to day struggles of the people under study (Duncan 1981). As Barbara Tedlock has written about E.E. Evans-Pritchard's classic ethnography of *The Nuer* (1940), for instance, in perhaps typical style he:

"...included a seven-page first-person confessional account of the terrible living conditions and informant difficulties he experienced during fieldwork in the Sudan. In sharp contrast, the remainder of the book, written in an omniscient third-person authoritative voice, describes highly abstract, nonempirical entities, such as lineage and age-set systems, and the idealised actions of common denominator people: the Nuer do this, the Nuer do that" (1991:74).

The point here, then, is that such essentialised "common denominators" who *all* 'do this' and *all* 'do that' - whether 'at home' or 'abroad' - have not simply been discovered in the third person by a detached researcher, but constructed out of an *intersubjective* research process always saturated with relations of power/knowledge.¹ If mentioned at all, these kinds of relations have usually been either consigned to the introductions, footnotes and appendices of an "academic"

¹. The concept of power/knowledge will be used throughout this booklet to stress that power is inescapably bound up with the production of knowledge and, therefore, that the production of ethnographic knowledge is an inherently political act. For the genealogy of this concept, see Foucault (1977, 1980).

text, or written as a separate account under an assumed name or by the researcher's (usually female) partner, and published as a "non-academic" text, as if one could be so easily prized apart from the other (Abu-Lughod 1990; DeVita 1992a; Grimshaw 1992; Pratt 1986; Tedlock 1991).

So, in contrast to this masculinist Scientific stance which has spuriously claimed a cool, calm and collected detachment for the heroic fieldworker, other approaches have emerged which critique this for concealing the fact that they/we are equally positioned, interconnected, and involved in the social and cultural relations under study (Bondi & Domosh 1992; Conquergood 1991; Haraway 1988; Katz 1994; Kobayashi 1994; Nast 1994; Oakley 1981; Rose 1993). As such, many have argued that researchers should conceptualise themselves, as well as the people they study, as variously positioned (by intersections of class, gender, sexual, ethnic, {dis}abled, generational, national, local and other identities), interconnected, and capable of changing, as well as being changed by, the societies in which they/we live. So, rather than claiming some sort of separate Archimedean point from which the world can be critiqued, the researcher's viewpoint is largely a product of social relations both within the academy and between it and the world at large. The detached Scientific approach which has been taken in the writing of many ethnographic texts - an approach which often mystifies this kind of work - has usually been taken both to 'prove' researchers as worthy of their jobs in the Scientific academy and to legitimise their projects as worthy of external funding (Bourdieu 1988; Delaney 1988; Mascia-Lees *et al.* 1989; Pratt 1986). Therefore, whether it is acknowledged or not, it is important to understand that research on social relations inevitably *arises out of* social relations stretching between the field, the academy and beyond (Clifford 1992; Giddens 1984; Hunt 1989; Keith 1992; Probyn 1993; Taussig 1992).

(iii) THE PURE SUBJECT?

As much as the researcher is embedded in these multiple contexts, so are the subjects of her/his research. People experience and act on the world at multiple points, times and places and, strung together throughout their/our life-courses, these experiences and actions form different biographies and self-identities. In turn, these identities are gendered, classed and coloured and, therefore, cannot be understood without understanding the histories and impacts of these and other categorisations. Moreover, while various groups have specific ethos and habits which condition what they take for granted, they/we also try both to overcome and to utilise the materials and obstacles encountered on the way. As a result, it is not

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enough for researchers to identify where people are (both socially and spatially) - they must also question where they/we are coming from, going to, and where on this path the research encounter has occurred.

Given these various histories, a person's identity can be understood as an assemblage of thoughts, feelings, memories, ways of doing things, possessions and so forth which does not fit together in a dedicated pattern but is always a compromise, always pragmatic, always in flux, and never pure (McCracken 1988a; Miller 1987). It is therefore reflected in, and reinforced by, such things as household furnishings which are chosen because they reflect and promote certain self-conceptions or are lived with because they are gifts which reflect how someone else saw them/us. People take snapshots to commemorate significant events and thereby mark what is and is not significant to commemorate, and so on. When studying people's lives, then, these can all be brought in as testimony to how people see, shape and are embedded in the world around them (Csikszentmihalyi and Rochberg-Halton 1981; McCracken 1988a; Reme 1993; Walker & Moulton 1989). In light of this, researchers should consider how the context in which the research encounter takes place can provoke memories and insights into the world views and self-conceptions of differently positioned people. Different memories may be evoked by various belongings or locales associated with different facets of people's identities (Rowles 1983), and it is also important to recognise that people live out their lives between different social locales and emphasise different facets of their identities to different people as they/we move between them (van der Ploeg 1986). In these contexts, the ways in which people make sense of them/our-selves and the worlds in which they/we live are often the result of discussions and debates with different groups of people as events are reported and interpreted *socially* through hearing about them from others, or even thinking about what someone else has said or would say about them. Therefore, not only is the *place* where the researcher and her/his 'subjects' meet important to any study, but also the social context in which this is arranged.

Through doing ethnographic research, then, academics inevitably find that the boundaries of the pure subject must break down, as thoughts are traced back to books, to friends or relations, to newspaper stories, and so on. However, at the same time people cannot simply be expected to report all the 'facts' of their lives. In their telling, life stories involve a recasting of the past, omitting some elements, stressing others, 'forgetting' much more and constantly referring outside the frame of the research encounter. As a result, it is more than likely that within and between parts of these accounts there will be numerous inconsistencies and contradictions (Hedges 1985; Miles & Crush 1993; Pile 1993). Therefore,

ethnographic research is not only a matter of finding out what a spuriously pure subject might think and do but, through tracing these connections and critically engaging with these stories, it is also one of trying to get both at *why* this has come to be the case and at what wider causes and effects this might have.

(iv) THE PURE CULTURE?

Much recent writing on place and personal identity has emphasised that the 'purity' often striven for in academic accounts of peoples and places has usually been founded on the repression of connections with those in other times/spaces. Therefore, in deciding who and where to study, researchers run the risk of embarking on a self-fulfilling prophecy in which the definition of a researchable community can lead to findings which imply that its boundaries are secure and that it exists as a discrete entity. Doreen Massey, for instance, has argued that the history of geographic research has been very much bound up in this process because:

"Geographers have long been exercised by the problem of defining regions, and this question of 'definition' has almost always been reduced to the issue of drawing lines around a place. But that kind of boundary around an area precisely distinguishes between an inside and an outside. It can so easily be yet another way of constructing a counterposition between 'us' and 'them'" (1991:28).

This process of active distinction, it has been argued, can go directly against the experiences of vast numbers of people (including researchers) who, while being 'placed' in both academic and popular accounts as within or outside such cultural or geographical borders and thereby ascribed discrete identities, continue to live lives very much across and between them.

To give one example, in recent years this question of borders and identities has been increasingly problematised by researchers who have attempted to tackle the relationships between the local and global power/knowledges which have given rise to variously nuanced and connected "cultures of colonialism" (Ashcroft *et al.* 1989; Thomas 1994). These are by no means pure nor simply situated within any cut-and-dried borders, nor are they simply black or white, male or female, 'First' or 'Third' world. So, for instance, as the black British sociologist, Stuart Hall, has argued:

"People like me who came to England in the 1950s {from the Caribbean} have been there for centuries; symbolically, we have been there for

centuries. I was coming home. I am the sugar at the bottom of the English cup of tea. I am the sweet tooth, the sugar plantations that rotted generations of English children's teeth. There are thousands of others besides me that are, you know, the cup of tea itself. Because they don't grow it in Lancashire you know. Not a single tea plantation exists within the United Kingdom. This is the symbolisation of English identity - I mean, what does anybody in the world know about an English person except that they can't get through the day without a cup of tea? Where does it come from? Ceylon - Sri Lanka, India. That is the outside history that is inside the history of the English. There is no English history without that history" (1991:48-49).

By acknowledging and studying the histories of, and influences on, such diasporic (post)colonial cultures, popular and academic depictions of a distinct, pure, bounded and, usually, white sense of 'England' and 'Englishness' have been seriously challenged (Gilroy 1987, 1992, 1993a&b; Hall 1992; Hebdige 1990; James 1992; Jeater 1992; Jones 1988; Linebaugh 1982; Linebaugh & Rediker 1990; Massey 1991; Rediker 1987). And, the same kind of argument could be made with any research topic which you might consider studying. Local cultures cannot be ring-fenced from large-scale, political and economic processes because the global is not 'out there', intruding annoyingly on the study, but is always 'in here', only existing *through* variously connected localities (Giddens 1984; Knorr-Cetina 1981a; Marcus & Fischer 1986; Morley 1991; Rosaldo 1989; Thomas 1991).

(v) 'SUBJECTIVE' CONCLUSIONS?

The above may well leave prospective researchers somewhat nervous that admitting the positionality of their knowledge will mean that they cannot draw valid conclusions, unlike their colleagues using more 'objective' approaches. However, ethnography does not imply a retreat from the rigorous analysis of social and cultural processes because researchers *do not need* to assume an abstract vantage point in order to study them. Rather, it can lead to interesting work on precisely how local cultures reproduce and transform more global 'structures' (e.g. Carney & Watts 1990, 1991; Willis 1977). Here, through the largely taken for granted routinisation of their/our day-to-day lives, people can be seen to reproduce and to transform processes that extend far beyond the conscious spheres of their/our actions (Bourdieu 1984; Giddens 1984; Marcus 1986). So, as Roy Bhaskar has put it, for instance, "people do not marry to reproduce the nuclear family or work to sustain the capitalist economy. Yet it is nevertheless the

consequence (and inexorable result) of, as it is also a necessary condition for, their activity" (quoted in Thrift 1983: 31).

If anything, then, ethnographic research reveals that a range of neat theories produce erroneously neat conclusions - a situation which has been coined in the phrase that societies are always messier than our theories of them (Mann 1986). So, as Alan Hedges has explained:

"There are very few golden rules and certainly no magic formulae for cutting through to Truth - if indeed there is any single monolithic truth, which is not typically the case. Human beings are complex, ambivalent, inconsistent creatures; not even the brightest and best organised of us lives in a sharp-edged world where we have all consciously and consistently sorted out our attitudes and beliefs on all conceivable subjects. It is a mistake to assume that there is a pristine Platonic reality under the muddle of our public utterances to which really sharp research tools can cut unerringly through. Underneath the mess of language lies a mess of thought and a tangle of behaviour. If our research tools cannot recognise ambivalence and inconsistency as real and important, they will not help us to a very profound understanding of human thoughts and behaviour" (1985:85).

This lays ethnography open to the claim of relying on 'subjective', or 'biased' data. However, we argue it is this very subjectivity that gives ethnography its reliability. Ethnographers can not take a naive stance that what they are told is the absolute 'truth'. Rather, they/we are involved in the struggle to produce *inter-subjective truths*, to understand why so many versions of events are produced and recited. It is the ways in which people make sense of the events around them, and render these 'true' in their own terms, that is most revealing about how their/our lives are embroiled in larger social, cultural, economic and political processes. Therefore, stories told in the research encounter are not simply to be regarded as means of mirroring the world, but as the means *through which* it is constructed, understood and acted upon.

Given all of this, ethnography requires a different set of principles through which its truth claims can be validated. To do this, its researchers have employed a number of concepts. The first of these has been termed *theoretical sampling* which, although sounding like a term straight out of the positivistic canon, refers to the means by which the researcher decides *who* should be approached to take part in her/his work. Here, in place of the random sampling of statistical research, this approach involves gaining selective access to appropriate groups of people who may be concerned and/or involved in living through the research problem and encouraging them to teach the researcher about it from *their* various perspectives.

Therefore, it is not the sheer *number*, '*typicality*' or '*representativeness*' of people approached which matters, but the *quality* and *positionality* of the information that they can offer (Geiger 1990; McCracken 1988b). Second, researching the lives of every member of every interest group is not only impractical in most studies but is also unnecessary because there usually comes a point in the research process where the range of arguments which *can* be made concerning a particular matter *has* been made. Here, researchers often find that the accounts they/we are told begin to have the same ring about them and that "you have heard the range of stories that people within the community have to tell you about their experiences and explanations of what is happening to them" (Burgess 1992a: 209). This is termed the point of *theoretical saturation* and indicates that the ways in which each person has made sense of the matter in hand has been a result both of talking about it with other community members and through their/our access to similar non-local sources of information. Therefore, in trying to make sense of the situation under study, researchers often find that the people with whom they work have drawn on a relatively small number of shared discourses in various combinations. Therefore, as these discourses and their combinations begin to repeat themselves in the research process, this may either be the point to move on to analyse them or to seek out viewpoints from another, differently-positioned group. Finally, ethnographers have been encouraged to strive for *theoretical adequacy* in their work (Schutz 1967) in that it is important to understand the various contexts of the study, and its similarities and differences with others. Therefore, library visits are vital in order to search for other researchers' interpretations of similar situations, as well as more general theoretical concepts within which the study could be situated. The main idea is that, for the researcher to have confidence that her/his study has been rigorous enough, s/he must have sought out and explored the tensions and commonalities between *multiple* perspectives on the research problem.

We argue, then, that taking the above points into consideration must mean that the 'validity' of ethnographic truth-claims have to be gauged on their own terms. We do not believe in being defensive on this issue because there is virtually no other way of studying the vital interrelationships between subjectivity and the kinds of processes we have mentioned. So, what we wish to stress here is that, in order to have rigorously studied something, the prospective ethnographer may often have to use multiple methods to gain a more rounded picture of the research topic. Thus, rather than being a source of weakness, the always already positioned and intersubjective nature of ethnography can be seen as a strength out of which more rigorous understandings can be built.

(vi) SUMMARY.

Throughout this section we have argued that the conceptualisation of subjectivity is of profound importance at all stages of an ethnographic project. It is important to acknowledge that, firstly, researchers cannot claim to (have) isolate(d) 'local' cultures from more 'global' political, and economic processes because the latter are never simply 'out there' but, rather, are always 'in here', constituting and being constituted by *variously connected* 'localities'. Secondly, they/we can neither make similar claims to (have) isolate(d) 'presents' from 'pasts' nor 'individuals' from the 'societies' in which they/we live and learn. Thirdly, prospective researchers should take account of this in all stages of their research project, not only by tracing such connections as necessary but also by recognising that the resultant enquiries will inevitably be both partial and positioned within a particular web of interdependencies whose horizons will define the limits of possible interpretation.

**3. PREPARING FOR FIELDWORK.**

"Quite unlike its pristine and logical presentation in journal articles - 'the reconstructed logic of science' - real research is often confusing, messy, intensely frustrating, and fundamentally non-linear" (Marshall & Rossman 1989:21).

In this section we try to outline what the considerations in the previous section may mean in terms of setting out to start an ethnographic project. As such, we stress the importance of blurring the division between reading and doing through discussions of casting a preliminary research net, gaining access to appropriate people and, through this, building up a 'doable' project which is sensitive to the inevitable power relations and ethical issues of research. This section is intended as a general prelude to any and all of the methods discussed later.

(i) CASTING YOUR NET.

As a first step in any ethnography, it is important to develop early contacts in the organisation/industry/community/area in which you are interested to find out what research may be possible within the constraints of access, time, mobility, and money available for 'fieldwork', and to undertake methodological, theoretical and linguistic preparations accordingly. Here, it is a good idea to talk about what or who you plan to study with friends, family, fellow students, or faculty members; contact appropriate governmental and non-governmental organisations, community groups, campaign groups, the authors of relevant academic and other articles; place advertisements in the personal columns of appropriate local/national newspapers or special-interest magazines, place posters on community notice-boards and/or phone local radio stations to air your plea for participants; and/or try mail-shots in the place you intend to study. Whoever you contact, always outline the project you have in mind, look for contacts who might be of further assistance, identify the 'gatekeepers' who may be most sympathetic to your project, and arrange to meet with them.

As a general guide, one of the most important tasks to work on at the start of a project is that of developing a wide network of contacts *loosely* based around the germ of your project. Moreover, once contacts have been cultivated, the researcher can ask who else might be worth talking to about the topic in hand: ask for an address, a telephone number, or an introduction and try to snowball contacts on from there (Cassell 1988). Ian's ethnographic research on a Jamaican 'exotic' fruit farm, for instance, resulted from the development of a complex web of contacts involving a Professor known by his supervisor who played tennis with a managing director of one of the 'Big Four' British supermarket chains who arranged an interview for him with its trading and marketing directors. Also, letters he sent to each of these chains' trading managers outlining the project and asking to meet with them to discuss their exotic fruit sourcing and marketing practices led to contacts subsequently being developed in the HQs of two of the other chains which, in turn, led to introductions to the people responsible for buying their exotics and, via them, to executives working for the companies which supplied them. Still other contacts were made through his office-mate whose partner was doing research in Jamaica who, in turn, introduced Ian to one of his colleagues who had met the farm manager and his friends on a previous visit there. Although this had not been his cynical intention at the time, when the introduction was finally made to this farm manager, these discussions with people who he knew and, by and large, trusted probably made Ian seem a somewhat accepted part of an already known community rather than a completely unknown and difficult to place

stranger. And, it must be stressed here, this is a far from unusual research tale and illustrates how a project is often focused in on.

In these initial stages the prospective researcher should also consider the need for research permits and visas needed for overseas fieldwork. These are not needed for all countries - British citizens, for example, do not need any sort of visa to spend up to six months in a country in the Commonwealth Caribbean - but in some cases researchers may have to apply for such a visa perhaps six months to a year in advance with no guarantee of getting it. For instance, it took Mike an unexpected seven months to get a research visa for Malaysia, and the delay threatened to stop the research project altogether. If an overseas destination is vital, then such practicalities must be taken into account at an early stage. Write to the embassies of the countries you might expect to work in, and to other researchers who have recently conducted fieldwork there, and ask about these procedures. If all else fails, you may have to weigh up the pros and cons of entering your chosen country on a tourist visa (Sidaway 1992).

Casting your net widely in the early stages of an ethnography, then, is vital. And, in more general terms, it is this process that is more influential in determining its shape than any theoretical minutiae poured over in the academy. Ethnographic projects do not emerge in the form of pristine hypotheses to be tested later 'in the field' but require a fusion of knowing what is interesting and what is accessible. Thus, in preparation for your fieldwork we strongly recommend a combination of reading *and* doing, rather than a separation of reading *then* doing.

(ii) GAINING ACCESS.

So far we have been discussing some of the intricacies of contacting people. However, as we argued earlier, it may also be necessary to get into specific locations to observe processes or to talk to participants in particular contexts. If, for instance, the aim is to study labour processes, then the researcher may have to negotiate an entrance into the spaces where these take place. Some researchers using participant observation methods have studied the kinds of casual work which are conventionally taken on by student types anyway - such as waiting in restaurants (Crang forthcoming), or picking vegetables (Thomas 1985). In this kind of case, preparation for a research project may begin by simply scanning a local newspaper's 'Situations Vacant' column, enrolling with an employment agency, going down to the job centre, or contacting previous employers to see if they have any vacancies for a tried and trusted worker. And, indeed, with the current financial difficulties experienced by many undergraduate and postgraduate

researchers alike, one advantage of taking on such work is that it can double as a means to earn much-needed cash.

In contrast, if the researcher's interest is in studying domestic or leisure activities such as domestic labour, TV watching, shopping activities, or membership of particular social clubs, political/campaign groups or subcultures, then s/he must somehow negotiate access to their appropriate spaces. Although the aim, at this stage, would be to gain access to a single place - village, neighbourhood, festival site, and so on - ethnographies can also cross-cut such places. Here we are thinking of Gill Valentine's (1993a&b) research on the management of multiple sexual identities by women in a lesbian community who lived their lives somewhat differently between various settings such as the local high street, their homes, workplaces, gay bars and clubs. Moreover, when setting up interviews or group work especially, the researcher may also be involved in creating a space in which participants are free to talk about the research topic. Again, much of the same types of advice apply as with seeking initial contacts, but even in the best organised study no one ever achieves a 100% response rate. One of the more nervous and dispiriting times during research can be receiving a steady stream of rejections. All that can be said is that, if you keep trying, sooner or later something will give somewhere and this phase will pass. This situation is much the same whether mailing potential interviewees or seeing 'gatekeepers', and it is important to keep this in perspective. Rejections should not be taken personally - you are seeking to inconvenience people so their rejections are hardly surprising. You may be able to improve the proportion of favourable responses a little by remembering this and being sensitive to the constraints and pressures on potential respondents (McCracken 1988b; Stewart & Shamdasani 1990). Perhaps the main point to keep in mind here is to follow up your ideas and contacts, but always to think about a second, and perhaps a third, point of access in case one or other closes up as the work progresses.

