Doing Ethnographies

Preparing for Fieldwork

[p. 17 ↓]

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> 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 and
> Rossman 1989: 21).

INTRODUCTION

In this chapter we set out in detail the kind of preparation which we feel is necessary to avoid the pitfalls of the read-then-do-then-write model of research. The main issue here concerns the 'surprises' which emerge when deduction and induction, data and theory, collide, by accident and design (Comaroff and Comaroff 2003; Willis and Trondman 2000). For us, the most important issue is how researchers can set up and deal with these surprises. As we argued in the introduction, organising work via the read-*then*-do-*then*-write model can engineer big surprises as researchers move from the reading to doing stages. This is not perhaps the best way to experience the most fascinating aspect of ethnographic research, i.e. what you don't expect to discover. However, we argue, by dispensing with that linear model and, instead, mixing up reading, doing and writing from the very beginning of a project, surprises are still encountered but they're often much smaller, easier to respond to and should help to shape research that's simultaneously interesting, relevant and doable. Below, then, we build on the considerations outlined in the previous chapter to think through the more practical aspects of starting a research project. Before thinking about the kinds of detailed relationships between ideas, literature and methods which have to be outlined in research proposals, we argue, it's important to have cast a preliminary research net, initiated access to appropriate people and places and thought through the role of language, power relations and ethics.

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 **[p. 18 ↓]** methodological, theoretical and linguistic preparations accordingly. Here, it is a good idea to:

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, you can ask who else might be worth talking to about the topic in hand: ask for an address, a telephone number, an email address or an introduction and try to snowball contacts on from there (Cassell 1988). Ian's ethnographic research on a Jamaican papaya 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 he knew and, by and large, trusted probably made Ian seem **[p. 19 ↓]** a somewhat accepted part of an already known community rather than a completely unknown and difficult-to-place stranger. This, it must be stressed, is a far from unusual research tale and illustrates how projects often

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come into focus through this kind of networking (see, for example, Davies 2003; Keith 1992).

In these initial stages, you might also consider the need for research permits and visas needed for any overseas fieldwork. These are not needed for all countries. When Ian did his research in Jamaica, for instance, British citizens did not need any sort of visa to spend up to six months in a country in the Commonwealth Caribbean and this was one of the reasons he decided to do the bulk of his 'fieldwork' there. However, in some cases researchers may have to apply for a research visa perhaps six months to a year in advance with no guarantee of getting it. It took Mike an unexpected seven months to get a research visa for Malaysia, for instance, 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. Our advice is to start off by contacting other researchers who have recently conducted fieldwork there, and ask their advice about official and unofficial procedures. When the former routes seem too difficult to negotiate, researchers often end up weighing up the pros and cons of entering their chosen country on a tourist visa (Sidaway 1992), a processes which raises some thorny political and ethical issues about who should control what kinds of research get done by whom and where.

Casting your net widely in the early stages of an ethnography, then, is vital. This process may be more influential in determining the shape of your research than any theoretical minutiae pored over in the academy. As we have said, 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, relevant and doable. Detailed research projects will eventually come together this way, but not without time, effort, imagination and, to mix metaphors, a willingness to see things - at least at the start - in a relatively soft focus.

INITIATING ACCESS

Earlier, we argued that research *on* social relations is *made out* of social relations and that, given the geographical aspects of identity politics, the subjects and sites of ethnographic encounters are intimately related. Thus, we argue, it is important early

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on in a research project to think about issues of access to social groups you wish to work with/in and/or the spaces in/between which you could conduct your research. Perhaps **[p. 20 ↓]** the stereotype of research is that it has to be 'all new': going boldly where you have not been before. However, for many researchers, projects develop out of already-existing memberships of social groups and/or access to particular spaces. First- or second-hand experience of an issue both 'out there' in the 'real world' *and* 'in here' in academia often provides the spark and motivation for ethnographic research projects (e.g. Saltmarsh 2001). Here, it is important to acknowledge, the 'expanded field' of academic research is already at work. Students may already have been working at a restaurant during the holidays and been concerned about working conditions, food contamination and food marketing; they may have been looking after their friend's children for a number of years and asked why it was so difficult to get a child in a pushchair around a city centre; they may have been to a number of music festivals and wondered what produced that fleeting sense of community they often felt with so many strangers in a field; they also may have come across related literatures during their degrees, and may wish to bring these 'outside' and 'inside' interests together in their research (see Cook 1997b).

