

Stuck in GUM: Life in a Clap Clinic

Melissa Parker

Introduction

Over the last ten years or so anthropologists have undertaken an increasing amount of research in health-care settings in the Western world (e.g. Donegan 1998; Kayser-Jones 1990; Krause 1998; Modell 1989; Rapp 1988; Sinclair 1997; and Pulman-Jones, Chapter Six in this volume). This chapter adds to that growing literature by focusing, for the first time, on a clinic specializing in the treatment of sexually transmitted infections. The clinic is part of a teaching hospital in a British city and the fieldwork took place over a two-year period between December 1993 and December 1995. In view of the dearth of anthropological writing, the chapter is deliberately descriptive and sets out to do the following: first, to describe the atmosphere and ethos of the clinic and, second, to demonstrate the different ways in which the emergence of the viral infection HIV in the 1980s has helped to create a culture of work that is unique to clinics which have a large number of patients registered with HIV and AIDS and specialize in the treatment of other sexually transmitted infections. With respect to the latter objective, the chapter analyses conflicts between staff as well as the fears and prejudices displayed by staff towards different types of patient in order to explore the extent to which wider social fears and concerns are both warded off and acted out within the clinic.

Before engaging with these issues, I present here a brief description of the clinic. This is followed by a discussion of the type of contact I had with staff and patients with a view to generating useful background information and highlighting both the strengths and the limitations of the research findings presented in this chapter.

A Description of the Clinic

The clinic is part of the Department of Genito-Urinary Medicine which, in turn, is part of a teaching hospital. The department is often referred to by its acronym 'GUM' and it is located in a modern, three-storey building. This building is solely used for clinical practice in genito-urinary medicine and HIV medicine as well as clinical research investigating a broad range of issues. These include clinical trials

documenting the relative merits of different drug combinations for the treatment of people with HIV and AIDS; research seeking to develop an effective vaccine against HIV; and research investigating and developing non-invasive screening tests for the bacterial infection chlamydia.

With respect to clinical practice, there are three clinics. Each clinic is located in a different part of the building and has a different name. The first clinic is a walk-in GUM clinic (where anyone can walk in off the street and request to see a doctor, nurse, or health adviser without prior appointment). The second clinic monitors the success of drug treatments for infections such as chlamydia and gonorrhoea (which have been diagnosed and treated at the walk-in clinic) and it also provides follow-up treatments for chronic sexually transmitted infections such as genital warts and genital herpes. The third clinic is solely for patients who have been diagnosed with HIV and/or AIDS.

All three clinics are open five days a week and they are attended by a large number of people. In July 1995, for example, more than 5,000 people came to this particular clinic and the majority (70 per cent) attended the walk-in GUM clinic. A further 18 per cent attended the second clinic and 12 per cent attended the HIV clinic. A number of other services are also provided by the clinic. These include drop-in centres for male and female prostitutes respectively. The drop-in centres provide confidential, advisory services and the staff run special clinic sessions for those prostitutes who wish to be screened and/or treated for sexually transmitted infections.

Numbers of Staff

A large number of staff are employed at the clinic (approximately 80–100 people) and, in common with many other biomedical institutions, the employment structure is shaped much like a pyramid. At the top are the consultants in HIV medicine and genito-urinary medicine and then, in ever-increasing numbers, staff are employed at the level of senior registrars, registrars, and nurses. The nursing staff outnumber the doctors by a ratio of approximately 2:1 and there are a further four to six health advisers employed at the clinic as well as a large number of administrative and clerical staff.

It should also be emphasized that the clinic has a dual hierarchical structure. That is, there is a consultant who is also responsible for all clinical practice at the clinic and a consultant/professor who is responsible for all research undertaken in the department. In theory, these two heads work closely together. In practice, their relationship is full of tension and conflict and this is not always creative or to the public's advantage. It is also interesting to note that the majority of staff employed at the clinic are in their 20s or 30s. Even the consultants are young. When I first started working at the clinic, for example, the head of clinical practice was in her late 30s and the head of research in his early 40s.