Setting out to take these first, often tentative steps, it is important to note that *this* is where the 'fieldwork' starts. The processes through which particular people and/or positions are found make for good ethnographic materials because they are likely to involve 'gatekeepers' assessing aspects of your identity which are considered (in)appropriate for them. Much can depend on how you can be placed or positioned by these early contacts - especially if they are government officials who will assess your proposals and have the power to grant or to deny access to an entire country. It is necessary to consider how you are portraying yourself and your research to these and every other 'gatekeeper'. To give an example of this process, when preparing to undertake some interviews in electronics firms in Malaysia,

Mike encountered great difficulties in contacting workers. The firms were surrounded by barbed wire, the workers were suspicious of the motives of anyone who wanted to know about their jobs and he came to realise that many Malay women were suspicious of the motives of western men. Many were also worried about the consequences for their employment and for their reputations, given the local meanings associated with being seen to rendezvous with a man, unaccompanied. Mike therefore worked via the contacts of local academics with the Malaysian Trade Union Congress, but found even these people very cautious. At his wit's end after a stony meeting with the Deputy President of the local branch, Mike produced his research permits from the Prime Minister's Office which, if anything, added to his problems. He tried the ploy that he was a student and was thus no threat to anyone. This also did not appear to be working, but, in the process of digging through his wallet to find something which would prove his status, he came across his Labour Party membership card. The Deputy then began to take interest - which was an improvement - so Mike showed it to him. The Deputy then read out loud from it the statement, "To secure for the workers by hand or by brain the full fruits of their industry and the most equitable distribution thereof that may be possible upon the basis of the common ownership of the means of production, distribution and exchange, and the best obtainable system of popular administration and control of each industry or service", paused, and then said, "That is possibly one of the most beautiful things I have ever seen written". Unexpectedly, then, this aspect of Mike's identity, once expressed, opened a number of important doors for his research.

In the process of gaining access, researchers usually endure days or weeks of doubt and frustration before, as in the case above, becoming quite suddenly overjoyed when things somehow start to work out, often better than could have ever have been planned. But in terms of time this can indeed be very unpredictable, particularly in the initial stages of forming contacts. It may take a couple of weeks to arrange a first formal meeting with someone in a company who may then refer you to another employee. If this meeting takes as long to arrange, you could have spent a month on just two interviews. Therefore, we suggest that attempting to establish as many contacts as possible helps to increase the speed of access, both in the event that one meeting falls through or that a 'gatekeeper' proves uncooperative or uninformative. What will tend to happen is that, as more contacts are established, you will begin to get multiple suggestions for further contacts and it will become easier to know who to contact and how. Thus, in later stages of your work, the problem may be less of an inability to see people and more one of being overwhelmed by possible contacts. So, on the one hand, it can be a good idea to

stagger different stages of your work so that everything does not happen at once but, on the other hand, some comparative research can be aided by studying what different people are doing in the same period. We would therefore strongly suggest that a good deal of thought be given to how the research is likely to occupy time in the field in order to most productively use it. That said, we have never got responses or access according to any pre-planned schedule. So, again, perhaps the best advice here is to prepare to be flexible.

(iii) POWER AND KNOWLEDGE.

Research is always bound up in networks of power/knowledge and is, therefore, inherently political. Many writers have argued that this is something that the researcher should tackle head on, rather than simply deny through sheltering behind the traditional veil of 'objectivity'. Yet, the energy which researchers have to direct at tackling the immediate problems of getting through each part of their work may mask how s/he has also struggled through these in contexts of unequal power relations. Among the 'Third World' peoples usually studied by ethnographers, for instance, Jarvie has argued that "many people would not tolerate the white stranger snooping around were it not that he {sic} belongs, as far as they are concerned, to the powerful white society which they hesitate to brush with" (in Cassell 1988: 93; Clifford 1992). Also, where researchers are suspended between differently-powered groups, their/our roles and responsibilities may have to be compromised (Wade 1984); and, in situations where more powerful elites are being studied, on the one hand they/we may be seen as a threat through having the power to open out these people's lives for ridicule or ruination by other groups (Cook 1993; Johnson 1992) yet,¹ on the other, these are also the people who usually have the power to bar the researcher's access in the first place (Cassell 1988). So, in terms of gaining access, not only must the significance of the researcher's position and apparent intentions be considered but so too must her/his responsibilities over how the people being researched will be represented in any account produced, how this will be circulated, and the impact that this might have on their lives in the future. As Michael Taussig has insisted, researchers in the Americas have a responsibility to ask themselves "who benefits from studies of the poor, especially from their resistance? The objects of study or the CIA?" (1992: 52; Katz 1994; Tedlock 1991).

We therefore believe that it is vital for the prospective ethnographer to consider whether the community in question might resent and/or suffer badly as a result of

¹. See McDowell (1992) & Schoenberger (1991, 1992) for a different angle on this.

having such a "viper in its bosom" (as Mike was described, half-jokingly, by some Civil War re-enactors). This issue has become particularly sensitive, and the tradition of the archetypal white, male, middle-class, Western, heterosexual, able-bodied researcher studying and pronouncing upon his poorer and/or less powerful 'Others' has been strongly critiqued from various quarters. As members of various subaltern groups have made their presences increasingly felt in academic and popular debates, dominant white (mis)conceptions of black people, male (mis)conceptions of women, middle-class (mis)conceptions of working-class people, Western (mis)conceptions of non-Western people, heterosexual (mis)conceptions of homosexual people, non-disabled (mis)conceptions of disabled people, and so on, have been persistently highlighted, researched and challenged (Oliver 1992; Tedlock 1991). Central to these challenges has been a questioning of researchers' precise motives: are 'we' indulging in an heroic mission to "make the world a better place" for 'them', hoping to discover a 'true' or new self via a detour through the "Other", and/or jumping through a hoop to get or keep a degree or job? Indeed, as a result of these questions being so repeatedly asked, dominant representations of the research process as a cool, Scientific, non-exploitative process have begun to appear quite ridiculous (Abu-Lughod 1990; England 1994; Mascia-Lees *et al.* 1989; Moore 1988; Oliver 1992; Schrijvers 1991).

In this light, a number of suggestions have been made regarding what and how research might be set up in order to be more sensitive to the power relations in academic work. Researchers could frame questions "according to the desires of the oppressed group, by choosing to do work that 'others' want and need" (Mascia-Lees *et al.* 1989:33; Schrijvers 1991); could shift perspective to undertake work which will "expose the colonisers, the powerful, the affluent, who cheat, mistreat or oppress the colonised, powerless, and poor peoples of the world" (Cassell 1988:90; Douglas 1976; Nader 1974; Punch 1986; Thomas 1993; Wax 1980); could combine these approaches in studies which develop "insights and knowledge into global relations among people diversely located and vying for power" (Gordon 1988:21; Cook 1994; Marcus 1986, 1992; Marcus & Fischer 1986); could "turn the question away from Others, especially poor and powerless Others, and onto ourselves and our own quite violent practices whereby we figure ourselves through the creation of objects of study" (Taussig 1992:38; Abu-Lughod 1990; Agar 1980; Katz 1994); and could study 'our own' cultures, cease taking them as some universal benchmark and problematise their values (Bourdieu 1988, 1990a; Strathern 1989). None of these approaches, separately or in combination, will necessarily *solve* the problems outlined here, but the prospective researcher is

advised to read around these debates, discuss them with sympathetic colleagues and members of research communities, and have them in mind at all stages of her/his work.

(iv) SUMMARY.

Throughout this section, we have argued that in order to settle on a research project, it is vital to prepare in such a way that its horizons of reading and doing are thoroughly fused. Indeed, we would argue that to follow the standard three-stage protocol of *read-then-do-then-write* in preparation for such research can cause all sorts of problems (Ley 1988). In preparing for his ethnographic research on how blind people travelled independently through an American city, for instance, Ian spent approximately ten months reading the literature on which their mobility instruction - and therefore, he supposed, their travel - was based. Only then, after he had honed his research questions, did he arrange to meet with a blind person to see how these worked out in her day to day life. Having hypothesised that blind people's travels were limited to a portfolio of discrete, memorised routes, for instance, he asked her how well she knew them. But, she replied quite indignantly:

"These **aren't** routes. These are places. These are maps and I know where I'm going. I do have to think about what I'm doing and where I am within the map. ... You don't have to think. It's not a route, it's a space that I know. ... I **see** it in a real clear map so that at any point I know what I'm facing and, if I wanna go somewhere else, what way I've gotta turn to get there. I don't have to think because it's a map. It's a three-dimensional cognitive structure, sorta" (in Cook 1992:7).¹

This kind of description was very much unlike that which Ian had found in the blindness literature and meant that many months of work had, to a large extent, been wasted. Subsequently, in discussing travel experiences with three other blind people, he had to go back almost to square one and ended up addressing very much unthought-of research questions which emerged out of this kind of dialogue and which had to be situated in what he had previously regarded as 'unrelated' literatures.

¹. In this quotation, words in bold type are those which the person stressed in her speech. The sequences of three dots indicate where speech has been edited out and, here, most often indicates Ian's interjections such as "yeah", "right", and so on.



4. CONSTRUCTING ETHNOGRAPHIC INFORMATION.

"Traditional ethnographic 'pretences' about detached observation and scientific method reveal anxiety about the uncontrollable messiness of any truly interesting fieldwork situation" (Conquergood 1991: 182).

(i) INTRODUCTION.

In this section, we treat ethnographic methods as ways of studying a variety of communities. We do not propose a comprehensive list of 'approaches' - Renata Tesch (1990) has listed some forty-three of these - but hope to give a flavour of those which might be adapted, altered and/or combined to fit various purposes and situations. We do not treat participant observation, interviewing, focus groups and video/photographic work as discrete methods to be applied *after* the preparations mentioned in the previous section. Rather, we would strongly suggest that the dynamics and benefits of each approach should be kept in mind as the possibilities for research unfold, so that appropriate methods can be flexibly employed.

(ii) PARTICIPANT OBSERVATION.

Historically, ethnographic research has developed out of a concern to understand the world views and ways of life of actual people in the contexts of their everyday, lived experiences and the method of participant observation is the means by which ethnographers have often done this. In its basic form it can be described as a three stage process in which the researcher somehow, first, gains access to a particular community, second, lives and/or works among the people under study in order to take on their world views and ways of life, and, third, travels back to the academy to make sense of this through writing up an account of that community's 'culture'. But, straightforward as this may sound, when considering using this method it is vital to understand the key tension suggested in its oxymoronic title. To be a *participant in* a 'culture' implies an immersion of the researcher's self into the everyday rhythms and routines of the community, a development of relationships with people who can show and tell the researcher what is 'going on' there and, through this, an experience of a whole range of relationships and emotional states

that such a process must inevitably involve (Hunt 1989; Wax 1983). Conversely, though, to be an *observer of* a 'culture' implies a detached sitting-back and watching of activities which unfold in front of the researcher as if s/he wasn't there, a simple recording of these goings-on in fieldnotes, tallies, drawings, photographs and other forms of material evidence and, through this, a striving to maintain some form of dispassionate, 'Scientific' objectivity (Fyfe 1992; Maranhao 1986; Tedlock 1991).

Like many other writers, we argue that to talk about participant observation should not be to separate its 'subjective' and 'objective' components, but to talk about it as a means of developing *intersubjective* understandings between researcher and researched (Crapanzano 1986; Dwyer 1977; Spencer 1989; Tedlock 1991). And, it is important for the researcher to think not only about how s/he becomes 'immersed' in the community under study, but also about how s/he and, variously, *they* are immersed in other communities outside - which may be based around geopolitics, banking, the media, mass consumption, sports, leisure, friendship, family, etc. - and how this affects the ways in which the research process develops. In this section such development is discussed in terms of the ranges of researchers' potential access to, and roles within, certain communities, and how various types of information and understandings can be actively constructed, represented and contextualised for use in the subsequent stages of analysis and writing up.

a) Access.

We have already dealt with many issues that impact on gaining access to study areas, but it is worth noting that for this method there are some particular considerations. Much of the discussion on participant observation focuses around how researchers can, where possible, take on already existing subject positions in the communities which they study or, where it is not, to construct new ones. For instance, given that it is rare for researchers to be given enough time and/or money to develop a professional skill in preparation for their study, some labour processes are difficult topics for participant observation research. Unless researchers have spent some years qualifying and working as plumbers, nurses, accountants, or pilots, for instance, although they may be able to *observe* such work, it is extremely unlikely that they will be able to *participate* in it without anyone noticing their inability to solder a joint, administer a suppository, keep double-entry books, or land a 747. In other cases researchers may have spent a significant portion of their lives working in a particular profession and then, for whatever reasons, have gone (back) to college to do research which builds on these.

In the case of professional spaces, then, perhaps the nearest the relatively unqualified researcher can get to the community under study is to apply for perhaps more easily accessible jobs in the same spaces - as a plumber's mate, hospital porter, or clerical assistant - and to participate and to observe at this level. Alternatively, the researcher can search for already established positions in which participation in, and observation of, professional lives by amateurs is a legitimate role. In his research on the supplying and marketing practices of supermarket fruit buyers, for instance, Ian found that, like any other college graduate, he could have been suitably qualified to apply for one company's three month graduate placement scheme. With its purpose being to allow potential recruits to watch and to question employees already doing this work, and eventually to try it out for themselves, this was tailor-made for participant observation research. In this respect and, again, in the early stages of the research, it may be a good idea to find out if similar kinds of access may be possible into a particular profession or company. Otherwise, the option which can be taken is to ask more specifically to tag along as a researcher who might occasionally 'help out' as a driver, translator and so on. Such a role can have the benefits of providing the researcher with a legitimate occupation, new contacts and the chance to give something back to the community under study (Ley 1988), yet care must also be taken to prevent this role from swamping the research (Wax 1983).

Contrary to its traditional image, then, participant observation research is not always a matter of spending a year or two living in an isolated community in some remote part of the world. Most 'communities' are spatially dispersed and many are occasional, or intermittent. Therefore, if you are interested in studying a work or leisure community which comes together in different places and at different times, then the constant 'immersion' suggested in many anthropology texts will not be possible (Radway 1988). On many occasions, you could be doing participant observation on some days of the week and 'ordinary' work on another. Mike's participant observation work with an historical re-enactment group, for instance, involved meeting them for "musters" on separate weekends in fields near York, Bradford, and Yeovil. In this and most other experiences of participant observation work, boundaries between field and academic experiences become blurred. Often the days back in the academy will have a profound effect on your views of the field, and vice versa. Mike has certainly found this in that, while he was joining in with the activities of this re-enactment group, the 'normality' of the participants was never in doubt. However, the moment he returned to the office, there were constant jibes about the 'sad weirdoes' whom he was both studying and supposedly becoming like. He therefore found it extremely difficult to 'forget' this when he

went back to the next re-enactment, and this illustrates the importance of adopting different identities in the academy and in the field and attempting to understand and to build on the tensions between these (cf. Madge 1993).

b) Roles.

In terms of gaining access to particular communities, it is not only who the researcher contacts that is important, but also how s/he explains the project to them. The ways in which s/he *presents* aspects of her/his self in the process of negotiating access to a community is particularly important. Thus, the matter of to what degree the researcher's questions and roles should be overt or covert have to be breached at an early stage. When they first meet, for instance, what should the researcher tell the 'gatekeeper' about the purpose of her/his wishing to take up a particular position in a community? Should s/he immediately divulge the intricacies of her/his project on being introduced to community members or wait until relationships have been developed in which such revelations may more easily be made? Will community members care very much what this is, anyway? Will they (mis)understand the language the researcher uses, anyway, and vice versa? And, is it likely that s/he will or should have the same purpose at the end of the fieldwork as at its beginning?

After a number of initial enquiries with contacts in the field, the researcher will have had the chance to hone her/his 'purposes' in order to properly word any formal or informal application for access. This does not necessarily have to be couched in terms of a set of bare-faced lies, though. In the early stages, the researcher may simply want to enquire what constitutes the community's everyday activities. Later, once this has been established, a vague idea of what the research might eventually be about or a watered-down version of the research question(s) will often suffice. In terms of what these might be, there is often a huge difference between what the researcher tells her/his academic colleagues that the research is about and what s/he tells various 'gatekeepers' to 'the field'. Yet we do not mean to make a distinction between the former necessarily being the *real* reasons and the latter being their more *tactical* versions. In his fruit research, for instance, the question that Ian wrote for consumption by his research committee went as follows: "Given that many authors have argued that the global food economy functions through connecting, maintaining and often deepening extremes of wealth and poverty, overnutrition and malnutrition, and so forth, and that this has been regarded as 'obscene' by many of them, how can such an 'obscene' system operate through the everyday actions of people who, I assume, do not see themselves as 'obscene'?" Later, once it had been decided to settle on addressing this question in the context

of a commodity system linking the retailing of an exotic fruit in the UK and its production somewhere in the 'Underdeveloped World,' and when the time came to make contacts within the fruit trade, the project was variously rejigged as, for example: "Given that there has been a significant increase in the amount and variety of exotic fruits being sold in British supermarkets over the past ten years or so, I'm trying to find out why and how this has happened"; and/or, "I'm treating the commodity systems of fresh fruit as social systems stretched out over massive expanses of time/space, and am trying to find out how they are co-ordinated on a day to day basis from farm to shelf". The point here is that each account was a simplification of the larger project, and as Ian's relationships developed with people in various communities, each was eventually told more about the project and its other questions.

In terms of negotiating these kinds of problems, one guideline which is perhaps most advisable to follow is that the researcher "should adopt a role or identity that meshes with the values and behaviour of the group being studied, without seriously compromising the researcher's own values and behaviour ... {and} not ... inventing an identity; we all have several, ... but ... the most appropriate one can be stressed" (Cassell 1988:97). It is unusual for researchers to have absolutely nothing in common with, or no location within, the world-views of the people whose lives they are attempting to study. Joan Cassell (*ibid.*), for instance, has stated that as a doctor's wife she had a ready made position where surgeons were prepared to discuss issues such as malpractice suits, patient billing or the costs of education with her just as they might with any colleague's wife. Equally, researchers using photography have found that it can provide a readily understood reason and purpose for their presence. Saying that you want to hang around and take photographs of what people are doing can often be seen as a far more acceptable and less disruptive role than that of asking questions and taking notes. Indeed, because participants often ask what the photographer is doing, the former may be an excellent entree to the latter (Cohen *et al.* 1992; Collier & Collier 1986; Schwartz 1989). Yet, at the same time, these commonalities and placements may not turn out as the researcher expects them to. Take Tony Whitehead's (1986) account of his initial experiences in the rural Jamaican community that he studied:

"I am a black American who grew up in the rural South to impoverished sharecropper parents. Regardless of the upward mobility I experienced, when I went to Jamaica I still perceived myself as one of the little people (i.e. lower status) because of my experiences as a member of an ethnic minority in the United States. ... With such a self image in tow, I was shocked when the people {there} began referring to me and treating me as a

'big', 'brown', 'pretty-talking' man. 'Big' was not a reference to my weight but to my higher social status. I was aware of the West Indian correlation between skin colour and social status, but I was not prepared for the personal experience of *my* lightness of skin colour being associated with higher socioeconomic and moral status. ... {And, m}ore embarrassing than bothersome were the references to how 'pretty' I talked, a comment on my Standard English speech pattern" (214-215).

Researchers can also be placed by the people under study within numerous other maps of meaning which are often revealed as the research progresses. How the researcher is made sense of can therefore often reveal aspects of how the world is understood by researcher and researched alike (see DeVita 1992a; Murphy 1992; Pollner & Emerson 1983; Rapport 1993).

In terms of deciding how to settle on particular presentations of the researcher's self and project, then, there are no easy answers because, to a large degree, these can be outside her/his control. Many aspects of her/his identity inevitably end up being played off against each other in various contexts as her/his appearance, ideas, intentions, feelings, politics, ways of doing things, and so forth (have to) change through the experience of setting up and seeing through the project. Through initial conversations and particularly through sustained periods of interaction, researchers can, first, learn which aspects of their identity allow them to be more or less acceptably placed in the world views of both their key informants and the community under study and, second, thereby establish how any common ground might be found. Some questions to think about here, then, are - if the researcher is expecting the people s/he lives and/or works amongst to be frank about their opinions and experiences, should the researcher do likewise in order to foster the development of a genuine intersubjective understanding?; or, should the researcher step back, at least for a while, observe, ask innocent questions (see the following section), and be careful what they reveal about themselves?; how long should s/he spend skipping between different members of the community before relationships can emerge in which researcher and researched develop the trust necessary for both to 'open up' to share (often private) experiences and frankly argue out the issues which each thinks are important, both in the community and more widely?; if the researcher comes to form an opinion about the people s/he has been working with, should s/he present this to them to see whether this gels with their experience, or should s/he preserve the perhaps delicate nature of the relationship by keeping quiet until either the closing stages of their fieldwork or, indeed, the writing up stage when it can, perhaps, be most carefully worked out and handed back for comment?; having promised confidentiality to her/his informants, can the

researcher ask questions of members of one fraction based on information gleaned from members of another? (Johnson 1983).

Most researchers make uneasy and improvised compromises about such things as the research progresses. Some find them/ourselves in situations where they/we are trusted with extremely private and/or damaging information which they/we feel should not be written about, even in the most carefully anonymous account. They/we can also feel shocked, disgusted or threatened by some of the opinions that certain community members hold dear and/or act upon (and, no doubt, the opposite may also be true, e.g. Keith 1992; Nast 1994). At the same time, though, it is not uncommon for people under the researcher's gaze to feel self-conscious or threatened knowing that anything they say may be "written down and used in evidence against them". It is a good idea to keep in mind the fact that few people, including the researcher, are ever 100% (dis)honest, earnest, flippant, sure what they think, consistent in what they say across all contexts or anything else. And, it can take quite some time before the researcher comes to understand these kinds of subtleties and to respond to them appropriately. First, second and third impressions can often be wrong because members of the research community may well be just playing on their expectations of the researcher's expectations to wind her/him up, to provoke a reaction and enjoy themselves at her/his expense (Taussig 1987; Whitehead 1986). You should always be suspicious, then, of why you understand what you understand within the contingent, intersubjective, time/spaces of your fieldwork (Crick 1992).