The examples above have direct connections to the students' lives but, as we argued in the previous section, researchers' involvement in diverse social networks can mean that access to apparently distant groups and spaces can often be only a few steps away. Workplace ethnographies, for example, can start with jobs which students already have but also with the *kinds* of jobs or job training for which employers would expect them to apply. Thus, the early stages of a research project could begin by scanning a local newspaper's 'Situations Vacant' columns, enrolling with an employment agency, enrolling on a training course, asking a friend or family member to put in a good word with their employer or contacting previous employers to see if they have any vacancies for a tried and trusted worker. Indeed, with the financial difficulties experienced by many undergraduate and postgraduate students alike, one advantage of taking on such work is that it can double as a means to earn much-needed cash (Crang 1994).

In contrast, if a researcher's interest is in studying domestic or leisure activities such as household labour, TV watching, shopping activities or membership of particular social clubs, political/campaign groups or subcultures, then she/he must somehow negotiate access to their appropriate spaces. Although the aim, at this stage, may

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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 work such as 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 within and between various settings such as the local high street, their homes, workplaces, bars and clubs (see also Taylor 2004). Moreover, when setting up interviews or group work especially, **[p. 21 ↓]** 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 a research process is when you receive a steady stream of rejections to initial enquiries. 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 and 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 the 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 'data' because they are likely to involve 'gatekeepers' assessing aspects of your identity which are considered (in)appropriate for them (see Mountz et al. 2003; Thornton 2000). 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 therefore necessary to consider how you portray 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 wits' end after a stony meeting with the Deputy President of the local branch, Mike produced his research permits from the Prime Minister's Office that, 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 **[p. 22 ↓]** something that would prove his status, he came across his UK 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 that, at the time, was printed on every card, '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 work out, sometimes better than could have been planned. But, in terms of the time that this can take, this can 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, for example, who may then refer you to another employee. If this meeting takes just 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 over the same period of time. We would therefore 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 preplanned schedule. So, again, perhaps the best advice here is to prepare to be flexible.

TALKING THE TALK

In the process of casting your net and of initiating access to the people and places you wish to study and/or work with, issues of language will **[p. 23 ↓]** inevitably surface. You may notice differences in the ways in which you and your contacts tend to talk, in your styles of written and spoken language and/or in how you use often taken-for-granted bodily gestures in communication. Those planning to undertake a significant part of their research in a second (or third, etc.) language may expect to encounter such differences. Those with a multi-lingual background may already do so as a matter of course (Marcus 1998; Temple and Young 2004). But those working in their first and only language may also (perhaps unexpectedly) have similar issues to tackle. Given that the goal of ethnographic and related qualitative research is to understand something meaningful about the lives of other people, the language(s) within and between which this understanding develops requires some detailed thought. When preparing for fieldwork, two main questions need to be addressed in this respect. First, to what extent should the researcher's linguistic competences or 'pure' research interests decide where and with whom their research is best undertaken? And, second, how effectively can they then usefully translate meanings from the language(s) used by their research participants into those that they and their likely audiences like to hear?

We argued in the previous chapter that researchers' projects often develop and are shaped through (im)possible connections. What we want to argue here is that linguistic competences, capabilities and opportunities are important elements of this process. One of the reasons why Ian undertook the overseas research for his PhD in Jamaica, for instance, was that he was fluent in only one language: English. While he was initially