Participant Observation in the Clinic

I first became interested in the medical speciality of genito-urinary medicine when I was asked to contribute to epidemiological, microbiological, and anthropological research investigating sexual networks and the transmission of gonorrhoea. The research project was funded by the Wellcome Trust and, since it was anticipated that the majority of participants were going to be recruited at a clinic for sexually transmitted infections, I was appointed as an honorary senior research associate at the above-mentioned clinic (where the study was going to be based) to enable me to have easy access to patients.

Throughout my time at the clinic, I had two distinct roles: first, I worked as an honorary health adviser with responsibilities to provide appropriate health advice to all patients who tested positive for gonorrhoea (as well as their sexual contacts who may or may not have tested positive for gonorrhoea) and second, in my capacity as a researcher, I undertook open-ended, unstructured interviews with some of these patients both in- and outside the clinic. Interviews with patients usually took place after I had dispensed appropriate health advice in my capacity as a health adviser.

In both cases I carried a bleep and whenever a patient tested positive for gonorrhoea (or returned to the clinic for additional treatment, tests, or a research interview) a member of staff would bleep me and I would go and find the person concerned. The rationale for working in this way was to acquire (in as friendly a way as possible) an understanding of the number and type of sexual contacts each person had had and to persuade that person to introduce me to each of his or her contacts as well as to the contacts' contacts. In so doing it was hoped that it would be possible to identify sexual networks and thereby further our understanding of the transmission of gonorrhoea.

However, the majority of people either refused outright to participate in the study (usually by saying something like 'I don't want no bloody social worker') or said that they would be willing to participate but did not have time to be interviewed that day. The latter group then arranged an alternative day to meet but made themselves scarce when it came to the time of the interview. The following example captures some of the frustrations and difficulties of finding people who had recently had gonorrhoea to participate in the research (and thus highlights the near impossibility of talking to contacts and identifying sexual networks): Andrew¹ was a 20-year-old, black Caribbean heterosexual male who was diagnosed with gonorrhoea in January 1995. He reported having had five female sexual contacts in the three months prior to his diagnosis of gonorrhoea at the clinic and he seemed

1. The names of men and women attending the clinic, staff employed at the clinic, and the clinic itself have been altered to preserve anonymity. Similarly, information that could lead to the identification of anyone mentioned in this chapter has been modified too.

happy about participating in the research 'so long as it was not on camera'. Indeed, he gave me one of his two mobile phone numbers and willingly agreed to a date for an interview. He did not turn up at the agreed time and when I rang him at 8.00 p.m. later that day the conversation went like this:

- A: 'ello.
MP: Oh hi, is that Andrew?
A: Yeah (*this response was against a background of loud disco music and lots of conversation*).
MP: It's Melissa Parker here.
A: Who?
MP: Melissa . . . we met briefly at the Radcliffe Centre.
A: Do I know you?
MP: Yeah . . . you said you'd be willing to help me with some research that we're doing.
A: Oh yeah (*acknowledgement in his voice*) . . . umm . . . ring back in 10 minutes.

I rang him ten minutes later and a smart, female-sounding voice said: 'The Vodafone you have called is switched off. Please try later.' The next couple of times I rang I got the same response but a week later I got through. This time a man called Mark answered and said:

- M: Who is that . . . who is that?
(*To which I responded by saying:*)
MP: Is Andrew there?
M: No . . . who is that?
MP: It's Melissa, a friend.
M: (laughing) How good a friend?
MP: A reasonably good friend.
M: Oh well, you'll get him on 4443-883355.
(*Hesitating, he went on to say:*)
You're not a police officer are you? . . . um, tell him Mark gave you the number.

I rang this number at least ten times over the next few weeks and each time the telephone was switched off. At this point I called it a day. Andrew was clearly unavailable and it seemed pointless to pursue him further.