Whatever role the researcher ends up adopting, communities are extremely unlikely to be so homogeneous that to understand them from *one* perspective is to understand them from *all*. So, the question becomes to what extent the researcher can/should try to gloss over these divisions by attempting to get on with everyone. For many, the ideal stance is that of "an intelligent, sympathetic, and non-judgmental listener" to all of its members (Cassell 1988:95). Yet, there can be problems here because this approach can, in practice, make the researcher stand out in that few, if any, members of a community take up such a role themselves. On this note, Jacqueline Wade (1984) has argued that:

"To present oneself as an unalterably 'neutral' character in the course of the subjects' life events courts an impression that the (researcher) is gullible, amateurish, inane, or uncommitted (or some combination of these) and, thus, unworthy of subjects' attention and time. Furthermore, such a stance could convey to subjects that the (researcher) has, in truth, a negative regard for their inner workings, thereby potentially causing inimical involvements in future areas of field relationships" (219).

At the same time, though, an entirely partisan, single-focus stance would preclude the possibility of critically understanding the meanings of particular situations or problems from the perspectives of differently positioned people who struggle over these with each other in the course of their everyday lives.

So, using this method, if we participate in people's lives and expect them to be honest about their thoughts and feelings, should we also do likewise or bite our lips when this might mean that rapport may be broken?; or, should we simply agree with whatever people in our research communities say or do, and even join in, so as not to sever access to their 'real' lives?; or, in a subtly worded or more straightforward way, should we challenge them to justify themselves? As we stress throughout this booklet, there are no easy or final answers to such ethical questions. All we can do here is to raise these issues and suggest that they may only be precariously resolved at any given point in a research project. Once access has been gained to a community, the researcher can hardly if ever simply blend into it via an uncontentious process of 'role-playing'. So, with so many factors being played off against each other in the field, any student's first stab at participant observation research is almost bound to take unpredictable twists and turns which are alternately fascinating, disturbing and challenging (Crick 1992). Moreover, as in the standard texts, this all assumes that the researcher is a 'lone explorer'. But, this 'role-playing' can get even more complicated and difficult to control when researchers find themselves to be the latest in a line of researchers to study the community (Pratt 1986; Shostak 1981), when partners and/or children have accompanied them into the field (Cassell 1987; Rosaldo 1989; Tedlock 1991), and/or when team research is conducted (Barley 1984; Douglas 1976), and/or when her/his work is part of a broader (non)governmental initiative (Katz 1994).

c) Language.

Finally, another aspect of the researcher's ideal 'blending-in' with the community under study is that of conversing in its own language as a means of gaining access to its 'culture'. Here, though, even if the researcher has learned the official language of a profession or of a cultural group, it is likely that, in its local improvised, slang and/or creolised version, s/he will by no means be identified as an 'insider' by the way s/he uses it - indeed using it may be incompatible with a role of well-meaning outsider (Whyte 1955). In many conversations, the researcher may understand perhaps only a few sentences, phrases and/or words and have to guess what the other person meant in order to continue the conversation - a process which is likely to be gone through by both parties. Here, on the one hand, there are plenty of opportunities for misunderstandings to develop on both sides and, indeed, for the

researcher to go through periods of not wanting to talk to people s/he is working among for fear of being damagingly misunderstood (this was Ian's experience, at least). On the other hand, though, such breakdowns in communication can be advantageous in that they can render previously unnoticed processes apparent (Agar 1986; R. Thomas 1993).

This kind of linguistic difficulty is not just confined to those researchers who have to work in a second language, though. There may also perhaps be equally difficult technical vocabularies being used by speakers of her/his own language. Cassell, for instance, has stated that, "It took me more than a year to understand surgeons' abbreviations and technical terms" (1988:98). Moreover, she has also stressed that there is more to language than words because "the researcher must {also} decipher the significance of esoteric gestures, movements, behaviours" (*ibid.*). Whatever the case, if the researcher has the time, sustained interaction can allow language skills to be improved and misunderstandings to be resolved. And, if s/he has the equipment and the money, it may be an idea to tape-record some or all conversations and have them professionally transcribed so that they can be slowly pored over later to decipher the subtle nuances of the interactions and to get at how intersubjective understandings developed out of such dialogue. However, at the same time, if the researcher tells the people under study that the tape-recorder is on (it is usual for researchers to ask permission of, rather than just to inform, participants of this), s/he should be aware that members of the research community may well play up to this in a particular way. Ian, for instance, found that a Jamaican farm manager who he worked with was much more 'politically correct' when the recorder was on than when he left it behind on the occasions that they 'socialised', often with his friends, and got involved in much looser conversations. In this, as well as in all of the other circumstances outlined here, there is consequently no simple and unproblematic 'collection of data' in the field.

d) Constructing information.

However the researcher negotiates access to a community and subject position(s) within it, s/he must somehow 'escape' with detailed information on which to work in representing it. Having emphasised in the preceding sections that any understanding which the researcher can gain about the community under study must be partial, positioned, intersubjective, contingent on any number of (un)planned interventions and connections (Spencer 1992), and thus inherently uncertain, it is vital to consider *what* s/he should escape with. The following is by no means a checklist of what the researcher should do but, rather, a number of

means by which s/he might consider constructing ethnographic information from any planned participant observation work.

At some stage, you might consider conducting a brief survey of the community in order to glean some basic information concerning its composition by age, gender, occupation, education, income, life-course, social networks and any number of other factors appropriate to its specific membership. This can serve a number of purposes: first, if it is undertaken early in the work and limited to fairly innocuous questions, it can serve as a means of introducing yourself and your project to community members in a relatively unthreatening way; second, it can generate descriptive statistics which can be used in the write-up to outline the community; third, it can be used to identify the community's key groups and networks within which you might wish to develop contacts; fourth, if you are keen to develop a research project which will address concerns within the community, this may serve as a means to gauge what these might be and how widespread they might be; fifth, if you develop close relationships within the community and then undertake the survey, you may be able to make your questions more appropriate to its members' lives, and the information thereby generated can be used to position their (and, indeed, your own) understandings of the community as being from one or more perspectives.

In a similar vein, it is likely that within and/or outside the community, someone, somewhere has kept relevant records. People take photographs (see later), get telephone bills, keep school reports, compile tax returns, keep time-sheets, compile reports and inventories, file letters, memos and faxes, trace their family trees, listen to radio shows, watch the TV, the list can go on and on. You should perhaps have this in mind at all stages of the research and, when relationships are developed sufficiently with people who might have access to such things, you can ask to see them and copy down or otherwise record them. Such records may also be in the public domain, so a questionnaire administered by a researcher may not be necessary if the community under study falls within a census tract. Finally, the researcher can keep her/his own, and/or ask community members to keep, tallies of various objects and movements in the form of tables and/or maps (Gregory & Altman 1989). Whatever form these records take and however they are gleaned, it must always be realised that, just because they can produce numbers, networks, and stories, these are no more nor less 'objective' or 'subjective' than any other form of information brought back from the field. Such apparently 'cold', 'scientific', or 'unbiased' 'data' is just as much socially and culturally constructed as any other form of information, and has to be sensitively asked for and interpreted with this in mind.

Perhaps the most important document which has been used to contextualise the construction of these kinds of 'data' in the field is the researcher's *field diary* and, in a similar vein, her/his letters 'home'. Kept, at worst, every few days during the period of 'fieldwork', its purpose is usually to keep some kind of record of how the research progresses, day by day, and to chart how the researcher comes to certain (mis)understandings. Diaries should represent the doubts, fears, concerns, feelings, and so on that the researcher has at all stages of her/his work (indeed, there is good reason to argue that the researcher should just as much keep a diary outside 'the field' as inside, because interactions at 'home' with her/his advisor, fellow students, bank management, research council officials, etc.. can have as much, if not more, of an effect on how the research 'turns out' as can members of the designated 'target community').

Therefore, in such a diary it is advisable to be sensitive to a number of different factors: for example, how your understandings are affected by particular perspectives; your developing positionality in the community; power relations which can be discerned in this; how your expectations and motives are played out as the research progresses; what you divulge, and why and to whom and how they appear to react to this; how various aspects of the research encounter make you 'feel' (it is extremely common for researchers to swing between emotional states depending on the circumstances) and how this affects what you do; what you dream about; what rumours have come back to you about yourself and the reasons for your presence in the community; when you were taking pictures, sketching maps or recording a conversation, what kind of context did this take place in (a photograph of smiling workers may indicate that they were happy with their work when, in fact, they were merely smiling at the camera, for instance¹); in what kind of place the interaction took place; who introduced you to whom and how they described and/or reacted to you, your purpose and/or equipment; what your immediate impressions were and how they changed; and so on. Rather than answering these kinds of questions in any particular order, Ian has found that this kind of account is best written as a stream of consciousness which may better set out, and allow him to think about, the developing connections between different aspects of his work and relationships (see Figures 1. and 2.), while Mike has preferred tape-recording his thoughts to be sorted out later for the same reasons. It is also better to get all this down while it is fresh in your mind because even eight hours later your recollections may have become more blurred. Having said this, first-time ethnographers often report how surprised they are at how much detail they *can* remember at the end of the day.

¹. Ian would like to thank Phillipa Superville (1993) for pointing this out to him.

As can be imagined, writing about these issues can involve setting aside a great deal of time each day to get this done. First, the cumulative notes from six months' of fieldwork can amount to tens, if not hundreds, of thousands of words which, because of their bulk, can perhaps be best analysed in the same way as (and alongside any) interview transcripts (see later), so it can be an excellent idea to tap these notes directly into a word-processor. If the researcher cannot get to an office each day from the field-site to use such equipment, s/he may be able to borrow a laptop computer. But there are potential problems again, first concerning how community members may react to such flash equipment which may represent several years' wages to them, and second over the matter of insuring expensive fieldwork equipment for use outside the EC. On top of this, some types of fieldnote, e.g. thematic sketches and diagrams, cannot be easily typed in (Pfaffenberger 1989). But, timewise, you should weigh this up against the fact that you may have to set aside weeks or months when you return from 'the field' to type up your diaries/notes. In terms of minimising this time commitment, we often wish we had taken touch-typing courses early on in our research.

Second, and perhaps more importantly, if the researcher's writing function is known by community members, it is likely to become a distinctive part of her/his identity because s/he is observing, writing things down, and forming opinions about 'them' (Miles & Crush 1993). As a result of this, for instance, Joan Cassell (1988) found that the surgeons who she studied were very perturbed by her taking out a notebook and jotting down what they were saying and doing. Therefore, she adjusted her writing strategy so that: "Eventually, I put {it} away ... and carried 3"

x

Figure 1.

A Field Diary Extract¹...

¹. Here, all proper names have been blanked out in an attempt to preserve the anonymity of those involved.

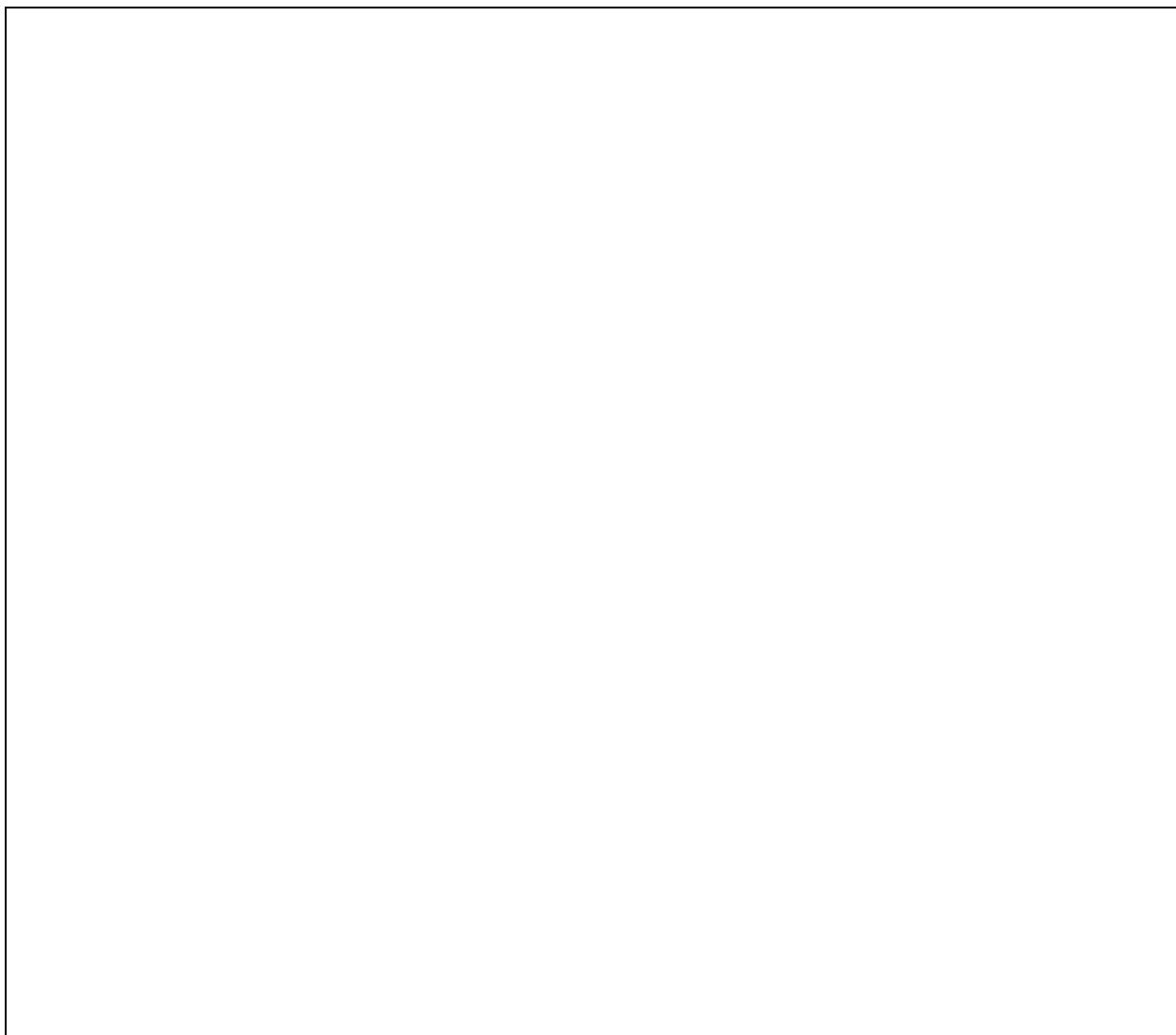


Figure 2.

... as word-processed later for analysis:

Unlike the previous hand-written entry, the version below does not contain the sketch of the watch above but does contain pseudonyms for the people and places involved in an attempt to preserve their anonymity. When using the latter, it is a good idea to draw up a table of real names and their intended equivalents, and to refer to this at all stages of your analysis. Indeed, it is probably better to analyse your diary and interview materials with the real names in place - in order to avoid the constant distraction of having to translate from one to the other when making sense of your materials - and to then substitute the pseudonyms at a later stage. See later for a discussion of the (time/space) diaries mentioned in the first sentence which were kept by 'Cerene' and five other packing-house workers who lived in 'Ibrox', along with their supervisor and the manager of the farm (page 55). Here, the events mentioned were important to note because none of the workers involved possessed a watch or a pen to note in their diaries the times when

certain things/events happened, so Ian gave them one of each. This simple event led to a number of unexpected reactions which had to be noted down because they had an effect on the research process as, here and elsewhere, Ian by no means blended in to 'observe' the day-to-day workings of the farm's packing-house unnoticed.

.....
 .
Saturday 30 May 1992

Too bored with myself last eve to write anything, despite the importance of it being the first big day of diary stuff. Yesterday, I trooped off to the packing house at about 9.30 (probably after an hour or so of reading and sorting out tapes and forms - actually, it's probably unreasonable of me to expect those keeping diaries for me to remember much from the day before if I can't), stood by the end of the packing bench looking around to see if Emilie, Lana, Gloria and/or Pru were there. As I did this Cerene, at the other end of the table with Vivette (I think) was looking at me, calling me over. So I went. She asked me how come I had not given her the watch and pen? She lived in Ibrox? So I said something like "OK. I was going to ask you" and suggested that we talk about it in the canteen. As we walked over, I told her that I had meant to catch her yesterday but didn't find her on her own. If I asked her in a group, I didn't want others asking me for a watch, too. As we sat down, I unzipped the small pocket of my pouch (which formerly held the small microphone which I broke) and took out the watch. Having given the others the choice of watches, this was the remaining one - a kind of square clunky LCD watch whose face and strap were fashioned after army camouflage (not Desert Storm) and the watch face had 2 thin metal bars to supposedly protect it from the brutality of War, ie..(sketch of watch). Anyway, I was a trifle embarrassed about this watch - I thought it was easily the most hideous of the 6(5?) before showing it to anyone in the packing house, and then nobody chose it - even after I gave it to Gloria the day before when hers was showing the wrong time and I couldn't mend it: she gave it straight back when Baldwin had done the business - using the badly chosen fine fibre tip pens...

5" cards in the pocket of my white coat or operating room scrub suit; I took as few notes as possible, scribbling a few words every once in a while on the white cards, using them as mnemonics for each night's session at the word processor" (*ibid.*:96). Other researchers have made sudden and frequent trips to the loo to write things down - symptomatic of the so-called "ethnographer's bladder". Whatever the case, even if community members accept and involve the researcher in their lives in one way or another, having been told what they are 'up to', this is unlikely to be forgotten even when the researcher thinks that s/he has successfully 'blended in'. Michael Keith (1992), for instance, found himself in the following situation:

"Having at {one} time spent several nights with {one} pair of {police} officers we had got to know each other 'reasonably well' (an oxymoronic phrase?). We had got on well, finding some issues of common interest, though my usual reserve topic of conversation, football, was of no interest on this occasion. This had not stopped us going out drinking together outside

work hours. Apart from the football, the routine facets of male bonding in a gendered research methodology were readily present.

However, getting onto politics was a big mistake. Some interests might have been shared, but politics clearly were not and in the early hours of the morning silence followed a disagreement that was capped with the comment: 'So what are you going to do once you have finished with us then? Write up your horror stories about the brutality of the police in London in a book then or just put them in stories in one of those left wing newspapers?'" (554-555).

This begins to raise issues of fairness of representation and right to reply, and we would draw your attention to three examples of how this has been treated. First, Graham Rowles' (1978a) ethnography of an ageing community in a North American city contains one chapter written by one of his informants about himself and his involvement in the research. Second, Paul Willis (1977) gave his completed ethnography to the people who had been his key informants, asked them to read it, then tape-recorded and transcribed their opinions on it at a meeting with him, and included the transcript as an appendix to his book. And, third, on sending a draft text to her informant, Katherine Borland (1991) found that she strongly disagreed with being characterised as a 'proto-feminist' (a situation made perhaps more difficult because her informant was also her grandmother), and it was only after repeated exchanges that a mutually acceptable text was produced.

(iii) INTERVIEWING.

Along with participant observation, interviewing has been a primary means through which ethnographic researchers have attempted to get to grips with the contexts of different people's everyday social, cultural, political and economic lives. As a means of gleaning information from conversations within various research communities, interviews can range from the highly structured (akin to questionnaire survey in which the researcher asks pre-determined questions in a specific order), through the semi-structured (where the researcher and participant[s] set some broad parameters to a discussion), to the relatively unstructured (akin to a friendly conversation with no pre-determined focus). These approaches have, in turn, been allied to various types of research project involving, at one extreme, the generation of numerical 'data' from one-off visits to tens, hundreds or thousands of people which are then *statistically* analysed and, at the other, the recorded conversations between a researcher and the handful of her/his

research participants which unfold over a number of visits ready to be transcribed and *discursively* analysed. And, as with much of the writing on participant observation, that which discusses interview techniques has been similarly torn between treating interviews as ways in which the 'detached scientific observer' can *extract* relatively 'unbiased data' from her/his interview 'subjects' or as ways in which the researcher and researched participate on a much more equal footing in relatively frank discussions from which the development of *intersubjective* understandings is actively encouraged. Moreover, such interviewing can by no means be treated separately from the other approaches in this booklet. In Ian's experience, for instance, some 'interviews' have been exercises in participant observation with the tape-recorder switched on (see Rowles 1978a&b) and/or have been based on questions which arose through participant observation work. Still others have ended up as group interviews when other people have been (unexpectedly) present and joined in with the conversation and/or have involved looking at interviewees' photograph collections and asking about who was in certain pictures, why they had been taken, and so on (Becker Ohrn 1975; Collier & Collier 1986).