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keen to learn Spanish in order to increase his options for overseas research, this had been ruled out in supervision because of the tight deadlines for the completion of PhD theses in the UK. One of his priorities in the early stages of his research was, therefore, to find out from supermarket buyers what English-speaking countries in the 'Third World' they got their tropical fruits from, year-round. Jamaica was the only country, at that time, which fitted the bill. Please don't think that this story has been told in order to argue that allocating limited research time and resources to learning a new language may often be unnecessary.[1] In many specialist academic fields (here we are thinking, in particular, about area studies), language learning is expected and incorporated into (in)formal research 'training'. And many researchers enter academia with already-existing multi-lingual skills gleaned from previous schooling, travels, family life and other experiences. The point we want to make is that, in order to make decisions about where and with whom a research project should be undertaken, equal attention should be paid to practical issues like researcher's linguistic abilities and opportunities as to more theoretical issues like where and with whom a literature review suggests a project **[p. 24 ↓]** might best be done. These issues of language and theory are clearly connected. George Marcus, for example, has suggested that anthropology's move towards studying and theorising transcultural worlds has coincided with its recruitment of more transcultural researchers who have 'fluency in more than one language and who are at home, or at least familiar, with several culturally distinct places through their autobiographies' (1998: 247). So, the question becomes, how could you make the most of your abilities in this respect?

Whatever language(s) in which a research project is conducted, there will inevitably have to be some kind of translation between the language(s) that the researcher learns to use in 'the field' and that/those which she/he should use when presenting her/his findings to academic and other audiences. When the results of qualitative research are published and its research participants are quoted, what is often exciting for readers is that sense that we are gaining an insight into the lives of other people as described in *their* 'own words'. But because this aspect of language is so often made invisible - as if translation from one language into another is a technical, data-handling exercise and does not, therefore, need to be discussed - questions of precisely whose words they are and whose insights they represent are rarely asked (Borchgrevink 2003). As readers of such work, Bogusia Temple and Alys Young (2004: 163) argue, we should wonder:

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> What language was the data collected in? At what stage were
> the interviews translated and transcribed? What translation and
> transcription issues were there? [An interview] quote could be from a
> woman speaking English or it could be from an interview in another
> language that has been translated, presumably by the researcher. What
> is the researcher's relationship to the interviewees …?

Most of our discussion of transcription is presented in the next chapter. Here, however, we need to pay more attention to the role of translation in ethnographic and related qualitative research.

Shirley Ann Jordan (2002) argues that three strands of translation are woven through any ethnographic research process. The first consists of those translations made, over time, in field-setting encounters where both researcher and researched try to make sense of the other's ways and lives, there and then, in their own terms. The second consists of those made by researchers in order to communicate this sense-making in terms which can be understood by audiences elsewhere who weren't there, then. And the third consists of those made by members of those audiences as they attempt to make sense of these accounts in *their* own terms. Here, as you may imagine, there is an awful lot of room for 'meaning [to be] lost and invented' (Hoggart et al. 2002: 260). Translation can rarely, if **[p. 25 ↓]** ever, be a straightforward technical exercise of matching 'conceptual equivalence[s] across languages' (Temple and Young 2004: 165) because languages are rarely, if ever, structured along parallel lines and expressions of meaning are multi-dimensional, taken for granted, contextual and only partly 'linguistic'. In field settings, it is important to appreciate that 'almost any utterance in any language carries with it a set of assumptions, feelings and values that the speaker may or may not be aware of but that the fieldworker, as an outsider, usually is not' (Philipps 1960 in Temple and Young 2004: 165). What the fieldworker brings into this translation, however, are her/his own set of assumptions, feelings and values. And, if a translator is also involved - as Mike found out when a Malaysian Trade Union official helped him on a couple of occasions - yet another set of assumptions, feelings and values becomes part of the process.

What translation produces, therefore, are hybrid, in-between forms of cultural understanding in which choices have been made about whether and how to hide and/

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or highlight the failures of fit between one language and another (Twyman et al. 1999). Temple and Young (2004) illustrate this point nicely in their discussion of the choices that can be made when translating British Sign Language (BSL) into written English. These two languages by no means work along parallel lines because:

> BSL in common with other sign languages is not grammatically structured in a linear subject-verb-object structure. Rather it is a topic-comment language in which inflection is produced through facial expression, visual orientation, movement and spatial location. It is thus possible to produce complex multi-layered expression in what may seem to be a very short sign utterance but which in fact corresponds to an awful lot of English words and long sentences (2004: 166).