Increasingly anxious about how I was going to undertake a substantial piece of research when I could not find anyone to talk to, I thus turned my attention to two more promising areas of research: the transmission of HIV, and writing an ethnography of the clinic. The former topic was not supported by senior staff at the clinic but I pursued it vigorously, and useful and interesting material was eventually generated (see, for example, Parker *et al.* 1998; Parker 1999). The latter topic also emerged by accident. I had not been employed to write an ethnography of a clap

clinic and I probably would not have kept an on-going account of events, conversations, and conflicts at the clinic if the project I had planned to work on had not been such a failure. However, the clinic proved to be an extraordinary place to work and while the culture of work undoubtedly contributed to the difficulties of identifying sexual networks and understanding the transmission of gonorrhoea, it also proved to be a fruitful avenue of research in its own right.

The Atmosphere and Ethos of the Clinic

This section attempts to capture the unique atmosphere and ethos of the clinic by focusing on four themes which are central to the organization and running of the clinic: sex and sexuality; machismo and the intolerance of sensitivity; confidentiality and anonymity; and the clinic as a market place. The writing presented under each of these headings will ultimately be used to demonstrate the different ways in which the emergence of the viral infection, HIV, has created a culture of work that is unique to this particular clinic and, in all probability, other genito-urinary medicine clinics which have a large number of patients registered with HIV and AIDS in this city.

Sex and Sexuality at the Clinic

To an outsider, one of the most striking aspects of working in a clinic for sexually transmitted diseases is that it is characterized by a sexually charged atmosphere. Indeed, it would not be an exaggeration to say that the atmosphere is more charged than any other work environment that I have ever been in and that I have ever heard anyone else describe. It is unique and it was, in fact, the first thing that struck me when I was shown around the clinic in December 1993. Indeed, one diary entry dated 20 December 1993 simply stated:

John (a white, English, gay male nurse in his early 30s) was in the laboratory of the walk-in GUM clinic looking down a microscope at a urine sample. Stella (a white, English, research nurse in her late 20s) came up behind him and greeted him by putting her arms around his waist. She hugged herself tight against his buttocks and, playfully, pulled out the front of his trousers – heavily suggestive of the fact that she was going to put her hands down the front of them. My first thought was: 'Oh my God! Is this how people greet each other here?' And: 'I don't have to do that as well, do I?'

A month later, and still perplexed by the extent of sexually charged interactions between staff, I noted in my diary that I had overheard and/or participated in four different conversations within the space of 30 minutes. All of these conversations were full of innuendo and they either explicitly referred to a desire for sex or

explicitly acknowledged sexual feelings between different members of staff. Each conversation was fairly insignificant in itself but together they drew attention to the fact that sex permeates even the most mundane thoughts and requests. The following vignettes describe some of these exchanges.

Example one A white, male, heterosexual doctor (aged about 30 years) walked into the laboratory of the GUM clinic in January 1994. He had been working at the clinic for about a week and asked one of the nurses about the whereabouts of another nurse. The nurse replied: 'Oh, he's tied up with another patient.' At this point a group of four nurses (who were also standing around in the laboratory) started to laugh and chorused 'Tied up? . . . that sounds interesting doesn't it?!' The rather innocent, fresh-faced doctor looked a bit bemused but he clearly needed to talk to the nurse in question so he persisted by saying: 'What shall I do next?' The nurses responded in a teasing manner by saying: 'Well, you could go and join in . . . you could always lie on top of them.' At this point everyone in the laboratory groaned. It was, undeniably, a weak joke.

Example two This interchange was quickly followed by a chat between Carole (a black Caribbean nurse in her early 30s) and Paul (a white English 24-year-old gay male nurse). On this particular occasion Paul was telling Carole about a conversation he had just had with a doctor called Charles. Charles was about to leave the clinic to take up a new post elsewhere and Paul found him 'very pretty'. He thus told Carole in a matter of fact way that he had said to him: 'You know, you should let me have you [consent to sex] at least once before you go.' Carole responded with exasperation and fondness in her voice: 'Oh . . . you need one of those closet things . . . so that we can lock it [penis] up and throw the key away.'