The key questions contemplated by researchers who have not done any ethnographic interviews before usually concern the nuts and bolts of arranging and conducting them. How on earth do you approach (almost) complete strangers and persuade them to talk to you about their thoughts, feelings and actions? How do you decide how many interviews you would like to arrange with the same person and how do you fill these up? Once such meetings have been arranged, what sort of questions should you ask so that you can get at the information and stories you are interested in without embarrassing or offending your interviewees, or yourself, in the process? And, in asking such questions, how do you decide what is and is not any of your business? These questions have to be thought through each time you approach potential research participants for 'initial interviews', i.e. those approaches you might make to various 'gatekeepers', those you expect or want to be just one-off meetings with particular people, and those which you hope will become the first in a series of interviews with the same person over a period of time. All such encounters require some care in setting up and seeing through and, as with participant observation and group work, this is again a matter of making compromises between what it takes to gain access to, and to maintain contact with, potential research participants (not doing anything to make yourself *persona non grata* for subsequent interviews with the same or other people in the communities under study) and what it takes to continue addressing your research concerns in this context (asking questions which you believe are important to your project, but

which may prove difficult, awkward or embarrassing to your interviewees, in a manner which will not offend them).

a) Making arrangements.

When setting up initial interviews, many researchers feel most comfortable going through a reasonably formal, businesslike, procedure which goes along the following lines. When a good contact is suggested from whatever source, get her/his address and phone number. Then write to introduce yourself, your institutional affiliation, what your research is about; who suggested that s/he be contacted as someone who has important knowledge about this subject; the fact that you would therefore like to talk with her/him at a mutually convenient time and place; that you would like to record the conversation; that everything said would be treated, if necessary, in the strictest confidence; that all efforts would be made in the final text to mask her/his identity, and that you will telephone shortly after s/he receives the letter to arrange a possible appointment. Most researchers find an hour to be sufficient for a single interview in that it is long enough for some rapport to be established and to enable the discussion of a range of issues, while being short enough to be 'user friendly' for most interviewees.¹ Then, in the couple of days after sending the letter, telephone the person, remind her/him of your letter, ask if s/he would mind talking with you, and try to arrange a meeting at a mutually convenient time and place. Once this is set, telephone her/him again a day or two before the meeting to confirm that s/he will be able to attend. On the day of the meeting, try to dress in such a way that will allow you to close some of the distance that may exist between yourself and your interviewee, and turn up early. Then, in the few days after the interview, write and thank the person for her/his time and remind her/him of any names, addresses, data, references, and so on which s/he may have promised to supply you with during the interview, and perhaps suggest that a follow-up interview might take place some time in the future (Fetterman 1989; Thomas R 1993).

As part of this protocol, careful consideration should be paid to when and where these interviews take place. As we argued in the section on 'conceptualising the subject', it is important to understand how various facets of people's identities are very much immersed in the different spaces and places of their/our lives. So, in Ian's initial interviews with three of the four people who took part in his blindness research, the fact that these took place in their homes became important to the

¹. However, once underway it is quite possible for a meeting to go on for two or three hours, so always be prepared with extra batteries and tapes; and, if this is a good interview, don't call time yourself - let your interviewee do it.

outcome of his research because the homeplace provided a form of reference and reminder for both interviewer and interviewee alike. As stories were elicited about each person's day to day life in that place, illustration and corroboration could easily be made through reference to objects near at hand. Moreover, arranging an interview in such a meaningful place for the interviewee can provide a basis for asking questions which do not entirely refer to what s/he says - e.g. through asking questions such as "why have you arranged this room in this way?", or "have you kept photographs of your family?"

When arranging meetings in such a manner, then, the selection of an appropriate time and a place must be considered at an early stage. If you have a preference, it is a good idea to state this in your introductory letter but, particularly if you suspect that your potential interviewee might have a problem with this, always suggest an alternative and/or negotiable time and place to meet. Whatever the case, it is important to bear in mind that the ability to arrange meetings with appropriate people in appropriate places can have an important bearing on the outcome of your research. As Andrew Herod (1993) has argued, any amount of reading that a male researcher may have done in order to focus in on a particular research topic may be wasted because "many women understandably are often reluctant to invite unknown men into their homes {so t}his can pose problems for male interviewers conducting research into, for instance, the economic geography of industrial homework since the overwhelming majority of homeworkers are female" (309). Similarly, in Ian's case, the importance he attached to interviewing legally blind people in their homes meant that, although he gained valuable insights from his male participants, he had (a largely unexpected) difficulty in involving women in his study: his first, female, participant initially agreed to take part but later withdrew saying that she was very busy, that she gave her husband priority in her free time, and that if Ian was to visit her house he was in danger of being attacked by her doberman; and a second female participant would only be interviewed on the university campus or at home over the phone (yet after much discussion, she suggested that they try a telephone interview which she would record on her answerphone and would then send him the tape). Therefore, although he was aware that there were important differences between the experiences of legally blind women and men (Asch & Sacks 1983), he was not able to explore these in any great detail, and thus his understandings were very much of masculine blindnesses (albeit differentiated by age, class, levels of visual impairment, biography, and so on). Again, as we have argued throughout this booklet, such necessary compromises between what is feasible and what may be significant highlights the

importance of not separating the so-called 'stages' of reading and doing as your ethnography progresses.

Having so far talked only about interviewing people in their homes, it is important to realise that this is by no means the only place where an interview can or should take place. Not only is it very possible that your research project will not have a domestic focus, but you (as well as your research participants) may also feel nervous or vulnerable in the presence of a stranger in such a private space. Many female researchers, for instance, are reluctant to interview strange men alone in their houses.¹ A compromise for researcher and researched alike may be to arrange interviews in a more public space which the latter's life also flows through, such as a place of work or leisure. Interviewing people in these locations, firstly, acknowledges that most people's lives are stretched between a number of different locales which can therefore serve as references and reminders of different aspects of their identities, and secondly may provide places where both interviewer and interviewee can feel relatively at ease in each other's company. Indeed, following on from this, it is also worthwhile to point out that interviews need not involve sitting and talking in a person's home, workplace, leisure space or any other fixed setting. Given both the nature of certain research questions and the practicalities of arranging meetings, there can be considerable room/necessity for invention, here. As part of his blindness research, for instance, Ian arranged to accompany three of his research participants on a walk along one of their well-trodden paths through the city in which they lived in order to ask them, in this kind of 'travelling situ', how they negotiated these spaces. Similarly, Graham Rowles (1980) interviewed an elderly woman in his car as they drove along the roads of her childhood. Conversely, in order to hold an interview away from workday distractions, Ian came to the conclusion with one supermarket executive that the best place for them to hold a discussion was in his car on his journey to and from work and/or to or from his various visits to stores, depots and suppliers. However, when the initial interviews with this executive took place in his office cubicle and *were* interrupted by phone-calls, unexpected visits from various reps, and so on, this was far from disastrous as they provided first hand access to the very stuff of his daily business and were, again, an unplanned blending of participant observation and interview approaches.

b) Preparing a checklist.²

¹. In using such examples here we by no means wish to imply that female researchers should only consider interviewing women, or that male researchers should only consider interviewing men.

². This may also be known as an 'interview schedule', 'interview prompt' or 'topic guide'.

Once an initial interview has been arranged, it is common for this to be conducted in a fairly formal manner in which, before it commences, the researcher has done a certain amount of background reading, has set out a clear agenda of issues to be covered in the meeting and then uses this as a checklist which s/he then tries to steer the conversation through in whatever order. This, it has been argued, serves to ensure that the researcher always meets her/his objectives in each interview, that there is some equivalence across them, and that interviewees are, to a certain extent, allowed to raise their own issues for discussion and potential inclusion in the researcher's *continually modified* checklist of questions (Burgess 1992a; Ostrander 1993). In particular, this approach has been put forward as vital, first, to 'corporate interviewing' where researchers are often spared only perhaps an hour or two with busy executives to find out about their company's decision-making processes and/or changing involvements in different markets (McDowell 1992; Schoenberger 1991, 1992),¹ and, second, to life history work where a broad knowledge of events and institutional structures which are likely to have had an impact on a person's lifecourse can also be an advantage, a situation enabled particularly if the interviewer has to a certain extent also lived through these (Miles & Crush 1993).

So, how, where and why can/should background research be done for such interviews? Erica Schoenberger (1991) has suggested, for instance, that in corporate interviews it is necessary to know about a firm's business strategies, relationships to its markets, production technology and methods, labour relations and the behaviour of its competitors. So, in the early stages of such a project, it is not unusual for researchers to pick up on an issue which has gained prominence in the financial press and/or national or local media which is directly related to their/our academic interests. To gain further background information, most issues can be further traced through back-copies of newspapers and magazines which are held in many libraries, and/or from annual reports, market surveys, various kinds of corporate literature and articles in the trade press. Annual reports are usually available for reference in local business libraries and/or can be obtained directly from the companies concerned who will often send backdated as well as current versions if requested. Market surveys, corporate literature and trade press articles, though, can take some time to hunt down as insider knowledge is often needed to find out what is available. When starting out on his 'corporate' fruit research, then, Ian found that extremely useful places to visit were the City Business Library in

¹. Although, see Nash (1979) for her *serial* corporate interviewing.

London¹ and the Jamaica Promotions Corporation (JAMPRO) Library in Kingston. By simply going up to their counters and stating what business he was interested in, he was directed to sections containing various market surveys, feasibility studies and statistically-laden reports covering levels of fruit production, consumption, imports and exports over a considerable period of time. Furthermore, after initial interviews with UK fruit executives in which he specifically asked what trade journals they referred to, he was directed to publications and, with one company, to a file of articles on exotics which they had kept and allowed him to photocopy on the machine in their office. Moreover, with these same people, he also asked if they had gained their information about these fruits from any particular books, asked to see them, and copied down their titles in order to order them through the interlibrary loan system. And, finally, with the help of both his friends and the executives who he met, he also collected and read the various exotic fruit recipe leaflets which each supermarket chain had produced for distribution in their stores. So, again, rather than being a discrete stage of his research to be conducted *before* doing his interviews, much of this reading was, and could only have been, done *as* the interviews progressed over a period of eighteen months or so.

According to Erica Schoenberger (1991), such background preparation is essential for making the most of the limited time which executives are often willing to set aside to talk with researchers. She has argued that, through being able to ask knowledgeable questions and to have an understanding of a trade's technical terms, researchers can, first, reassure their interviewees that they understand the issues and thereby encourage more open and detailed answers; second, invite the kind of responses as they would usually be worded in the office rather than asking questions in such a way that interviewees have to translate their responses to approximate the researcher's language and frame of reference; third, be more able to spot and question any issues which the interviewee may either have glossed over or contradicted her/himself over; fourth, ask specific questions about their competitors' strategies in similar markets to encourage a detailed comparative approach;² fifth, build the kind of detailed problem-solving questions into the interview which executives may be most accustomed to dealing with in their daily work; and, finally, use this knowledge as a form of power to redress, at least partially, the usual imbalance which favours an interviewee who is "accustomed to being in control and exerting power over others" (*ibid*:182; McDowell 1992). In

¹. City Business Library, 106 Fenchurch Street, London EC3 (Tel: 071 638 8215). Also worth a try is the Newspaper Library, Colindale Avenue, London NW9 5H (Tel: 071 323 7353).

². Something which may be enabled by the fact that many executives have, at one time or another, worked for their current competitors.

other scenarios, though, particularly if interviewees may be more used to being in the position of supplicant when faced with a typical middle-class, university-educated, ethnographer, this background knowledge needs to be used in a much more subtle and hesitant manner by the researcher in order to redress power imbalances which may well be skewed in the other direction.

c) Asking the 'right' questions.

As with the other approaches discussed in this booklet, in interviewing the initial research encounter can be an anxious time for both interviewer and interviewee as each has to carefully weigh up the other before, during and after the conversation. What is each person prepared to reveal about themselves in the form of questions, responses, and responses to responses, how are these likely to be received, and what is motivating the other to take part in the research? Whoever you interview and whatever preparations you have made before doing so, if you are a first-time interviewer, you are likely to find your initial interviews rather stressful and, instead of encouraging a conversation through which the expected issues on your checklist (as well as the unexpected ones which emerge in the course of the conversation) can be easily threaded, you may end up nervously going through your list almost as if it were a questionnaire. To prevent this happening, it is a good idea to prepare to ease into the conversation by remembering to exchange pleasantries, to introduce yourself, to ask where you should sit, to confirm the topic of the interview, to explain why you want to talk to her/him about this issue, to go through a standard ethics protocol (if you have decided to use one, see McCracken 1988b:69), to ask if s/he minds if you use a tape-recorder (you can emphasise here that note-taking is slow and distracting and that taping conversations minimises the risk of misunderstanding and misquoting, but do not be surprised if s/he is reluctant to be taped) and to remind yourself to do these things by writing them down as *instructions* at the top of your list. To begin the interview, it is a good idea to ask a *prepared* first question as a means to combat any nerves that you may have at the start of the meeting. Then it is usually better to introduce subsequent questions/issues more 'naturally' into the flow of the conversation, although you may well need to have outlined key phrases, questions, 'facts', and so on in your checklist to get them right.

These early stages of an initial interview should not be arranged solely to combat *your* nerves, however, as those of the interviewee must also be taken into consideration. As Grant McCracken (1988b) has written:

"Whatever is actually said in the opening few minutes of the interview, it must be demonstrated that the interviewer is a benign, accepting, curious

(but not inquisitive) individual who is prepared and eager to listen to virtually any testimony with interest. Understandably, {interviewees} are not keen to reveal very much about themselves, or to take a chance with an idea, if there is any risk of an unsympathetic response. {Interviewees} must be assured that the potential loss-of-face that can occur in any conversation ... is not a grave danger in the present one. ... It is better here to appear slightly dim and too agreeable than to give any sign of a critical or sardonic attitude" (38).

Your questions will usually need to be of a non-threatening kind, then, and the standard approach is to begin by employing so-called 'grand-tour' questions (Spradley 1979) which ask the interviewee to outline the general characteristics of the place and/or social networks which s/he is involved in and which you intend to research. Through asking simple 'who?', 'what?', and 'where?' questions about the topic in hand, the basic grounds for your conversation can be established. And, given that the main aim of interviewing in ethnographic research is to allow people to reveal their own versions of events in their own words, it is important to ask follow-up questions in such a way as both to encourage and to critically question the stories told.

When the researcher is able to get her/his participants talking about certain issues, the stories which are then told are often not simply made up on the spur of the moment but, rather, are those which have been told, retold and refined on a number of occasions, in a number of places, with a number of different audiences. Therefore, instead of taking the stories as told, it is important to ask questions which will allow the teasing out of the contexts in which such stories are usually told. So, for instance, to establish how your interviewee has become, and is, involved in the situation under discussion, you can ask questions which refer to what s/he has stated such as 'what do you mean when you say ...?', 'why did you do that and not something else?', or 'how did you get involved with those people?', and so on. In order to ask critical questions without appearing to criticise, you can ask questions such as 'is it true that ...?', 'what do you think about the critical coverage that this issue has recently had in the news - did you see the piece about...?', or 'how does this relate to what you said earlier about ...?', and so on. In order to get beyond blanket statements about a subject, where possible ask for examples of when this affected the person directly. Finally, if you think that you have asked an inappropriate question, simply apologise and/or say '... or is that none of my business?'

On top of these different forms of relatively non-threatening question, you will also need to develop skills of keeping a conversation going, many of which you are

likely to have grown up with anyway as means of holding everyday conversations. Thus, for instance, you will need to pay close attention to what the person is saying in order to ask follow-up questions based on this (noting these down as they come up can help remind you to go back to them when the person finishes a particular account) and, when possible, you can use these to introduce themes from your prepared list if and when they are skirted in the conversation. To check that you have understood an argument that s/he has made, it is often a good idea to put it in your own words and ask if your understanding is about right. You may also have to resist the temptation to nip your interviewee's digressions off in the bud as these may lead to unexpectedly interesting insights. And, you may consciously have to stop yourself jumping into pauses in the conversation with new questions, since it is often a good idea to allow such pauses to go on a little longer because your interviewee may simply be thinking about the question and/or about her/his first attempt to answer it before having another go (Burgess 1992a; Stewart & Shamdasani 1990). At the same time, though, this may simply be the result of your daft, ridiculously wordy, inappropriate or otherwise awkward question. So, if and when you consider such a silence to have become overly long and you cannot think of a question relating to what your interviewee has already said, this may be the time to introduce a new theme from your list.

As part of this keeping-a-conversation-going, you should not be overly concerned about covering all the points on your checklist within the allotted time for your interview as this may result in a 'panic interview' where speed becomes all important and time is not allowed for rapport and intersubjective understandings to develop at a relaxed pace. Faced with this situation on many occasions, Ian has said to his interviewees as their allotted time came to a close that there were still a number of issues that he had wanted to ask her/him about and wondered if it would be possible to arrange another meeting to discuss them (indeed, he made a point of preparing checklists which covered all of the issues he wanted to bring up, regardless of whether it might be possible to cover them in a single, hour-long meeting). And, finally, unless the topics which you thought your interviewee may be least comfortable talking about are broached by her/him earlier on in the conversation, leave them to the end as, in the likelihood of her/him taking such questions as 'unsympathetic' and thereby becoming uncooperative, at least you will have some 'cooperative' dialogue on tape which you will be able to work on later.

It is important to appreciate that many interviews which end up being quoted in academic texts have not been fully transcribed, complete with questions as well as answers, mumbblings, misunderstandings, repetitions, embarrassing ideas, directive questions, warts, and all, often because of the prohibitive amount of time it takes to

do this but also because researchers often fall foul of wanting to tidy up what is already regarded by many as an overly 'subjective' and messy approach to constructing knowledge. The resulting neatness and order of such representations in the published literature can therefore serve as another means of making the first-time interviewer feel somewhat inadequate as s/he bumbles through her/his checklist. But, while it may be extremely counterproductive to ask a barrage of *directive* questions (i.e. those which imply that you have already made up your mind about the answer, e.g. "how badly do you exploit your workers?" or "you must really enjoy living here, right?"), blurting out one or two of these accidentally in the course of an interview is not a disaster and may even lead to interesting responses. Most researchers' interviewing skills and confidence improve with experience, but these rarely gravitate towards any 'standard' approach because the contingencies bound up in different research projects and the people likely to become involved in them varies so much. Given this, a number of researchers have argued that the accidental or deliberate flouting of the 'rules' is not necessarily always a bad thing because much can be learned from reactions to this. Any researcher new to a particular cultural scene may, for instance, not have the right cultural competence or cash to dress appropriately (McDowell 1993). In this context, Robert Thomas (1993) has mentioned the conservative, navy blue suit which he routinely wore to interview corporate executives for his research. When he visited one particular company, though, this became an issue as, "I found that I stood out like a sore thumb by comparison to the pullover sweaters and slacks which were the norm. ... several times ... people chided me for dressing too much like a consultant - a comment that was tantamount to an insult" (93). Rather than this being a problem of 'biasing' his information ('away from what?' should always be the question), his accidental flouting of a dress code drew attention to its importance in that environment and therefore opened up a potential line of inquiry which might otherwise have been missed. Other researchers, instead of trying to minimise this kind of reaction, have played it up deliberately seeking to do the 'wrong' thing in the 'wrong' place among the 'wrong' people in order to disrupt such taken-for-granted rules and to try to understand their intricacies and influences (Garfinkel 1984; Giddens 1984, 1991). Whatever the case, though, it is vital to understand that, as mentioned already with respect to participant observation, there is no ultimately 'neutral' scientific identity which the researcher can attain. Rather, to a large extent an interview style appropriate to your topic and to the people you talk with about it will emerge over a period of time through self-critical experience (Rowles 1978b) Moreover, because initial interviews are always situated in these ways, they can only allow a partial glimpse of the interviewee's life, not only in

terms of the time available for rapport to be established between interviewer and interviewee and for stories to be told, but also in terms of how the place where this takes place will be one of many in which that person's identity has been immersed throughout her/his life. These limits, then, need to be taken into account when considering what you want to get out of any interviewing which you might do.

d) Serial interviews.

The main difference between arranging a series of multiple interviews with the same people and a range of single interviews with many more is that, after repeated visits with the same person over a period of time, the quite formal interviewing style discussed above may/should dissolve. Here, interactions which are much more like informal conversations can usually be developed in which both parties feel more able both to reveal their often undecided, ambiguous, and contradictory feelings about the matter in hand and to challenge each other about these in an atmosphere of mutual respect and trust. And, it is this 'atmosphere' which is the most valuable product of this approach to interviewing. However, these interview series can come in a variety of forms: as overflows from single interviews where time ran out before the issues on the researcher's checklist had all been discussed (as was often the case with Ian's 'corporate interviews'), visits with the same people over a period of time to follow their lives as they relate to the topic under consideration, and/or similarly-arranged series which researchers have used to try to get at the taken-for-granted aspects of interviewees' everyday lives.

In terms of arranging serial interviews, though, while 'overflows' may be arranged very much on an *ad hoc* basis and can provide the interviewer with not much more than a chance to finish off her/his checklist (although the ability to supplement it after having listened to the recording can make this a good idea), most interview series are proposed to potential interviewees as a block at initial meetings. Taking this latter situation, in addition to going through the kinds of protocol mentioned previously here, you should also clearly set out what you want to discuss, why this will require a series of meetings, how long you would like this series to go on for, how much time it will be necessary to set aside for this, what each session will hopefully involve, and that your interviewee has the right to withdraw from the series at any time without any need to justify this. As Jacquie Burgess *et al* (1988a&b) have argued about the convening of serial focus groups (see later), this approach provides a clear structure for researcher and researched alike in which clear parameters to the research encounter are established, therefore enabling an easier withdrawal from such a relationship for both parties (although friendships may develop between researchers and researched which can carry on

informally once the research is over) and the construction of a set amount of ethnographic data with which to work. Even after setting out such provisos at an initial meeting, though, it must be realised that the commitment which is required by both parties, not to mention any anxieties that interviewees may have about promising to attend repeated meetings with such an inquisitive stranger, may mean that getting people to agree to take part in this kind of work may be more difficult than gaining consent for an initial meeting. Again, the advice here is to try to be sensitive to the reasons why some will turn you down while others will not, and to try to gain some appreciation of the dynamics of positionality, intersubjectivity and the partiality of knowledge which will affect what you consequently learn.