So, should the translation of BSL into written English involve turning these very short, multi-dimensional topic-comment expressions into much longer, one-dimensional subject-verb-object expressions? This is the neat option: undertaking an apparently direct translation from one language to another. However, other translations are possible and can, themselves, make important points about the topic under consideration:

> It is interesting that in his work as a deaf academic who uses BSL, Ladd (2003) often chooses to self-consciously represent the translation act in the English rendering of data originally produced in BSL. (Typically he 'translates' the BSL into atypical English grammatical forms with added contextual information and extensive use of ellipse and phonetic play). However, in doing so he is not simply demonstrating the problems of language equivalents…. He is also using the strategy of making translation visible to make Sign Language visible through drawing attention to the structural differences of signed and spoken/written languages and celebrating the failure of fit between the two (2004: 166-67).

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The politics and practicalities of translation (and, it must be said, transcription[2]) are, therefore, intimately connected. So, it is worth asking, how can these thorny linguistic issues be dealt with earlier on in the research process, as you are trying to put things together?

First, it may be sensible to develop a linguistic self-reflexivity from start to finish of a project, because a researcher's (and her/his translator's) language(s) and world view(s) will shape her/his/their findings just as much as those of the researched (Borchgrevink 2003). When difficulties in establishing shared meanings become apparent during field work, these will need to be described in the researcher's field note book, as will subsequent encounters in which, hopefully, these meanings become clearer (Jordan 2002; Twyman et al. 1999). Moreover, to extend this reflexivity to include the role of translators, one extra duty should be added to their job specification. Temple and Young (2004: 170) argue that translators should be treated as 'key informants rather that as neutral transmitters of messages'. Thus, not only could you, perhaps, ask your translator to interview people and transcribe and/or translate the recordings, but you can also ask her/him to take part in (tape recorded?) discussions with you about how they were and could be interpreted (see also Borchgrevink 2003; Twyman et al. 1999). Second, this means that research methods may need to be adopted and adapted so that the contextual meanings of words can be better appreciated. For instance, a project that was initially going to comprise only interview research might usefully be complemented by participant observation because, 'The solutions to many of the translator's dilemmas are not to be found in dictionaries, but rather in an understanding of the way language is tied to local realities, to literary forms and to changing identities' (Simon 1996 in Temple and Young 2004: 165; see also Jordan 2002). Given that qualitative (and other) research involves *informal* participant observation anyway (e.g. hanging around, waiting to meet people etc.), all that may be necessary is to plan to keep a participant observation style research diary (see later) detailing relevant conversations, observations and so on which take place 'off the record'.

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POWER, KNOWLEDGE AND ETHICS

As our discussion of language briefly showed, research is always bound up in issues 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 that researchers have to direct at tackling the immediate problems of getting through each part **[p. 27 ↓]** of their work may mask how she/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)[3] yet, on the other, these are also the people who usually have the power to bar the researcher's access, or stifle what they say through research contracts (Bradshaw 2001; 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, and we would argue elsewhere, 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; Sidaway 2000b; 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 having such a 'viper in its bosom' (as Mike was described, half-jokingly, by some Civil War reenactors).[4] 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). What may be seen in the academy as rigorous scientific accounts often seem ludicrous and/or happenstance to those whose lives they describe. But these experiences become far more than 'funny stories' when researchers' initial impressions produce tragicomic misunderstandings **[p. 28 ↓]** that then shape others' research in, and government policy relating to, the same people and/or place (see Smith 1999; Torgovnick 1990). For a many oft-studied peoples, then, 'research' may be 'the dirtiest word… [their] vocabulary' (Smith 1999: 1). What therefore need to be questioned are researchers' precise motives. Are 'we':

As a result of these questions being so repeatedly asked, dominant representations of the research process as a cool, scientific, non-exploitative process have *themselves* begun to appear quite ridiculous (Abu-Lughod 1990; England 1994; Mascia-Lees et al. 1989; Moore 1988; Oliver 1992; Schrijvers 1991; Smith 1999).