One of the most important considerations when setting up serial interviews is that, depending on the time and resources available to you, the numbers of participants and meetings involved have to be set off against one another. To give an impression of what these numbers can turn out like, Ian's research with legally blind people involved four participants, one interviewed on three occasions as part of a pilot study and a further three interviewed on ten occasions for the main body of the research; Graham Rowles' (1978a) research with elderly people involved five participants who were interviewed on an unspecified number of occasions; Steve Pile's (1991) research with dairy farmers involved six participants who were each interviewed on twelve occasions; while Anne Oakley's (1981) research with impending mothers involved fifty-five participants who were each interviewed on four occasions.¹ When weighing up how many participants you want to recruit and on how many occasions you want to meet with them, what is usually sacrificed in terms of a breadth of experiences is more than made up in terms of a depth in understanding. And it is this potential 'depth' aspect of serial interviewing which has had two major attractions for ethnographic researchers. First, it can allow, at least in part, a dismantling of the hierarchy of knowledge between researcher and researched which is often at work in questionnaire and other 'initial' interviews where both parties by no means participate in the construction of knowledge on an equal footing (Herod 1993; Oakley 1981). And, second, serial interviews can also enable research encounters in which there is sufficient time, space and trust to plumb the depths of people's taken-for-granted lifeworlds in order to study actions and feelings which, if they ever reached the light of day in an initial interview, might be difficult for either party to enunciate or to reflect upon in any sustained and detailed fashion (Rowles 1978b).

¹. Here it is important to note that, although Oakley conducted most of these interviews herself, she was able to employ a research assistant - something which most readers of this booklet will no doubt be unable to afford.

To elaborate on this first 'attraction', a number of writers have criticised traditional accounts of interview methods (which most closely approximate the advice given here on conducting initial interviews) because they advise researchers to develop a rapport with their interviewees which is sufficient to elicit responses without closing what many have seen as the "necessary social distance {which} ensure{s} that she/he does not 'bias' the outcome of the interview by interjecting personal opinions or values" (Herod 1993:309). Researchers interested in conducting both single and serial interviews, then, have been instructed to employ a number of tactics which can minimise this 'bias' ('away from what?' should, again, be the question) and, in particular, to *prevent* themselves from making interjections in the face of the very common and awkward situations when interviewees ask questions back. So, for instance, Anne Oakley has pointed out how such advice has often gone along the following lines:

"Never provide the interviewee with any formal indication of the interviewer's beliefs and values. If the informant poses a question ... parry it.' 'When asked what you mean and think, tell them you are here to learn, not to pass any judgement, that the situation is very complex.' {Or,} 'If he {sic.} (the interviewer) should be asked for his views, he should laugh off the request with the remark that his job at the moment is to get opinions, not to have them'" (1981:35).

Yet, as a feminist researcher involved in quite intense and highly revealing interview series which developed over a period of time as she followed a number of women through the latter stages of their first full pregnancies and into the few months after they gave birth, these tactics seemed morally reprehensible to her because they simply advised that "...such questions as 'Which hole does the baby come out of?', 'Does an epidural ever paralyse women?' and 'Why is it dangerous to leave a small baby alone in the house?' should be fobbed off" (*ibid.*:48). So, at least in these kind of situations where interviewers may be in a more powerful position in society *vis à vis* their/our interviewees, she has argued that researchers should not only *admit* that they contribute their own ideas and feelings into such conversations, but that they are *morally obliged* to do so as part of a necessary dismantling of the traditionally hierarchical and exploitative research encounter. For these reasons, then, researchers who are (or become) concerned with such ethical/moral issues in their work may feel more at ease interviewing relatively few people on a number of different occasions in order to try to develop such relationships.

The second major reason why many ethnographic researchers have chosen to use serial interviews in their work is because they can allow time for researcher

and researched alike to begin to think about, explore, and make sense of the contradictory, inconsistent and taken-for-granted natures of their/our everyday lives. In response to questions during an *initial* interview, most interviewees will not come up with concepts, stories and arguments which they have not (recently) thought about and/or told before. Rather, these will more than likely have been pieced together, put forward, argued with, transformed and retold in different versions in the multiple contexts of her/his biography and everyday life. Thus, if these responses are likely to contain the kinds of discourses in which you are most interested, single interviews can be quite sufficient for your needs. But, unless your interviewee's statements have been very highly 'rehearsed' - a situation which can be the case if your interviewee is highly accustomed to talking about her/himself - s/he is unlikely to make perfect sense. In this situation, serial interviews can allow you the time and the opportunity to discover why this might be the case because, even if not noted down and/or questioned as they come up in a conversation, such niggling issues can be broached at subsequent meetings. Explanations are not usually provided by the interviewee after repeated questioning on the same issue, therefore, but emerge through dialogue with the researcher which develops over a number of visits. Here, sufficient knowledge and trust can be developed for both parties to speculate on, and to discuss, what the more deeply-rooted reasons for thoughts and actions might be.

This potential move from rehearsed and/or one-line explanations to thoughtful introspections and sensitive explorations of the times, places and social/cultural/political processes from which the identities of both interviewee and interviewer have emerged is one of the great strengths of serial interviews because it allows explanations of the taken-for-granted routinisations of people's everyday lives to be constructed. As has been amply illustrated in the ethnographic work of geographers such as Graham Rowles (1978a&b, 1980, 1983) and David Seamon (1979; Seamon & Nordin 1980), as well as in more theoretical work such as that of Anthony Giddens (1984, 1991) and Nigel Thrift (1983), the vast bulk of 'knowledgeability' which lies behind what people do on a day to day basis does not operate at the level of 'discursive consciousness' (i.e. where everything has to be consciously put into words before being perceived, felt and/or done) but, rather, operates at a far more routinised level of 'practical consciousness'. In this 'practical consciousness', it has been argued that people's everyday lives are: first, routinised in physical space and necessarily involve taken-for-granted 'body awareness(es)' of every detail of the physical configuration of the places travelled through on an everyday basis (Rowles 1983), and that these allow them/us to make elaborate manoeuvres through many complex environments without giving them much

thought (Seamon 1979); second, people's everyday lives are also routinised in terms of social interaction, which necessarily involves taken-for-granted rules of behaviour which give an intelligibility and predictability to everyday life (Giddens 1984, 1991; Thrift 1983); and, finally, the routinisation of practices in meaningful places also allows people to maintain taken-for-granted senses of autobiographical continuity given that who and where they/we are at any one time are, as we have argued previously, very closely intertwined (Rowles 1980, 1983).

Given that so much of this knowledgability is somewhat buried in 'practical consciousness' and can often require a considerable amount of introspection and speculation to begin to make sense of it, bringing this out for discussion can be enabled by giving serial interviews a certain tactical structure. Both as a non-threatening start to a series and as a way of getting an impression of how a person's everyday life may be rooted in other places, times and social relations, it is often a good idea to begin with a general discussion of her/his 'life story'. As a first interview, this does not have to involve as much homework or to be as analytical as, perhaps, a corporate interview. Rather, researchers can simply encourage their/our interviewees to talk about themselves both in terms of the more 'factual' aspects of their biographies - where and when they were born, what their parents/guardians did for a living, where and when they went to school, what they did subsequently and where, and so on - as well as their reasons for and feelings about them both at the time and perhaps more recently.

At this stage, the interviewer's task can be little more than that of asking very general questions in order to get the person talking ("where were you born?", etc.), occasionally putting aspects of these stories told back to her/him as questions ("do you mean that, after you did that, you moved straight away to live there, or did you wait a while?"), asking follow-ups when certain details are mentioned but not explained ("who was she? how did you meet her?", "what is this [unknown object/term]?"), inviting speculations about paths *not* taken ("why did you choose to do that rather than something else?"), and asking to see any objects, texts and/or photographs (see later) which would help to illustrate these stories. In taking this approach, then, researchers should not only be able to 'break the ice' and to get to know interviewees in a relatively non-threatening way, but should also be able to encourage the telling of a wealth of stories which both parties will be able to refer back to in subsequent discussions when trying to make sense of other issues.

This attempt to make sense of how the person has lived her/his life *into* the issue(s) you are interested in can be followed up in subsequent meetings either by concentrating on how they make sense of events and issues in a longer term perspective through their biographies and life-histories (Geiger 1986; Gluck &

Patai 1991; Holland 1991; Miles & Crush 1993; Personal Narratives Group 1989; Portelli 1981), or in the shorter term through meetings which focus on her/his present everyday life. Here, to encourage detailed discussions of activities which will often be very much taken-for-granted, a number of other tactics can be employed to bring these out. Many researchers, for instance, have asked their participants to keep *activity diaries* over a period of time in which they are asked to record various mundane details of their everyday lives. Here, narrative accounts may provide fascinating insights, but it is possible that asking for this kind of diary may prove intimidating for many participants because of a reluctance or a lack of time to write one out. Another approach which can be taken is one in which, for every day of the period in question, the researcher provides her/his participant with a blank table on which to record certain details about their everyday activities. In both his blindness and fruit research, for instance, Ian has asked his participants to fill out tables with the following headings:

Time of day.	What did you do?	Where did you do it?	How did you get there?	Who was with you?
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Once filled in, (in practice, often by researched and researcher together¹), these tables can then be used as a *basis* for discussions of the taken-for-granted aspects of a person's everyday life rather than as a simple table of information.² Through enabling the discussion of the bare bones of each day, these can be fleshed out by asking for *explanations* of the whens, whats, wheres, hows and with whoms of the person's day, encouraging comparisons with other diary days and/or with other, perhaps more distant, times and places in her/his life, and asking about her/his feelings about such relations. Thus, as this kind of interview series progresses from an initial biographical discussion through a week of daily activity diary meetings (Ian has found that diaries lasting any longer tended to become repetitive and boring for both parties), this can enable the development of a *relatively* non-threatening, non-hierarchical research relationship. In doing this, both parties may well come to feel comfortable making debatable arguments and connections between aspects of various accounts made and can try to *understand* any apparent inconsistencies and contradictions in them rather than seeking to distinguish between the 'true' and the 'misguided'. On top of this, interviewees can often develop sufficient confidence to express themselves without fear of criticism, to

¹. In both of Ian's research projects, in the course of the daily diary discussions these tables were either filled in by the participant and then annotated by him, or he filled them out entirely.

². Although they can be used to construct time/space diagrams of everyday activities (see Rose 1993; Thrift 1983).

disagree with the researcher's interpretations as they surface in discussion and, if/when the researcher takes notice of these, to play a much greater role in shaping the course of the research.

As far as the structure of such a series is concerned, it is important to tailor these to achieve some kind of balance between the general issues in which you are interested and the more idiosyncratic biographical and everyday contexts in which these will have been embedded and interpreted throughout the courses of your participants' lives. Moreover, as mentioned earlier, such considerations should also include making decisions over where and when these interviews should take place in order more easily to approach certain aspects of a person's identity/life. An advantage of serial over initial interviews, then, is that the former can be scheduled to take place in (and between) a variety of times and places which are associated with that person's biography and everyday life. Thus, a degree of flexibility is both possible and desirable within such series depending on the project to be undertaken. To illustrate this, in the main body of Ian's (Cook 1992) blindness research, where the main issue he wanted to discuss was that of each person's degree of independent travel, his interview series began with each participant being asked to talk about both his¹ general biography and how his history of blindness fitted within it. In the next meeting each was asked to describe the extent of his day-to-day travels with reference to a map of the city in which he was living. Following this, each was asked to keep an 'activity diary' for a one week period which outlined where, when, why, how and with whom he travelled. Finally, Ian accompanied each of them on a familiar journey asking general questions and at various points stopping to ask where they were and how he knew. In his fruit research, though, where the concern was to study the relationships between the everyday lives of farm workers, farm management and their suppliers and markets, a different and more complex series of diaries was seen as appropriate. This involved: a) asking six workers, their supervisor, and manager to keep 'activity diaries' like the one above for two weeks; b) asking these workers also to keep 'money diaries' recording their incomings and outgoings (both in cash and in kind) over the same period; c) asking the farm's supervisor to keep a 'labour allocation diary' in which he was to record who was at work, on what days, and between what times (records which were already being kept for payroll purposes) as well as what he had told each person to do each day, where they had done it, and why he had given them those jobs at those times; d) asking the manager to keep a 'communication diary' in which he was to record his day-to-day contacts with suppliers, shippers, and European and North American markets; and e) asking one

¹. At this stage, all three participants were male.

British supermarket buyer to keep a ‘fruit diary’ during the six months that Ian was working on this fruit farm, in which the buyer kept a record of any decisions he had to make concerning the sourcing and marketing of the fruit being grown there. As mentioned, these diaries were then used as the basis for daily tape-recorded ‘interviews’ about the matters noted in them.

In the latter case, a ‘complete set’ of diaries detailing the everyday connections between these different people’s lives could not be constructed because of difficulties in conducting research between groups of people who wanted/needed to keep their activities somewhat hidden from others, who were somewhat suspicious of Ian’s motives, who were (or claimed to be) too busy to fill out the forms and/or to find time to discuss them with him every day and, in one case, because the supermarket buyer mentioned was sacked two weeks into the project in a huge executive clearout. The main points here, though, are that: first, depending on the circumstances and the aims of your research project, a whole variety of diaries can be used to structure, at least partially, an interview series; second, getting a ‘complete set’ in a more complex series can often be a problem not only for the kinds of reasons mentioned above but also because, for instance, you and/or your interviewee(s) may be unable to attend part of a series due to illness or problems of transportation, or may find that it is difficult to coordinate daily interviews of unpredictable durations with more than a handful of participants at a time (Ian, for instance, found that the eight interviews he had planned per day on the fruit farm were extremely difficult to manage); and, third, because keeping and discussing such diaries, maps, and so on, is usually intended to provoke the telling of stories more than to provide ‘hard data’. Thus, as with participant observation, such messiness and unpredictability is perhaps to be expected and, indeed, may provide many interesting insights into the issue(s) being studied.

e) Constructing Information.

In all kind of interviews, if you are more interested in the ways in which your research participants have *told* their stories and *described* their feelings than in the nuts and bolts of the who, where, what and how (many), it is vitally important that a tape recorder is used to pick these up. The reasons for this are numerous: first, constantly scribbling down phrases and other notes can be very distracting both for the interviewer and interviewee and may disrupt what could otherwise proceed as a fairly normal conversation; second, the researcher's memory, even straight after such a conversation, is unlikely to be good enough to remember the intricacies of not only what was said but how it was said, and comparing notes taken afterwards with actual transcripts often reveal important differences between what the

researcher remembers being said and what was actually said (particularly if you are not interviewing someone in your and/or their first language); and, third, many researchers find it mentally exhausting to listen very closely to everything that their/our interviewees say, so - particularly when faced with the common occurrence of (parts of) interviews being at the same time important *and* boring - it can often be a relief to know that if your attention wavers you can still listen to the tapes at a later date.

Given that the recording of conversations can end up being so important and that transcribing can take so long, it is vital to make sure that the quality of the sound is as high as possible. Much of this can be helped through having good recording equipment if at all possible (with stereo recording capabilities, recording level controls, etc.. being particularly useful) and using good quality audio cassettes.¹ However, even with less sophisticated equipment, it is important to maximise the sound quality from a range of possible interview situations. In his experience of interviewing, Ian has come away with difficult to transcribe recordings as a result of background noise resulting from a rumbling air-conditioning unit in a dorm room, the engine and radio noise in a moving car, the noise from a rainstorm and an overhead fan in a kitchen, and the wind noise which was inevitable when interviewing people in draughty farm buildings.

To give one example, on his first visit to the farm on which he eventually did his participant observation work in Jamaica, Ian recorded the guided tour he was given by its manager. The resultant recording was full of both important information and background noise which made its transcription very difficult in places:²

{In the packing house} You know, with [another farm] and myself shipping some, sometimes we're sending fifteen tons. {pause} You know.. ..Yeah.. ..three and a half thousand boxes a day. And that fills, pretty much fills the cargo allocations on the Jumbo. Really? Yeah. It's a lot of {indecipherable - loudening tractor noises}. OK. {tractor noise} So what's the relationship

¹. In terms of machinery, something like the £200 Sony TCD3 is an excellent combination of quality recording and compact size while dictaphones and recorders with built in microphones costing around £40 are normally fine for one-on-one interviews in quiet rooms but do sacrifice some sound quality and flexibility. And, in terms of tapes, something like a TDK AD90 cassette tape costing about £1 from discount stores is ideal.

². In the following excerpt, the farmer's words are *like this*, Ian's are like this and those of a woman they met on the way are **like this**. Square brackets indicate where names have been changed to preserve the anonymity, and curly brackets contain comments typed while transcribing to indicate why there were gaps in the transcript. To explain the uses of two full stops at the ends and beginnings of some words, in the second line - *You know.. ..Yeah.. ..three and a half* - Ian agreed "Yeah" while the farmer was still talking, but in the seventh line - *I used to work there before.. Yeah. ..and, we sell to* - the farmer's sentence tailed off, Ian said "Yeah", and the farmer then continued.

between [the other farm] and [yours] now? *Well, basically, just because I used to work there before.. Yeah. ..and, we sell to the same markets, you know {we climb into his six-wheeled buggy and he starts it up - very loud} that's how I {indecipherable buggy noise} ..when I talked to them, there was a lot of {sounds like 'vibrations'} between.. Yeah, yeah. Um.. ..Yeah, yeah. It's not, actually, you're sort of straight competitors. No no, not at all. {long loud buggy ride where only words such as "I", "yeah", "um" and "and" can be heard, if anything - 1 minute 50 seconds. Stops and talks in patois with a woman} ..better, one a you go go tell, uh, se, wan boy dem.. {Someone} gan already sir. Who you sen message wid? **Wid me daata.** Oh. {pause and wind noise - in the fields now.} To get the hermaphrodite, we have to plant a, what we do, we plant three trees.. Right. ..and, after about eight, nine, weeks when they're about three foot tall.. ..Yeah.. ..You see like at the end of those rows - they start flowering. And by the shape flav-, flower, that's how we determine the sex. OK. Are they grown {Walkie-talkie fuzz interrupts}, are they grown from seed..? {indecipherable but short walkie-talkie message received} ..or are they grown from seedlings? *From seeds.* From seeds, OK.*

Of course, the ideal acoustic environment to record a conversation may not be the same as the ideal social environment to encourage one, so it is more a matter of experimenting with the placement of the microphone, the setting of recording levels, and making a decision whether the interview should be continued another time and/or whether you should settle on making extensive notes afterwards on top of what may be left of the interview, if possible, and transcribing as much as can be heard at a later date.

All of the above, of course, depends on the tape recorder actually working and being switched on, so to help ensure this we suggest that as each meeting begins you should always check that the microphone is fully plugged in to the correct hole (we have both suffered from this one), that the 'record' button is on while the pause button is off, that the tape has been placed inside the recorder the right way round, and that the spools are turning. Moreover, as the meeting progresses, it is advisable to check whether your recorder is recording every now and again (subtly, if possible) and, afterwards, to check the tape(s) to see if this worked. Here, if you discover that your recording has not been successful, you will be able to write much more detailed notes of what you can remember being said. This may sound like rather patronising advice, but all researchers who have used tape recorders have their own embarrassing stories about 'lost discussions' when they somehow managed to bungle this simple operation. And, to make two final points on constructing ethnographic information through interviewing, it is important to realise that the full transcription of a one hour interview by someone with

reasonable typing skills will take between eight and ten hours, and that it is also vitally important to supplement these transcripts by keeping a research diary in which to describe anything of even the mildest relevance to your research which will not have been recorded on tape (please refer to the section on participant observation for elaboration on this point) .

(iv) FOCUS GROUPS.

Earlier, we argued that ethnographers should be reluctant to consider the participants in their work as pure and isolated sources of data. Rather, it is important to understand how people work out their thoughts and feelings about certain matters on the basis of interactions with others whom they/we learn from, react to, misunderstand and resist (cf. Schragger 1983). Focus groups are hence a key means through which researchers can study these kinds of processes by setting up a situation in which groups of people meet to discuss their experiences and thoughts about specific topics. So, a group is not just a way of collecting multiple individual statements, but is a means to set up a negotiation of meanings through intra- and inter-personal debates.

Focus group work has two genealogies in contemporary social science. The first is from psychotherapy where group meetings are used as part of the therapy. Here, in a series of meetings, group members explore their own personalities and identities, providing support for each other and the sharing of experiences helps them to come to terms with themselves. This is similar to the philosophy underlying such well known programmes as Alcoholics Anonymous. The second route from which such work has risen to prominence is market research. Here, groups of paid consumers are assembled for a one-off session to discuss their reactions to new products or advertising materials and these reactions are then used to inform and to develop larger scale surveys (Axelrod 1979; Stewart & Shamdasani 1990; Templeton 1987). We argue that there is great potential for group work in ethnographic research, however, precisely because it illustrates the intersubjective dynamics of thought, speech, and understanding. And, within geography this work has received enormous stimulus from the work of Jackie Burgess (1992b; with Harrison & Maiteny 1991; with Limb & Harrison 1988a, 1988b).

a) Setting up.

Before embarking on this kind of project, it is important to ask what kind of, and how many, groups you might need to set up. So, first of all, will your project

demand repeat sessions to enable rapport to develop between members and/or to cover multiple topics? If so, this will cut the number of groups that can be coped with. And, second, will already-existing groups be useful? Most authors recommend avoiding this since, on the one hand, with members already knowing each other there may be personal dynamics at work that the researcher will not be aware of which can have a significant bearing on what is said and who says it (Krueger 1988). On the other hand, though, finding an existing group does ease recruitment and can have advantages, particularly when groups are used to study not just the topics discussed but also the way that the group interacts (Stewart & Shamdasani 1990). In Mike's research with local history groups, he decided to tap into an already existing network in Bristol. Initially, he attempted to recruit in such a way as to mix up the members from different groups in order to avoid the above problems. Sitting in on meetings with a couple of existing groups, though, soon showed him that each involved fluid discussions of very local people and events. Yet, when these existing groups were disturbed, by new members or inquiring researchers for instance, their normally fluid banter (with its constant interjections, reminders of shared memories, and so on) broke down and was replaced by longer stories which were listened to politely. Therefore, he concluded that mixing these groups up would not only drastically alter the type of history spoken about, but also *how* it was spoken about.

Since any successful group will rely on interchange, banter and communication, it is vital that it promotes a relatively free exchange of ideas. However, there are a number of general problems inherent in group processes that may make this difficult. Most handbooks advocate a relatively homogeneous social group (Bellenger *et al* 1979; Greenbaum 1988). Mike encountered problems with mixed gender groups: while the men consistently spoke to a 'public' audience consisting of the group at large and the researcher in particular, the women often broke into one-on-one chats which were lost to the group as a whole, and were virtually impossible to record. At the same time, though, mixed groups can offer the chance to show up such gender relations, as they might not arise in single gender groups. Equally, age, ethnic and other differences within a group can affect the openness of discussion. So, we would suggest that some heterogeneity is both inevitable and can add to the group process so long as members share relevant common experiences (Krueger 1988). Although theoretical sampling and time pressures may encourage the researcher to get together members from disparate or antagonistic backgrounds in a single group, many researchers have found that this hampers their inquiries (e.g. Swenson *et al.* 1992). Instead, we suggest that it may

be better to convene a number of groups, each consisting of people with similar backgrounds, even if this may end up being more time-consuming.

The actual mechanics of approaching possible participants are much the same as suggested previously for interviews. And, as with other methods, it is necessary to bear in mind the practical effects of power relations in recruiting participants for group work. Busy professionals, for instance, will often not have the time to do repeat meetings and, even with one-off meetings, arranging a time which is convenient for all can be difficult (there can also be similar problems when convening other groups such as single mothers). Relatively disempowered groups, or those who have a practical interest in the success of the project, can often be easier to recruit for single or multiple sessions. It is also possible to use groups as an intervention in social processes, by bringing together people who experience similar situations but have not had a chance to share them. This can lead to rewarding sessions for participants who gain new ideas and perspectives, such as when Swenson *et al* (1992) brought together 'community leaders' from rural Georgia who then discovered a wide range of previously unsuspected common interests that they were able to follow up afterwards. Therefore, groups can be used both where normal social processes are based on small groups as well as where they are noticeably absent. But, in both cases the researcher is constructing a discussion situation and must be aware that s/he is never simply a 'fly on the wall'.

When making decisions about how many groups to convene and whether to hold one-off meetings or a series many of the same arguments apply as in interviews. In addition, it is useful to note that groups usually become more open as time wears on both in a single meeting and over a series of meetings. Here, then, it is important to consider how long it will take to cover the range of issues in which you are interested, and whether this will require single or multiple sessions. Either way, the suggested duration of a meeting varies, but Bellenger *et al* (1979) recommend one and a half to two hours as reasonable. We would instead suggest that two hours is probably going to be very tiring for all concerned, unless it is perhaps broken up with some other activity like, for instance, a tea break or watching a TV programme to which you wish to get a response (e.g. Burgess *et al*. 1991). On the other hand, though, an hour is a little short unless you can guarantee a prompt start. Therefore, an hour and a half is probably about right. Always try to allow some slack time at the end, though, in case the discussion runs on. Normally a group is slow starting but then gets under way, and sometimes just won't stop. Mike had meetings which, although scheduled for an hour, varied from forty-five minutes (a bad day) to one and a half hours and, even after this, the issue had to be deferred to another time.

Next, you should consider how the size of each group might affect the range and depth of discussion. Most commentators suggest a group size of ten to twelve is large and that one of six to eight is small but lively. On the one hand, a large group may intimidate some people and restrict those who do speak in terms of how much time they have to say what they wish. On the other hand, a small group can mean a reduction in the number of experiences that can be drawn upon and may need continual support and encouragement to keep it going. In practice, Mike preferred smaller groups but also found that group size was difficult to control. With one oral history group, a normal attendance of twelve people rose to nineteen because word got out to people who attended only occasionally that something 'different' was happening. However, because the group became so large it fragmented into mini-groups, and he felt that the only practical option was to tolerate this and to work with one subgroup as best as possible. With a different group, inclement weather and car breakdowns reduced the numbers from seven to four and here the group found it difficult to build up any momentum in their discussions. Whatever size of group you decide on, we suggest over-recruitment by about 20%, which will allow a margin for non-attendance. With repeat groups, providing a contract in which participants promise to attend and the researcher reciprocates by promising to provide summaries of meetings, researchers can be more confident about subsequent attendance (see Burgess *et al.* 1988a for a discussion of such contracts).

Another factor which can help to ensure attendance is offering money to participants, but, although market research companies can afford this, funds from university, department and research council sources are rarely available. Paying informants raises a welter of further issues regarding the relative power of group members *vis-à-vis* the researcher, as well as who will accept money and the level of remuneration necessary and what impact this will have on the issues discussed. But this issue doesn't arise if you haven't even got the money to pay participants their travel expenses, in which case it is essential to find a venue that is cheap and accessible for them while, at the same time, being quiet enough to hold a meeting which can be clearly recorded. In some areas, it may be necessary to give lifts to participants who are infirm or who are afraid of being assaulted or mugged (here, it may be possible to use a university or student union minibus). This was a regular problem for Mike in his groupwork, where community workers had to provide lifts for frightened members and all meetings had to finish before dark. The ideal arrangement here would have been to hold daytime meetings on a bus route in a low crime area, but beggars can rarely be choosers. However, in most places there are dozens of village halls, libraries, and community centres which let out rooms,

and prices, at least in Bristol, vary from £3 to £8 per hour, while some moderators even arrange meetings in the home of a group member. Together with the composition of the group, what is vital is that the setting helps members to feel able to talk about the matter in hand. Some people like to work in a setting that is homely - or actually a home - to put people at ease or to allow domestic topics to be broached. Others suggest that this relaxed set-up increases the pressure for consensus while still others suggest that people feel better able to speak their minds in a more formal environment because they can feel confident in their anonymity (Krueger 1988; Templeton 1987). Finally, it is a good idea to arrange the facilities to provide cups of tea, coffee and/or other refreshments for group members. Again, tailor these to what the group might expect, and remember that everyone will be thirsty after an hour or so of talking.

b) Group dynamics.

Once set up, focus groups rely on the frank and fluent discussion of the topics in question and it is the job of one person, known as a moderator (who may or may not be the researcher), to facilitate a free exchange of ideas while keeping the group somehow 'on track'. One way of describing the ways in which ideas are put forward, shared, argued, and/or supported is in terms of the degree of liveliness or 'energy level' of such meetings, something which it is difficult to appreciate until you have gone through a meeting or two. Moderating will be a stressful yet exhilarating experience. Take the following excerpt from one of Mike's groups:¹

Jim: But who are we saving for? Tourists?

May: Well who yes.

Jim: Children? The future..

Ruth: Well I'd like to think it was for our children yeah {grandchildren}.

Jim: Puts a different aspect on it.

This may look ordinary and uninteresting, yet what is important is that it was a group member (Jim) who asked the question - who are we saving [the past] for? - and thereby directed the conversation. In this kind of situation, comments made are usually reacted to and, as the discussion develops, group members often find that their memories are jolted or that their nerves become somehow touched by what the others have said. They may then butt in with support for a statement, an addition to it, or to disagree. So, as the group gets livelier, every statement that

¹. In this excerpt, speakers are denoted before the colon and curly brackets indicate words which could be heard during transcription but which could not be easily attributed to an individual. This whole interchange lasted twenty seconds.

members make gets added to or interrupted for some reason or another and this is what we mean by a 'high energy' meeting. As David Morgan (1988) has outlined, in an ideal case:

"The group begins with relative uncertainty about the extent to which participants share a common set of perceptions on the discussion topic. As more members of the group present their experiences and perspectives on the topic, they typically find some common means for representing areas in which they both agree and disagree: they may ultimately come to some further realisations about the sources for their various levels of agreement and disagreement" (27-28).

Therefore, one of the things a group session should most definitely *not* involve is the moderator going round the entire group asking each person the same question. Rather, s/he should try to get participants to react to what others are saying. Running a focus group, then, is usually a bit like riding on a roller coaster and hoping that, amid all the twists and turns and ups and downs, it doesn't go completely off the rails. Or, as Jane Templeton has described it, they "are always something of a horse race, in the sense that you don't know what information you'll get out of them until you've gotten it" (1987:11).

Given these sorts of dynamics, the researcher and/or moderator must be sensitive to the power relations which limit the scope of discussions and should encourage as many viewpoints as possible. A sensible start is to say at the beginning of the meeting that differing viewpoints are welcome, and that you are not looking for a final consensus. However, this is not always enough and other dynamics can still limit the discussion, such as what is termed *groupthink* where the group, or particularly a leading figure in the group, sets out an opinion early on which can become a mini-orthodoxy. Here other group members may be unwilling to disagree, and may essentially opt for the quiet life rather than face a possibly hostile reception to their views. Alternatively, individual members often rehearse stock arguments, particularly as a form of defence in disparate or hostile groups, rather than articulating and discussing how they have been generated. Thirdly, certain members may try to dominate, particularly if they feel they are experts on the topic and they can rapidly become the fount of wisdom and no one else will risk appearing foolish by arguing with them. Finally, there can be friendship pairs who interact with in-jokes rather than engaging with the rest of the group. Most people suggest that this is most easily eliminated by screening potential participants in advance (Burgess *et al.* 1988a; Krueger 1988) and then by separating friends, but this not possible with already-existing groups.

Faced with these kinds of difficulties, the moderator can employ a number of tactics to try to aid the discussion. In the case of groupthink, s/he has the nerve-racking task of trying to force this apart, and will be concerned that an intervention in support of a different viewpoint might result in losing the cooperation of the entire group. In Mike's experience, one way around this has been to rephrase points using the third person ("some people would say that.." or "didn't [so-and-so] say something different.." etc.) to broach issues and to hope that they will lead to renewed debate. A little pre-planning can also help. For instance, getting all the participants to write down what they think about certain matters before the discussion starts would encourage them to put together their ideas so that these may be more easily articulated later. The moderator would not then ask each person to read their answers out - this may cause humiliation or embarrassment and could lose the support of participants - but just thinking through and writing down their thoughts may help to give confidence to members who appear reluctant to speak. And, at the very least, they could be collected at the end of the meeting to get at the ideas and feelings of those who were not able to argue them out loud or to compare the initial written thoughts with what was later said (Greenbaum 1988; Templeton 1987).

Situations can also arise where one member of the group seems more reluctant to speak than are others, and the latter may need some kind of help to bring them into the discussion. The moderator can try specifically to include them in the conversation by expressing an interest in their point of view. For instance, simply repeating the question or asking for a second opinion, using lines such as "OK. [Turn to quieter member] Is that how you see it too?", can be effective. In contrast, when faced with a member who seems determined to dominate the conversation, it should be possible to get the support of the rest of the group who would probably like to get a word in edgeways. Another tactic that can be used if the topic is very ordinary or very familiar is that of asking people to think about it in metaphoric terms that may help to de-familiarise it and to force questioning beyond routine terms (Branthwaite & Lunn 1985; Stewart & Shamdasani 1990). This may also help to get round awkwardness with some topics, or to bring abstract issues into a discussible format. We would, however, emphasise that none of these tactics is guaranteed to work. Occasionally they may backfire, both in terms of breaking up group discussion or, very occasionally, disrupting an entire meeting. Every moderator has had this happen at some stage but, in general, moderating gets easier with practice.

Finally, Mike has found it better to have two people moderating, particularly with larger groups (cf. Burgess *et al.* 1988a; Krueger 1988). One moderator acts to

keep the conversations moving while the other can try to pick out themes and keep the overall process on target. It may be possible to co-opt a community worker or another figure in the community into this sort of moderating role. Existing groups will normally have someone effectively running the group anyway. Although standard texts would warn of 'bias' entering here (e.g. Krueger 1988), if you are studying group processes then the 'biases' of and towards a fellow moderator can be very revealing. However, if dual moderation is not possible, solo moderation is eminently within the grasp of most students, particularly when working with smaller groups.

c) Moderator roles.

Given the inevitability of these group dynamics, it is important to regard meetings as interpersonal events where each member, including the moderator and/or researcher, constantly manages her/his self-presentation to the others. As with the other methods we have discussed, this colours what will and will not be said. As with interviewing, the first rule of moderating is to think through the meeting beforehand. Working from a list of topics is very helpful, whether it be a general one of issues to be covered in any order or a specific sequence or 'route' to steer the group through (Krueger 1988; Templeton 1987). We would also suggest that this list should be virtually memorised to enable the moderator to recall what has and has not been covered in order to guide the discussion. A major consideration is also how prescriptive and detailed this topic guide should be. Most guides consist of six to twelve themes perhaps with some subpoints, and you may need all of them discussed or you may want to follow those in which the group is most interested on the day.

As with interviews there is scope to move between directive and non-directive approaches. Therefore, the moderator(s) should decide how powerful a role they want/need to play, notably, in terms of deciding whether they want to hear the world views of participants rather than to simply find the reactions to the views of the researcher, and we would suggest that most ethnographies should incline to the less directive approaches (Stewart & Shamdasani 1990). To achieve this the moderator can play the curious but uninformed role on an issue to elicit information from members. Mike has found this to be particularly effective in getting round what members take for granted, since by being willing to appear ignorant and to get things wrong more detailed explications can be prompted. The standard technique is to open with a 'grand tour' question such as "how would you describe this area to a newcomer?", and then move the question to another such as "how do you feel about that?" (but not the direct "do you agree?"). As the

description gets under way, s/he can cut in with "I'm not sure I quite understood..?" so as to prompt a more detailed account. This kind of approach also positions the group members as experts, something which can increase their confidence in speaking. Yet, too much 'ignorance' on the part of the moderator can lead to members patronising or rejecting interventions. Without being insincere, it is helpful if this ignorance is a role and you do know (or wish to know) something - it is much easier to run a meeting on a topic that you have both a real interest in and questions about. Groups can quickly catch on to a lack of interest or insincerity.

Perhaps a robust discussion will need a more formal role for the moderator, who will be able to lay down the law. This may vary with the size of the group and whether it is a repeated meeting or a one-off. This role-playing not only aids the running of a meeting but also helps the moderator to distance any conflict - and to avoid taking set-backs as personal. Less authoritative roles can lead to problems in running a meeting. Mike had some of his interventions brushed aside as elderly group members invoked the authority of experience over the 'whipper-snapper' in their midst, making asides like:

Ray: As regards leisure nowadays, something I can remember as a boy and I expect all the *adults* can as well, was that they used to serve...

Again this is not to say that these low-key moderating roles are somehow 'fly on the wall' because, by smiling and nodding, the moderator can still have a vast impact on what is said (Stewart & Shamdasani {1990} list several ways moderators can inadvertently alter discussion). By acting as a benevolently neutral arbiter of disputes, s/he can also come to be thought of as someone who is 'supposed to know', in the Freudian term (Lacan 1977), and who, returning to group therapy, can bring in issues of transference where participants can assume a variety of motives to the moderator and thus play out some of the unconscious parts of their personalities in the group (see for instance Hunt 1989). For example, if a group member treats the moderator as an 'airy fairy' academic, this can begin to suggest something about how they conceive of society, such as around poles of 'thinking' and 'doing'. Alternatively, in work such as Mike's involving self-appointed guardians of local history, the relatively silent academic researcher can be used as a welcome witness who validates the accounts being given (Myerhoff 1982). However, adopting a low-key role can also reduce moderator effectiveness in the task of encouraging the group to develop interesting points. When such points are brought up, it is vital for the moderator(s) to ask for amplification as this can produce both material for the researcher and other members of the group to work on. S/he/they must also lead in topics and get them discussed. One way of

steering conversations, then, is to pick up on issues that occurred earlier but weren't fully discussed, an approach which also provides the opportunity to bring in a quieter group member.

Another problem with groupwork is that it can be terribly tempting to get wrapped up in the discussion and to take part as a participant, but this can lead to a loss of respect, and thus of the ability to guide the discussion. If the moderator intervenes as a participant then group members may develop very clear ideas about what is wanted from them or may react and put the moderator down. Jane Templeton (1987) has advised that the moderator should establish her/himself as an expert on procedure, but leave the group to be the experts on the topic. This also avoids the moment when the moderator is told that s/he is wrong about an issue by a participant with first hand experience. In this kind of situation it does not matter who is right, but the moderator *must* avoid making the group member look stupid since this would immediately prejudice all of the trust built up in the group. Moderators have to probe and to challenge, but must also be very careful not to accuse, aiming to promote critical self-reflection rather than immediate defensive assertions. However, to avoid the hierarchical structure that this role can impose (Oakley 1981), the moderator can draw on experiences or information and then add them to the discussion not to dictate a 'correct' view but to enable others to talk about them.

At the other extreme, it is also possible to 'tune out' and become mesmerised by the discussion while following an interesting debate or anecdote, thus forgetting the purpose of the group. At this point, the moderator should try to stay alert to who is speaking and try to include people who appear to be being silenced. In a sentence, moderating is a balancing act of detachment and interest - the interest to follow up and probe and the detachment to keep the overall picture in mind. The whole operation should be kept on a friendly basis in order to foster the trust that may enable reticent members to speak openly. Unfortunately the correct balance often only becomes apparent with hindsight. Few groups work perfectly, and there are nearly always people who don't say what they could and others who dominate. There are also always moments when the moderator overplays her/his hand and everything goes dead. In practice, out of a series of four meetings, Mike generally reckoned that at least one would be a disappointment.

These problems occur and have to be coped with. They are not death knells to the project. Reading some accounts, you would think that there were never any personality clashes or problems in getting issues discussed. We suggest that some thought is given to coping with the ups and downs - the moments when the group appears to have telepathically understood what the research is about, or where they

reveal whole new dimensions to it, and moments where the group's purpose appears to be a mystery to them. It is worth thinking through some lead-in questions, perhaps a few illustrations if necessary, to get the concept across. Think how topics might be taken, and prepare to work on that and on how you can introduce topics so that they make sense with the flow of the meeting. With repeated meetings, it may be worthwhile giving out summaries of the last meeting as a matter of interest to members, to establish commitment and to help lay the ground for discussion. Mike gave all members brief discussion outlines so that everyone knew what they were getting into and had some idea where things were (meant to be) going. This meant that some members would actually pause in the middle of a statement and ask if they were getting off the point, or would deliberately change tack to link up with the next topic. At other times, they had entirely different ideas as to how the meeting should go. But we should say that even the worst meetings can provide remarkable conversations about many topics, even ones that might appear 'private' or off-limits. As the group gets going people are quite ready to talk about the most surprising topics, and the whole thing can be tremendous fun to do.

d) Constructing information.

No one comes away with a definitive version of what happened in a focus group meeting, neither the members nor the moderator. It is too easy just to notice what assertive members were saying, or to remember a moment when it all came together or when there was a bruising conflict (Bers 1989). Mike found it useful (and fair) with repeated groups to hear how the group felt about his version of events in the summaries that he gave them. But a taped record of the meeting was certainly essential, and this required a 360 degree microphone to pick up what all the members said. Although some recorders are supplied with two 180 degree microphones that will give stereo coverage, we have yet to see a satisfactory 360 degree microphone built in to a recorder. There are, however, a range of plug-in microphones available on the market of which Tandy's 'Pressure Zone Modulating Microphones' are good examples. The two most useful of these are one which is about the size of an over-thick credit card and will cover about a six feet radius and is suitable for small groups (cost around £12), and a larger one attached to a metal square the size of a small plate which has a range of about twelve feet (cost around £30). However, even with all the right equipment, accidents can happen and, as we mentioned in the subsection on interviewing, recordings can and do go wrong.

Having said that notes are insufficient on their own, we do not want to imply that these are 'subjective' as compared to the 'objectivity' of a verbatim transcript.