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 such work. Researchers could:

None of these approaches, separately or even in combination, will necessarily solve the problems outlined here. However, 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.

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As well as dealing with the politics of knowledge by thinking through more personal and situated ethics in your research process, you may also be required to submit a formal set of Research Ethics during the early stages of your research. Ian had had to gain

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'Human subjects approval' to undertake his MA research in the USA in the late 1980s, but the formal consideration of ethics has only recently become a common requirement for UK researchers. Increasingly an 'ethical review' of your proposed research may have to be written for assessment and approval by internal and/or external assessors before your 'field' research can formally start. The UK's Economic and Social Research Council (ESRC), for example, provided a very short list of three minimum 'ethical considerations' to be outlined in applications for PhD studentships:

However in the last year this has expanded to 37 sides of Research Ethics procedures and guidance (although that has only increased the substantive issues covered to 6 bullet points - see Box 3.1).

Box 3.1: 'Ethical Considerations' to be Included in ESRC Funding Applications

Source: ESRC Research Ethics Framework (2005: 1).

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Other bodies may provide lengthier lists and have more formal procedures for evaluating whether they are met.[5]

On many levels, such lists of considerations appear sensible and well intended. However, they also often seem to rest upon questionable assumptions about how research should be organised, how it can be done well and how institutional politics affect how ethical standards are assessed and monitored in different places (Bosk and de Vries 2004; Gordon 2003; Marshall 2003; Plattner 2003; Punch 1986; Thrift 2003). One assumption, for example, appears to be that the research process is divided up into stages (e.g. read-*then*-do-*then*-write) where 'ethics' must be 'sorted out' before starting 'fieldwork'. Throughout this book, however, we draw upon examples of 'ethical' research in practice that turn around every one of even the ESRC's older minimalist three-point list of considerations. In terms of the first, we have experience of situations where being 'honest' with the people involved in our research may have

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been 'unethical', and where such 'honesty' was extremely difficult when the purposes, methods, uses and risks of research were changing as projects proceeded. When research changes as you do it, yesterday's honesty can often become tomorrow's apparent lies. In terms of the second consideration, we have found ourselves in circumstances where confidentiality was very difficult to maintain and, indeed, where research participants have insisted on not having it. Finally, in terms of the third consideration, we have already questioned whether research can or should be 'independent' or 'impartial' when we live in a world where gross inequality and injustice is all around us. Indeed, it must be acknowledged that many researchers are drawn to issues precisely to tackle inequalities and injustices (Cloke et al. 2004; Scheper-Hughes 2004).

It is important to point our here, then, that challenges to establish, maintain and/or revise your ethical stance will not only come from within academia. Rather, they may also have to be negotiated between the various locales of your research. Ian, for instance, has written about the ethical challenges presented to him by the papaya farm manager ('Jim') and his friend the sugar farm boss ('Tim') who had introduced Ian to him. Both had been extremely hospitable, both to Ian and to Michelle another English PhD student working in the area. Following Michelle, Ian had rented a room in Tim's Great House in the neighbouring valley to Jim's farm, and Jim had subsequently asked Ian if he would like to look after his brother's house on the farm while he was away. Tim, Jim, their families and friends also invited Ian and Michelle to parties and on fishing trips and, when he couldn't get to Kingston with Michelle in her car, he relied on them for lifts. So, what did he owe them back? As he has written elsewhere:

> I got to know Tim and Jim very well through my research, both as people from whom I learned a great deal about fruit farming in Jamaica, and as people who **[p. 31 ↓]** I would hang out with socially. But, as my research progressed there over a period of six months, the hospitality and frankness which they had initially offered became increasingly punctuated by their anger over the 'brass-necked' nature of what I was doing. What, they argued, gave me the right to swan into their lives, look closely and critically at their finances, business methods, family lives, and, perhaps most sensitive, ways of dealing with their increasingly impoverished workforces and then fly away and write

about this as if I didn't equally owe my livelihood to the ugly means
of exploitation I obviously saw in theirs? Given that, at that time, my
parents had been running their own business for 32 years…, their
most disturbing question concerned whether I would even *consider*
researching how they had made their money off other people and then
speak about it critically in an academic arena. And, although much of
this line of argumentation could be seen as tactical - their playing off
what they saw as my 'misplaced socialist idealism' against what they
knew about my family background to persuade me where my ultimate
loyalties should perhaps lie - I could not deny that they had a fair point
and this was something which, if these ideals were to remain somehow
intact, I would have to deal with in my work (Cook 2001: 114-15).