The dry and desiccated nature of transcripts - through which the 'life' in a complex, overlapping and often emotional meeting is flattened out into something resembling the script for a play - is also a 'distortion' (Krueger 1988; Portelli 1981). Therefore, we would stress the importance of making notes as well as tape-recording your groups in order to be able to compare your immediate impressions of and feelings about a meeting with the subsequent transcript. These notes can also be used to record incidents which occurred off the record - including body language, the arrangement of people in the room, comments made before and/or after the taping, and so on. Here, of course, having two people recording all that happens certainly helps. Mike found that group members would often come up to him after a session to suggest what they would have liked to have said. Some confided that they knew he wasn't interested in what one member had said and, therefore, told him what they thought he really wanted to hear about. Finally, it is worth adding that while such group 'interviews' may appear to increase the number of people who can be spoken with in a given time, it must also be borne in mind that a) they take longer to set up than single interviews, b) they cost more in terms of facilities, and c) transcribing them takes about two to three times as long as a single interview (up to sixteen hours per group for an amateur typist).

(v) FILMIC APPROACHES.

Traditionally, visual methods have had only a very small impact on ethnographic research, a situation that has led to the characterisation of most anthropology as consisting of the visually illiterate seeking to study the verbally illiterate (Worth 1981). In contrast, we would seek to encourage researchers to look at the possibilities that such methods can open up. Most researchers confine themselves to the odd illustrative photo which is often assumed to be a factual record of the field (Ball & Smith 1992). Yet photographic theory has suggested that this is not so, and that photographs are not transparent media for recording or presenting facts (e.g. Burgin 1987; Taylor 1994). Referring back to our section on subjectivity, we argued that people invest meanings and significance in the material objects around them. Photographs are taken purposively and displayed in contexts that can drastically alter their meanings. This is true of both art and news pictures, the subject's photos and the researcher's. And, while in this section we concentrate on those photographs taken *by* the subjects of research, the same care should be taken over the ethnographers' photographs *of* subjects (see, for instance, the section on field notes in the participant observation subsection). Here we will firstly outline some of the ways of using people's already existing photos, secondly consider the

making of photographic ethnographies, and finally tackle the making of video records.

a) Already-existing photos.

All acts of recording take place in social, economic and cultural contexts that invest the practice of photography, as well as its images, with meanings. While taking snapshots is a voluntary 'non-essential' activity with no formal training, its images appear remarkably standardised within cultures (Chalfen 1987) and, as Pierre Bourdieu has written, "there are few activities which are so stereotyped and less abandoned to the anarchy of individual intentions" (1990b:19).

A compelling explanation for this standardisation is that people take photos which they know will receive a favourable reception. Thus, it can be argued that photos provide an opening into a person's sense of what is expected and acceptable behaviour, and also into how they might wish to be remembered. Each photo is an act of (self-)presentation that involves the photographer, the photographed, and the expected audience. Both the people taking the photographs and those in them are already aware of the social contexts that will determine the meaning of their actions (Jacobs 1981; Lesy 1980). Photos and the actions of photography therefore communicate social meanings and help people to express their identities - from the camera buff, to the privileged student trying to be arty, to the tourist taking a snapshot (Bourdieu 1990b). Each year billions of photos are taken, and any one person may have taken some 3000, each of which has captured just 1/100 of a second of an event (Chalfen 1987). And, as self-selected documents, these photos begin to relate how people see themselves projected through past times and places (Becker Ohrn 1975; Holland 1991; Jacobs 1981). Photos are not to be divorced from their contexts and treated as a record, but rather should be treated as a means of revealing the processes of selection which are used in composing such a record. Richard Chalfen (1987) has argued that, rather than giving immediate access to reality, photos show how people try to invest meanings and reinterpret the world in what he has termed the 'Kodak culture' of the west. In analysing domestic snapshot collections he has noted:

"Kodak culture promotes the visual display of proper and expected behaviour, of participation in socially approved activities, according to culturally approved value schemes. People are shown in home made imagery 'doing it right', conforming to social norms, achieving status and enjoying themselves, in part as a result of a life well lived. In short people demonstrate a knowledge, capability, and competence to do things 'right'. In these ways, a sense of belonging and security is developed and maintained" (*ibid.*:139).

Photos can therefore provide more insights into the social milieu of actors than into the 'reality' they supposedly capture, and as a means for studying the culture of groups they can also provide not only a useful research avenue but may already be part of the very culture you seek to study.

In addition to this, Michael Lesy (1980) has pointed out how photos can be used as a base for, and in combination with, interviews by talking around existing collections of photos to help develop a sense of a person's or group's life history or as part of finding out more about their world (Becker Ohrn 1975; Collier & Collier 1986; Holland 1991; McCracken 1988a). Enabling the construction of histories in this way allows the researcher to be more sensitive to a person's and/or group's passage through time and space since, not only do photo albums form a useful record in themselves (because of their selectivity), but they may also be a good way to prompt people to think back about their past(s) and to relate stories not directly represented within the photos (Becker Ohrn 1975; Lesy 1980; Reme 1993). This may then allow the researcher some vicarious access to the multiple times and places of the subject's life course, especially when used with interviews or groupwork (McCracken 1988a&b).

b) Autophotography.

Another approach to using photography might be termed 'autophotography'. The idea here is that, by encouraging participants to take pictures of parts of their environment or activities, it is possible to learn more about how they understand and interpret their world and themselves within it (Kenney 1993; Ziller 1990). Thus Robert Ziller and Dale Smith (1977) have argued that researchers could attempt to learn about the photographer through her/his photography. Studies of this type have shown consistent patterns in the photographic records produced by different groups. For instance, disabled photographers have recorded few instances of non-disabled people making eye contact with them, and have thereby captured the avoidance strategies of those around them (Aitken & Wingate 1993; Ziller 1990). Many auto-photographic studies have focused purely on the content depicted in such photographs, however, and have come dangerously close to being 'thin descriptions' of forms rather than thicker ones concerning the meanings invested in them by the subjects (Hastrup 1992). We would argue that the social meanings of photography allow the researcher to look at far more than how often things appear. As Sol Worth (1981) has put it, film (and photography) is not so much about what is 'out there' as what is 'in here'. As a non-verbal medium, photos may serve almost as projective stimuli that reveal routinised or unconscious responses that are scarcely thought about by the participants - for instance, in the

way some images are accepted or constructed and in the tensions between how some absences are not noted or remedied in photographs but, at the same time, might still be spoken about. Robert Ziller (1990), for instance, has given an example where there appeared (to him) to be a conspicuous absence of black students in photos taken by white students around their 'mixed' college, while the latter would never *say* that there was racial 'segregation' on campus.

In terms of material requirements an auto-photographic project can be quite cheap in that most people have access to a camera, while providing film is usually about £8 per subject, including processing. One thing worth thinking about, if the researcher provides a camera, is the level of anxiety that this may cause for the photographer, particularly if it is a sophisticated model. Our advice here would be to sacrifice some artistic potential and use a one-touch or even a throwaway camera. In autophotographic work, getting pictures is more important than getting 'fine art' - which is a point worth mentioning to participants to reduce the concerns which they might have about this.

c) Film and video.

In the realm of the moving image, much of the advice about still photography also applies. With the continued technological drive towards simplicity and user control, video technology now proliferates. The camcorder is part of a global trade in funny clips and is the staple technology of programmes of 'eye-witness TV' such as the BBC's '999'. This has provided a new visual aesthetic of gritty realism, or 'cinema naïveté' as Richard Chalfen has termed it (1987:49), a putatively spontaneous capturing of the world 'as it happens'. Like cameras, camcorders are symbolic goods involved in status games, being mostly used by men (or their sons), having distinct class overtones in terms of ownership, and being redolent with ideologies about technology. The researcher hence has to be aware that this is in no respect a neutral medium. But this is not necessarily a negative feature, as we have endeavoured to show with other methods. Using video technology, Mike has attempted to latch onto the momentum of the 'video diaries' series by the BBC's Community Programmes division, and to use an auto-photographic approach. Thus, his research on the serious business of representations of the past has deliberately called upon a form of video that is aimed at fun and a staged spontaneity. This form does not follow a cinematic grammar as the repeated exhortation of instructional magazines over the last thirty years would tend to suggest, with their imploring people to adopt a more cinematic approach (Chalfen 1987). Instead, this suggests how most home movies are dominated by a discourse of fun, special events and staged spontaneity, and it is these sorts of criteria which

will usually frame the movies that are made. Moreover, it is just these sorts of frames of expectation that may be helpful in an ethnography by revealing the practices constituting the bounds of the normal, the spontaneous and the exceptional. Camera work can therefore be used to prompt transgressions or 'deliberate barbarisms' against expected behaviour. Participants may goof around for the benefit of the camera, and this may serve to highlight where and how normal routines are experienced (Bourdieu 1990b). But the costs of this sort of enterprise need some attention: with a camcorder costing around £600, and tapes around £8 each, you may break your research budget simply on hardware. Having said that, it may be possible to work with groups who already have access to video equipment, in which case your costs may be reduced to those of supplying the group with tapes.

If you intend to do something more like action research then film (or photography) can again provide a useful tool (Aufderheider 1993; DOWMUNT 1992). For instance, work has been done with young working class kids to enable them to develop not only their own photographic representations of their world, but also the skills to critique other people's representations - and at the same time to acquire what may be a hobby or vocational skill (e.g. Dewdney & Lister 1988). This requires far more than existing 'snapshot' or home movie expertise as its intention is to alter the situation of the people involved by providing them with resources (equipment or skills) that were previously unavailable to them. As such, this approach may possibly call upon more resources (i.e. good quality photographic equipment), skilled instruction and time in which to develop a project. However, if a situation were to arise where you could become involved in such an already existing project, then this would be more feasible for most geographers. Perhaps you could try to provide the chance for a group or individuals to make a full film of aspects of their own lives - as in video diaries or films such as 'Through Navajo Eyes' (Worth & Adair 1972). In these circumstances, the researcher can seek to interpret not only what is portrayed in these films, but also what the form of portrayal might reveal about the group's world view. In 'Through Navajo Eyes', Sol Worth was struck by a sequence on smithying silver which began with a Navajo mining metal, when to his knowledge Navajos had never mined metal but had always traded for it instead. When he questioned the accuracy of this part of the film, it was explained to him that stories had to have both a beginning and an end so the film-makers reconstructed a beginning to make the film fit their rules of storytelling (Worth & Adair 1972; Worth 1981). Meanwhile, in Western cultures subtle changes in the use of film can also be found. Thus, Sol Worth has also reported how black urban youths have

filmed themselves in many activities and have been very eager to appear on film while white, male, middle class youths have preferred to remain behind the camera in control of the images eventually screened. This has led him to support theories of the way a 'gaze' forms part of a nexus of knowledge, power and pleasure (Worth 1981; cf. Pinney 1992).

Alternatively, you could try to make a film of and with the people studied. There is a considerable literature on the restrictions and possible benefits of this (e.g. Crawford & Turton 1992; Ginsberg 1991, 1993; Ruby 1991; Turner 1991). The most pressing problem for the geography student setting out to make an ethnographic film which will be both the finished product and the research material, concerns the skills and resources needed to do this. The cheapest introductory course on editing costs around £90, and you will need some practice after that. However, if you have the time and the money to learn how to make a full-blown film, then movies such as Michael Moore's 'Roger & Me' (a useful example of a filmed ethnography of a town destroyed by deindustrialisation: cf. Jones & Natter 1993), or Nick Broomfield's 'The Leader, The Driver and the Driver's Wife' (a useful illustration of the doing, as well as the subject, of an ethnographic film) would be well worth watching as both filmmakers have relied on and shown the intersubjective processes in their work rather than claiming to construct *cinema verité* or 'fly on the wall' documentaries. We would certainly draw attention to the possibilities of using films as inspiration and contextual material in many projects, as mentioned in the sections on interviewing and focus group work. Films may also serve as materials through which people understand places, informing the actions through which they reproduce and engage with places - places which, in turn, have been produced by other people reproducing and engaging with their representations of areas (cf. Burgess & Gold 1985).

We would suggest that using visual methods as part of ethnographic research creates both new avenues and new dangers around the issues of incorporation, representation and empowerment. Firstly, a multiplying field of visual techniques is being deployed by people that may well allow the researcher to make a wider impact than just on her/his own degree result. Up and down the UK, various groups have been producing videos for schools and/or to counteract the effects of global media corporations. As we write this, Mike knows of four community photography projects ongoing or exhibiting in Bristol alone. Equally, groups all around the world have been attempting to represent themselves on broadcast video. As the cost of video equipment has fallen, such counter-hegemonic organisations have grown in number, making a diversity of programmes whose contents have ranged from Australian aboriginal myths through to Inuit language soap operas

(Downmunt 1992; Ginsburg 1993; Thede & Ambrosi 1991). These groups have been trying to move from being images in the culture of hegemonic groups to producing their own images of themselves on TV. If you can get access to these sorts of groups, then you may be able to gain access to various processes whereby numerous communities are attempting to redefine themselves in the modern world.

d) Constructing information.

There are several ways in which visual information can be dealt with in an ethnographic account, and which is/are most suitable will depend on the project and the material generated. Firstly, Robert Ziller (whose work we referred to earlier) has developed an approach which involves concentrating on the *content* of the images produced, defining in general their items or styles, and then counting these in order to produce numerical summaries of what is depicted and by whom (e.g. Ball & Smith 1992; Collier & Collier 1986; Kenney 1993; Ziller 1990). We have already expressed our reservations about how this process reduces the importance of visual images as communicative events embedded in social life. A second approach which can be used comes from structural linguistics, and is part of a broad tradition that has treated visual representations as a form of language so that images can be interpreted according to the codes of representation which they appear to embody. This kind of interpretation can also be applied to ethnographic material, in terms of identifying key symbols and their relations. Here you might ask why certain groups are often displayed in certain poses or against certain backgrounds, or why advertisements consistently code some activities as masculine/feminine or high/low class (Goffman 1977; Hirsch 1981; Kenney 1993; Lutz & Collins 1993). We do not have the space for a full scale discussion of this form of analysis (see Ball & Smith 1992 for a summary), but in terms of doing ethnographies we would agree with Michael Lesy (1980) who has argued that this kind of approach can very easily allow the researcher to discuss higher orders of archetype and symbolic structure which can be abstracted from the 'rustle of daily life'.

A third method of photographic/video interpretation closest in spirit to the approach advocated in this booklet, however, is that used by Richard Chalfen (1987; cf. Musello 1980). His approach has involved the identification of patterns and regularities within and between images which can provide insights into social processes, while also being sensitive to the practice of photography and to the ways in which the taking of photographs or the making of videos are *communication events*. Following this, we suggest that the researcher could think of two axes roughly corresponding to practice and image as an heuristic guide. Along the first axis, questions of *practice* can be placed concerning:- (A) *Planning*: e.g. whether

the images were staged or candid, or staged as candid, and/or whether the people in them were aware of the photographer; (B) *Filming events*: e.g. what events were depicted and what others (often necessary for the image to have been produced) were not - such as the location from which the picture/film was taken, how it was set up, and/or how it was decided who should take it; (C) *Editing processes*: e.g. how and why certain images were selected to be shown to the researcher, and/or how are they stored, organised and/or edited by their owners anyway such as tucked away in family albums or framed on the mantelpiece; and (D) *Display*: e.g. for whom were these images usually displayed, for how long, and how often, were they intended to be viewed by the general public, the photographer's family, and/or just the photographer her/himself? Moreover, along the second axis, questions of *image* could be placed concerning:- (1) *Participants*: e.g. who the participants were, whether everyone was always included or certain people were left out on certain occasions, and/or whether you notice any differences between who was part of the group (or who was *depicted* as part of the group) in different kinds of representations; (2) *Topics*: e.g. what events and topics has the photographer tried to communicate, whether these were holiday photos, and/or if s/he was trying to show spontaneity and domestic life, or to record members of the family and/or their activities; (3) *Settings*: what settings occurred as contexts for what topics and what participants, e.g. whether mother was always placed in the kitchen or never, whether the purpose of the photo/film was to document that the participants were in a certain place at a certain time, and/or whether it was taken in a private or a public area; (4) *Form of the finished image*: e.g. was it a print to be circulated by hand and/or placed within a family album, a slide for communal display, a film to capture activity, and/or whether it was blown up or framed; and (5) *Stylistic devices*: e.g. what, if any, genre it drew on, whether it was a formal portrait, a landscape study, a snapshot, or a 'candid camera'-type shot.

All these contexts will be important in working out the meanings of particular photos/films to different people in different situations as, for instance, Catherine Lutz and Jane Collins (1993) have attempted to do regarding the multiple contexts of photographs taken and selected for reproduction - institutional, political and technical - by variously interested actors. We have not tried to construct an exhaustive list of questions, here, and nor do we advocate using these 'axes' mechanically to cross-tabulate frequencies of occurrence. Yet, they do highlight a series of related ideas which can be usefully borne in mind when examining images, whether they are already existing, 'commissioned' for an ethnography, or taken by the researcher. Mike has found that using this kind of work in conjunction with 'photo-interviews' has been valuable in interpreting how visitors use historic

sites. This in turn suggests that ties with other ethnographic methods can enable the researcher to understand both how images are used and what they can mean to the people who possess and/or produce them (Lesy 1980; Reme 1993). Moreover, if we consider what we said earlier about the struggles of various groups to represent themselves rather than to be represented by others, we suggest that researchers should also think through the power relations involved in constructing ethnographic information around, about and with visual imagery. Here the most important questions concern who controls and edits any such product, whether people(s) are just reduced to images simply for the viewing pleasure of others, who decides who these groups/people(s) are anyway, whether and how (through using photographic means or otherwise) researchers should make use of their/our power to invade people's private spaces to render them public (Berko 1992), and whether researchers are actually encouraging research participants to reveal themselves when researchers them/ourselves would not? Again, as with the other approaches outlined in this booklet, there are no straightforward answers to these questions, and all that we would advise is that you should try to address them as best you can within the political, ethical, moral and interpersonal contexts of your own project. Whatever problems you may encounter in these respects, though, we do not believe that such media can be ignored as societies around the globe are using visual images more and more to mediate their cultures.

(vi) SUMMARY.

The approaches we have outlined here are not intended to be taken as either/or options. Rather, we suggest that you should keep them all in mind throughout your project in order to respond to its contingent 'realities'. Ideally, you should use more than one method to enable you to *triangulate* the information generated, i.e. to cross-refer from one to another to see if it is consistent, and to look for new insights that one approach may bring to another (Ley 1988, 1992). We would hope that the approaches discussed will allow you to make many such productive cross-fertilisations.



5. ANALYSING FIELD MATERIALS.

"Every time I have been in the field and become truly involved I have had to struggle with an impulse to stay longer than I should have stayed. By this I mean that I felt an almost irresistible urge to gather more data rather than face the grim task of organising and reporting on the data I had. But in every case, the longer I stayed, the less time I had to write, and the poorer became my final report. Indeed most of the data gathered at the expense of the time I had allowed for writing is still languishing in my files. It's a horrid but inescapable fact that it takes *more* time to organise, write and present material well than it does to gather it. ... The sensible researcher will allow as much free time to write his [*sic*] report as he spent in the field. If he is really astute and can get away with it, he will allow more time" (Wax 1983:193-4).

In the geographic literature on qualitative methods, 'data coding' or 'transcript analysis' has tended to be conspicuous by its absence, forming only some 10% of all major accounts despite taking up at least as much time as fieldwork (Miles & Huberman 1984). This is remarkable as it leaves the impression that all the 'experiential richness' of the methods we have been outlining somehow effortlessly transfers itself into the finished report. Some (parts of) research projects may have tightly defined research encounters (e.g. a discrete series of focus group or interviews), while others may be more loosely defined as researchers follow up leads and adapt to the multiple contingencies of their/our ongoing research projects. In the latter more flexible case, researchers may end up constructing materials from a wide range of encounters, may only *later* be able to gauge their (ir)relevance and may therefore have to leave a proportion of their material untouched given the time pressures which Wax (1983) has highlighted above. Therefore, we do not propose some magic solution for analysing materials from the field but will try to point out some of the reasonings and issues that might be considered when trying to do this. We also do not pretend to provide either an exhaustive account or a mandatory process, but hope to provide enough of a framework to aid a first-time researcher without stifling her/his creativity.¹

(i) PRELIMINARY WORK.

¹. Alternative approaches can be found in Taylor & Cameron (1987) and Potter & Weatherell (1994).

Let us take a common situation where, after a period of work using a variety of methods, you find yourself with a stack of transcripts, field notes and so forth which, in the following pages, we will refer to as your *primary* materials. At this stage you will no doubt be able to remember lots of juicy morsels and not a few embarrassing slip-ups, but this of course is far from a rigorous analysis. Thus, in the early stages of your analysis you should begin by going back and rereading all of these primary materials. In doing this, you will be able to remind yourself of the contexts in which these were constructed as your research progressed and what your thoughts were on this at various times. At the same time it is highly likely that you will feel that (large) amounts of what you have compiled is somewhat irrelevant, naive and/or gauche (this is normal). Having gone through these materials once, though, you should a) have some ideas about interesting happenings at the time, and b) have refreshed your memory on your working ideas of how to interpret these events, and on how these ideas directed the collection of further materials (Bogdan & Taylor 1984). Having done this, you should then prepare for a more in-depth study of your primary materials. It is a good idea to start by getting typed copies of these materials formatted, say with a wide right margin, so that you have plenty of room to annotate them. Next, you should make sure that every page has the source of the material on it and that each speaker, event, setting, sketch, photo, etc. is labelled so that any page or excerpt can be placed in the wider body of the materials (for the same reasons, lengthy sections of text should be numbered by page and, probably, by line). If you have access to computer analysis systems, it is worth noting that some programmes provide built-in facilities for analysing different kinds of primary materials, together. The *HyperQual* programme (for Macs) has settings which allow the inclusion of materials from sketches, pictures and literature reviews alongside those from diaries and interviews. The *HyperResearch* programme (for Macs and IBMs) has similar capabilities and will include the ability to process video material. Moreover, other programmes may be more geared to specific sources or allow specific combinations (see Fielding & Lee 1991; Tesch 1990). On top of this, whatever you do, it is also advisable to make one copy of everything and put it in a safe place since this will serve as your backup should everything go horribly wrong.

After having done all of this, you should then take your main copy and begin to work on it. Most researchers recommend starting an analysis by reading these documents one line or sentence at a time, and trying to concentrate on what was going on step by step and to reconstruct the events to which each part refers (McCracken 1988b; Strauss 1987). In doing this, it may also be advisable to play

back your tapes again and to look through your research diary (which you may want to type up and include as part of, rather than supplementary to, your primary materials) in order to recapture some of the emotional flavour and the interpersonal situations that produced the material (Portelli 1981). This kind of approach may help you to avoid producing a cold, over-rationalised account which does not do justice to the intersubjective richness of the research encounters that it has drawn on (Hunt 1989). Such a close study of tapes, notes, and so forth invariably reveals a welter of things that researchers had not noticed at the time when they/we were trying to manage these encounters. The emphasis here should therefore be on thinking about what was being said and what the meaning and intent of each statement might have been, and as you go through your materials you should write these down in your wide right margin alongside the text. Anselm Strauss (1987) has termed this process 'open coding' (see Figure 3.), and has stressed that in this initial stage of analysis there is no need to look for significant themes or relations because this may lead to a prejudgement of events later on in the materials (but, in contrast, see Ingersoll & Ingersoll 1987). In practice, though, because the ways in which you will have constructed your materials, particularly in that they will have been inspired at various stages by specific ideas about what was important at the time, no researcher can confront such a text quite so 'innocently'.

Additionally, either as you go along or going back over the materials after having completed an entire 'cycle', you should then 'code' your annotations. Very simply, here similar events or themes or actions or parts of events or sentiments should be given similar labels. The form that these labels can take will vary depending on your preference and/or the scale of your study. In doing this, some researchers use different coloured pens to highlight codes which refer to different phenomena, others underline, highlight or mark sections with abbreviations. Whatever is most comfortable for you, though, will probably be best. These codes may cover a single word, line, feeling or a whole chunk of text. It may also be worthwhile to develop codes that allow you to analyse or to note the context of remarks or observations - whether they were sarcastic, defensive or said with insistence and, in group work especially, the pattern of dialogue i.e. whether a coded theme was a feeler put out by a group member to test the water, was said by several members or repeated by one, involved someone summarising another person's ideas (Krueger 1988), was triggered by something said earlier,¹ was a response to a question or was simply unsolicited.

After initially going through your materials, it will probably be time to begin rereading them to firm up your codes. As you go through the materials again, you

¹. Stewart & Shamdasani (1990:114-140) suggest how computers can be used to analyse this.

should try to note down all of the categories you have invented on a separate piece of paper. This can have several uses, in that it will allow you to see how many categories you have, whether there are any very similar categories that might be usefully amalgamated, and what categories you have already found/invented. This last point is by no means as flippant as it might seem: we have all had times when we have failed to use an appropriate code because we had temporarily forgotten about it. Moreover, we would suggest that the researcher needs to be sensitive to how much prior categories can determine what s/he subsequently looks for and to what extent such categories may be said to be 'found' in the material. This last problem is largely the reason for suggesting an 'open coding' procedure as the first

Figure 3.

An example of open coding:

It is worth noting here how code categories which survived or were transferred into later sections had been boxed, others over-written or altered. For example. 'AGE' (lines 426-7) initially referred to the date of participants' memories and their longevity but was changed to 'ERA' to fit in with other groups and to include the periods from which memories tended to cluster. The initial note made at the bottom of the page - "regret omitted from histor" (lines 439-40) - was later coded as 'ABSENT' (lines 434-440) with the addition of a '-ve' marker to show that this absence was apparently regretted. Moreover, while many categories were emic (e.g. 'Smell' - lines 394-411), a note that the group dynamic was one of 'Shared input' (lines 418-423) was later coded as 'GRP' to indicate group dynamics.

step, so as to avoid imposing some outside set of categories, and this requires a sensitivity to what have been called 'emic' and 'etic' categories in analysis. As Michael Agar has written:

"In many recent anthropological discussions, *emic* and *etic* are used to characterise a .. distinction roughly translated as the "insider's" versus the "outsider's" point of view. The problem here is that it is difficult to imagine any ethnographic statement that is not a blend of these" (1980:191).

Roughly speaking, then, emic (or 'in viva') categories are those which have been used by the people studied to describe their own worlds. Given that ethnographers usually have a clear interest in how people interpret the world to themselves, particular care should be taken to note these kinds of categories as you pore through your materials (cf. Patton 1980). However, as we have already argued, it is virtually impossible for the researcher to banish all of her/his prior thoughts from the analysis, since her/his research will have been based around a theory-driven selection of participants, and because even noticing an 'emic' code will have required interpretation. Like Agar, then, we would certainly not argue for a clear distinction to be made between these two categories since we have ample experience of how difficult it can be to interpret an allegedly 'emic' categorisation when, for instance, we have suspected that it was being used ironically or was the result of how the respondents tried to present themselves to us. Thus, instead of adopting a strict emic/etic binary, in the following pages we will suggest an approach which involves a general drift from emic to etic coding (in which we still consider it useful to *ask* questions such as "to what extent is this a participant's world view or some composite of my representation of her/his world view?"), but which is also subject to the provisos mentioned above. Thus, the move from one to the other is not taken here as being a simple or straightforward process.

(ii) DEVELOPING CODES.

While working through your materials you are likely to see vital connections and/or glimmerings of new ideas. It can be a good idea to take another sheet of paper to note down these sorts of insights and hunches, or indeed to create what might be termed your 'must remember to check' notes. Anselm Strauss (1987) has termed these *theoretical notes*, and has suggested that they should be reviewed regularly to guide and to develop more ideas. These notes and those from the field can begin to form cumulative chains relating (to) certain ideas, and they should

help you in making sense of your materials and in developing new ideas about how your codes relate to each other. Moreover, on yet another sheet of paper it is important to try to write down in full what you mean by the codes you are generating and how you think they are working. This is useful both because it should help you to think through what went on and because it is all too easy to forget what an abbreviated note meant or why you made it in the first place. Hopefully, this 'paper trail' of working notes should help you to remember and to trace those connections that you have thought of while going through the materials.

After this has been done, and in order to gather your materials into a manageable form, these connections and relations between statements need to be sorted through. There are again several ways of doing this depending on the amount of materials, the time you have to sort through it, and your inclination. It is possible to enter the materials and codes onto a computer using qualitative data analysis packages such as *The Ethnograph* (for IBMs), *Hypersoft/Hyperqual* (for Macs) or *HyperResearch* (for both).¹ It is equally possible to do this sorting manually by cutting up chunks of notes and transcripts and placing them in piles according to your codes and/or by making an index card for each code and recording on it the location of each occurrence in the materials. What should then begin to happen is that the focus of analysis shifts from the individual statements to the ways in which they relate to each other (see Figure 4.). To make sense of how your materials fit together (and to find relevant bits), many researchers end up establishing some sort of cross-referencing system to render a complex series of notes comprehensible by connecting relevant sections with other similar cases. It is important to realise that the categories and similarities on which your system will end up being based will not have been decided in advance but will develop only *as* you make connections.

(iii) SIFTING, SORTING AND MAKING SENSE OF IT ALL.

As you sort your materials and connect your codes, it is likely that there will be some things which don't appear to fit together or which contradict each other within your codes. This is where the idea of an *iterative process* is useful. When faced with these misfits and contradictions, you should go back to see why these occurred; was a particular case due to a 'clerical error' in filing, was it a genuine difference between sources, or was it because, through your coding system, you

¹. Before deciding whether or which programme to use, it is important to read the fuller reviews of their capacities and limitations by Dey (1993), Fielding & Lee (1991), Pfaffenberger (1989), Richards & Richards (1994), and/or Tesch (1990).

had erroneously grouped together differing phenomena? Moreover, you may be prompted to clarify interpretations between two conflicting sources (something which may necessitate

Figure 4.

Relating themes using qualitative data analysis software: an example.

This section was retrieved using *The Ethnograph* software, as a search for the overlap of two categories from Mike's groupwork material. At the top of the page, the source of the material (GRP1A May) and the categories which were searched for (SC: +COAL +REP MEM) has been printed, while the text in which these overlap has been indicated down the right hand side by the symbols \$ (for COAL) and % (for REP MEM). Other categories also punctuate the text and are bracketed down its right hand side (e.g. the code SOURCE by the symbol *, or the code LOCN by the symbol @). Within this there are various sizes of coded chunks and different types of codes. For instance, COAL was a broadly *emic* category covering those parts of the transcript where group members talked about topics relating to coal mining in Bristol, while the REP MEM (standing for REPORTED MEMORY) code which it was paired with was a broadly *etic* code covering those parts where Mike realised that they were recounting events which they had not personally experienced. The brevity of the codes used here again indicates the necessity of writing down somewhere what each means in full.

further fieldwork), you may decide that this contradiction is part of normal human inconsistency, or you might think that it is a crucial part of your understanding of a particular theme. Most likely, though, you will have to go back and reclassify the other occasions on which that code was used. However, it is only through this continual going back and forth from source to categories to source and so on that your codes will begin to become more consistent as you develop them to cover all of the incidents and differences in your materials. As this process goes on, though, you should find that there are fewer and fewer misfits and contradictions in your coding system.

By this stage, you should have some ready indicators of what the important themes/events or categories in your materials might be. You can then place your primary materials to one side and begin to work on your coded sections, i.e. your *secondary materials*. Here the idea is to sort these sections into piles by topics (stacks on *Hypersoft* or code segments on *The Ethnograph*) and then to move these around as you begin to think them through. Look at the relations within and between these piles, and reread them in order to trace out more carefully the similarities and differences you identified in the previous stage of analysis. The idea is to consider how categories (i.e. 'piles') relate or overlap with each other - are they 'sub-codes' of a major idea, are they mutually exclusive with another, or do they consistently overlap in one way with another code? These patterns among the categories bring us to looking at the connections among the materials.

These 'piles' will come to be the *dimensions* of your analysis. Each one may relate to some key issue, and Anselm Strauss has suggested (1987) that researchers should explicitly tease out the different properties of each pile/category. For instance, with reference to Mike's groupwork transcripts, he began coding statements that suggested different kinds of "loss". However, this category grew to contain an enormous number of segments which he then sorted into a continuum. Through this process, the category could no longer be labelled simply "loss", but became a progression of categories - gone, disappearing, derelict, unused, and remaining. Thus, beginning with a general code identified at the primary stage of analysis, a whole axis of codes was subsequently developed.

Another way to assist in your attempt to understand the material is to keep thinking through the theoretical notes that you made as you went along concerning what appeared to be leading where and so on. You could also draw diagrams of how your categories appear to relate to each other, or you could even physically move the piles of coded chunks around. Going back to noting these categories on a separate sheet of paper, Mike has tended to sketch out possible patterns and linkages between these by placing them in little boxes on a sheet of A4, drawing

lines/arrows between them and thereby constructing a 'code map' (cf. Jones 1985, Miles & Huberman 1984, Strauss 1987: see Figure 5). Moreover, the subtleties of connection can also be drawn out by annotating these lines to indicate antagonisms, similarities or causalities. We are not suggesting a need to draw neat conceptual boxes here but are insisting upon the usefulness of such diagrams in providing a framework to hang your ideas on. Drawing and redrawing these diagrams can provide ways of rethinking what you are doing and can usefully point out glaring gaps in your ideas when a 'flow diagram' stops or when all of the interrelations point at an issue which you have not yet raised. As an example of using diagrams to visualise patterns and to look for breakdowns and relationships, some researchers have constructed matrices (e.g. Agar 1986, Chalfen 1987). For instance, we mentioned earlier how Richard Chalfen (1987) took the practices of photography and juxtaposed them with the images produced. Again, the idea here would not be actually to produce a matrix as the final output from the research, but to use it as a framework on which to hang your ideas. So, again, you could ask yourself what boxes were empty or overfull, and why?

It is not wise to become too sold on the elegance of your 'code map' at this stage. For, as you progress through sorting your secondary materials, it is highly likely that there will be more breakdowns in your schema. Suddenly, what had appeared to be a logical distinction or relation may no longer work, so you may have to break up or subdivide a category or remodel the relationship between two or more categories. As mentioned above, you are likely to develop a schema until it breaks down, at which point you will have to go back through all the cases which were bound up in it in order to develop an alternative one. Each breakdown can thus force a rethink and a backtrack in order to reorganise your thoughts. This is one reason why Michael Agar has termed such analysis as 'maddeningly recursive' (1986:29) and why others have referred to it as "a messy, ambiguous, time-consuming, [and, we must emphasise] creative, fascinating process" (Marshall & Rossman 1989:112).

Any idea that analysis is some process of mysterious cogitation should be dispelled by now: every researcher faces a task which is nearer 99% perspiration and 1% inspiration. This fact has two important implications for *doing ethnographies*; first and foremost, the successful analysis of ethnographic materials is very far from a mystical process of intuition that only the chosen few are able to perform; second, it is important, as we emphasised earlier, to allow plenty of time to interpret these materials; and, finally, the 'paper trail' of notes and revisions which is central to such analysis is evidence of its 'reliability' because the *reasons* for your interpretations are explicit, and you can show that your analysis is much

more than just 'quarrying out the good bits' or using field material to lend an eye-witness authority to your

Figure 5.

An example of a ‘code map’:

This sketch was used by Mike to keep track of the codes and subcodes referring to one group meeting about the presentation of a local area’s history by official bodies. As can be seen, this is not like a finished ‘matrix’ but is more like a ‘map’ of the interrelationships between the issues which were covered in the meeting. Broadly, these were traced from the general codes on the left where, among other things, the group referred to change occurring (‘Change’), to the presentation of history (‘History’), to how different the past was (‘Past as Diff^t’), to the industries then present (‘Industry’), to the causes of change (‘Causes’), to the relationships with different areas (‘Diff^t area’) and to the alleged role of town planning (‘Planners’). Generally the words are mnemonics for categories, those closer together are related and, as can be seen, this ‘map’ shows a dense development around some of these issues and not around others. Much of this will be fairly illegible to most readers, but it is important to state how this mapping worked as an aid to thinking, how it changed and how new relationships were drawn into it. Its messiness and sudden long connecting lines show how ideas can change, evolve and shift as new ideas come to light. Moreover, it may be worth noting that some computer packages (e.g. *Hypersoft* and *Hyperqual*) have facilities for incorporating and/or producing such ‘code maps’, albeit in a limited fashion.

account (Atkinson 1990). However, as with the ways in which we have set out the various fieldwork ‘methods’ in this booklet, it is also important to note that the ‘methods’ we have outlined are intended as a *means* rather than as an end to analysis. They are meant to *aid* the interpretation of your ethnographic materials rather than to dominate it.

(iv) RELATING ANALYSIS TO FIELD AND THEORY.

To finish this section, it is perhaps worthwhile to think through how this process of analysis may tie in with the theories and practices of fieldwork which we have discussed throughout the booklet. In particular, some care is needed over how the terms of analysis used here relate to ‘the field’. The analytical processes sketched out above follow what has been termed a ‘rules and units’ approach in which the researcher breaks up her/his field materials into conceptual units (Taylor & Cameron 1987). After this, s/he tries to find the ‘maps of meaning’ within which these units are organised and related in a ‘world view’, a ‘culture’, and/or a ‘life world’. Much care is needed in discerning how these units and rules relate to the ordering encountered in the field - as in the earlier mention of ‘etic’ or ‘emic’ categories. If the categories developed are broadly ‘emic’, then the interpretation is much more direct in terms of its relation to fieldwork experience. However, this is a profoundly tautological process, since it uses the researched’s own categories and how they see them working to define the relations of the researcher’s analytic categories and how these work. It does not require a huge leap in imagination to see cases where this can be wildly divorced from what might be termed the ‘objective relations’, or the academic account of relations, between either things or categories. To take an example of this as it has cropped up in the literature, Pierre Bourdieu, in his study of the social significance of ‘taste’ (1984), has suggested that while interviewees would assert that their tastes were unaffected by class, the practice of those tastes was highly class bound. For this reason he has gone so far as to suggest that searches for statements of motivations expressed by actors may sometimes be a blind alley, since believing the explicitly stated reasons may obscure the very constructed nature of those reasons (Bourdieu 1990b).

One way to follow up on how the rules and relations that you have constructed relate to those of the people participating in your study may be to think in terms of what ‘rules’ you are searching for, and how you theoretically see the ‘maps of meaning’ you are trying to reconstruct from the materials. A division could be made here between ‘rules’ conceived as those of a ‘cultural grammar’ and those of ‘socially sanctioned norms’. The former may be said to operate whether or not

people are aware of their functioning, while the latter suggest the possibility of deviation and amendment, although this may incur social sanctions (see Taylor & Cameron 1987). Equally, the work of Pierre Bourdieu (1984) and Anthony Giddens (1984), which we used earlier, suggests that many of these rules exist immanently in people's thoughts and actions, often in the context of the routinised practices of 'practical consciousness'. To account for these situations you may need to derive an 'etic' schema, but this can never be straightforwardly imputed to your participants. Their/our actions may respond in accordance with such rational rules, but these rules remain *(y)our* (academic) constructs and not the cause(s) of *their* (practical) actions (Bourdieu 1990a). Likewise, all these categories which we painfully construct may make it appear as if the world comes pre-packaged in neat soundbites. It doesn't. There is a danger of reifying categories until they become what the exercise is about. The fracturing of the field experience for interpretation constructs categories, it does not reveal 'truths'. A real danger here is that these constructions can become divorced from the experiences that they try to encompass, particularly with 'etic' categories or rules, whereas the strength of ethnography is in trying to grasp how people understand their own worlds. In balancing these two approaches, then, we would agree with Michael Patton who has argued that:

"It is the ongoing challenge, paradox and dilemma of qualitative analysis that we must be constantly moving back and forth between the phenomena of the program [being evaluated] and our abstractions of that program, between the descriptions of what has occurred and our analysis of those descriptions, between the complexity of reality and our simplifications of those complexities, between the circularities and interdependencies of human activity and our need for linear, ordered statements of cause-effect" (1980:325).

Thus, the caution we would urge applies to both *(y)our* own accounts of the world and those encountered in the field. We argued in the opening section that researchers have never had an omniscient view and that the temptation to set up *(y)our* analysis as such must be resisted by constantly cross-referring between *(y)our* abstractions and the contexts that gave rise to them. Equally, we would not wish to over-romanticise the accounts from the 'field' since people *(t)here* also have partial perspectives; there are no pure subjects or perfectly knowledgeable informants. Moreover, there are unlikely to be singular accounts of singular cultures but multiple competing versions, and it is by shuttling between these different versions that ethnographers can begin to perceive the way in which people produce and reproduce the world throughout their/our lives. The process of

analysis is not a matter of developing a definitive account, but of trying to find a means to understand the inter-relations of multiple versions of reality - including not least that of the academy - so that it serves to stress the interconnectivities which we outlined in the first section.



6. CONCLUDING REMARKS.

We hope that this brief essay will encourage students to do a variety of ethnographies. As a first-time ethnographer or otherwise we hope that it will give you enough of a start so that you can get an idea of the sorts of issues which you are likely to encounter. We do not expect it to have exhausted all or any of these, but do hope that it will both answer some of the questions which often concern first-timers and provide access to materials that may enable the study of further issues in more depth. As such, this booklet is very much an overview or a port of first call. We have attempted to write about how ethnographies can actually work out as *contingent* constructions, rather than to succumb to a temptation to make the contingencies of doing ethnographic work appear as theoretical advances anticipated from the start. Taking this latter approach here would have been, we suggest, a misguided attempt to emulate the appearances of a 'scientific' model which has either never existed (Knorr-Cetina 1981b) or at least represents an unhelpful mystification of the social processes involved in doing all research (Lowe 1992; Strauss 1987).

Some readers may be discouraged by how we have outlined the possible pitfalls and the wide range of complex issues which inevitably become part of doing an ethnography. It may appear to some that ethnographies are impossible to 'get right'. This may be true, and neither of us feels that we have ever fully succeeded in doing our own 'properly'. However, it is far more satisfying to feel that you have addressed these issues rather than tried to avoid them under some quantitative epistemology. Ethnographies may lack the apparently 'concrete' results of other methods (with hypotheses proven or not), but an honest and serious engagement with the world is not a failure because it admits that things are messier than that and tries to think through the various complexities and entanglements involved rather than to deny them. We would encourage any such effort.



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