But this was not all. 'Jim' also became concerned that, once published back in the UK,
Ian's research could provoke a consumer boycott of his fruit. And did Ian know who
would suffer the most if this happened? The farm workers he spent so much time talking
to and seemed to care about the most. This situation therefore led to an ultimatum,
presented when Jim was giving Ian yet another lift to Kingston. After pulling off the road
for a 'chat' about Jim's concerns, they ended up agreeing that, in order for Ian to be
allowed to continue his research on the farm, he would have to anonymise the fruit.
These two challenges, about writing about them and about writing about it, both had
to be met. Even though 'Jim' didn't remember making the first challenge (as Ian found
out years later after bumping into him at an 'ethnic food fair' at Birmingham's National
Exhibition Centre), Ian included discussion of his own family and family business in his
PhD and in subsequent publications (see Cook 1997a, 2001; Cook et al. 1998). Having
done this, he felt that he could more justifiably write about Jim's family and business,
still appropriately anonymised of course. Second, a decade after this research was
done, when Jim's farm was no longer supplying that fruit to UK supermarkets, and after
two of the intermediary companies had gone out of business or been sold off, Ian felt
that he could begin to say that he worked on a *papaya* farm, and show how the fruit
itself made a difference to its trade (see Cook et al. 2004a, b).[6] Naming it in print in
2004 could not do any harm to the people he cared about, surely, so Ian felt that he was
free of that in-car promise.

In sum, then, perhaps we need to think in terms of two kinds of research 'ethics'. First, there are those with a capital E that comprise the broad and fixed principles that might help to shape our plans when research proposals and 'ethical reviews' have to be submitted. And, **[p. 32 ↓]** second, there are those with a lower case e that feed into and emerge from the smaller, everyday encounters tied together throughout the research process. These are a messier, ongoing, impure, continually updated set of ethics that develop over time and through experiences. These result from situated decisions and ongoing debates about how we each should act in a world where behaving ethically often doesn't seem to be the foremost consideration shaping other people's actions. Few, if any, of us can act like a saint who is able to go into and emerge from their research unscathed by ethical wrongdoing. Doing 'the right thing', or knowing what the right thing is in the first place, is not always straightforward or apparent. Indeed, at the end of a process full of countless uncertain, failed and/or successful attempts to act properly with respect to all of the others involved in your research, you are likely feel that, despite your best efforts, your ethics have been compromised; that they are, in fact, quite grubby, and that, if you had been a better person (or at least got more sleep), you would have been able to do a better job. We have certainly felt all of this. And this is surely normal.

SUMMARY

In the previous chapter, we argued that in order to undertake ethnographic and related qualitative research, it is necessary to have a critical, conceptual, geographical understanding of the (inter)subjectivity of researchers and researched, and the groups (e.g. 'cultures') they may be seen to be part of. In the next chapter, we outline the practicalities of a variety of approaches to undertaking qualitative research which you may wish to adopt during the kind of intensive 'fieldwork' that usually gets done later on in a project: for example, when the event that you've been waiting to happen eventually takes place, when you have to make that overseas trip or when a systematic series of interviews finally gets arranged. This chapter has outlined the 'doing' that, we argue, should be done alongside reading and writing from the very start of a project. Specifically, we have tried to encourage readers to recognise and make the most of the skills and opportunities that they already have, and could usefully develop, by:

This is practical advice that, we believe, can enable prospective ethnographers to avoid the pitfalls of the read-then-do-then-write model of research. Putting together a doable research project, with its formalised methodology, should result from reading, doing and writing taking place alongside one another, being informed and critiqued by one another, so that that project can change and take shape from the start.

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