They laughed and Paul went on to ask Carole whether or not he should contact a 17-year-old male whom he had met at a club a few days previously (with a view to having sex with him). He drew attention to the fact that although the man was young he had taken the initiative by giving his telephone number to Paul and it was not as if Paul was that old himself. As he wrestled with the issue of the age of consent and power relations it struck me that he felt totally uninhibited about discussing recent sexual developments in his life with an array of staff listening in and adding their twopenny piece.

Example three At the same time as this conversation was going on, a male senior registrar hugged and kissed a male nurse in the corner of the laboratory. Several staff acknowledged the sexual exchange occurring by giving each other a nudge and a wink but they did not think it was particularly unusual. It was just their way of saying 'hello' and wishing each other a Happy New Year.

Example four I was sitting in the receptionists' work area of the downstairs walk-in GUM clinic reading a patient's notes when the following exchange took place between a Spanish male heterosexual doctor and a black Caribbean female receptionist: the doctor had asked the receptionist 'to pull' a patient's notes and she had not got round to doing it. He had thus come into the receptionist's work area to complain about not having them and in a jokey, overtly sexual manner said: 'If you're not careful I'll take you outside and show you what I've got.' The rather busy receptionist replied by saying: 'And if you're not careful, I'll take you up on your offer!' At this point all the receptionists convulsed into a fit of giggles and the doctor beat a hasty retreat.

Needless to say, I was no more exempt from the joviality and sexual comments than anyone else. Thus a psychiatrist who ran a clinic for patients with psychosexual problems thought nothing of saying in the corridor, at the top of his voice and surrounded by patients and staff: 'I'm more in love with you than ever, Melissa' and 'I'll submit myself to anything you require . . . just say the word.' Such comments are part of the 'anything goes' ethos and no one batted an eyelid.

Sexual banter often features in the relationships of heterosexual and gay-identified staff working in different kinds of organization in the UK. Unlike the majority of workplaces, however, it was striking that at this particular clinic a large number of sexual partnerships and friendships were established across hierarchies and that sexuality was often perceived to be a more important link with colleagues than professional status. Not surprisingly, therefore, one of the first questions that anyone asked about new members of staff was: 'Are they gay or are they heterosexual?' And, in the words of one female gay health adviser: 'The fact that sexuality is a bond across hierarchies is a big attraction of the post.'

Moreover, some of the most fiercely contested issues within the clinic reflected divisions between the 'hets' (heterosexuals) and the 'gays'. For instance, there were often discussions about whether clinics should be set up exclusively for gay men and, if they were, whether these patients should be able to choose the sexuality of their doctor and, similarly, whether staff should be able to choose the patients they worked with according to the patient's reported sexuality. So far, clinics have not been set up along these lines. This, perhaps, reflects the fact that the majority of consultants are white and heterosexual and it would never be in their interests to witness these developments (as they would have access to fewer HIV patients and this would affect their research output).

Nevertheless, it was striking that the gay male nurses frequently socialized and developed sexual relationships with the gay senior registrars, consultants, and administrative staff. Similarly, the gay female nurses socialized far more with the gay female health advisers and doctors than with other staff of similar rank within the organization. Divisions along these lines undoubtedly made the day-to-day

working life of those heterosexual men who were employed at more junior levels within the clinic rather difficult. Indeed, they tended to retreat into the arms of the female receptionists at the walk-in GUM clinic as they were a group of predominantly young women in their 20s and 30s who were vigorously heterosexual.²

Machismo and the Intolerance of Sensitivity at the Clinic

A second issue to emerge during my time at the clinic was the fact that the staff employed at the walk-in GUM clinic and/or the HIV clinic and drop-in centres had to respond to an enormous number of difficult cases each day. In November 1995, for example, a senior registrar at the walk-in GUM clinic told me about the work he had done the previous day. Among other things, this had involved seeing three patients in the morning. The first patient had had to be told that he was HIV positive and the second patient that he had cancer of the testicles. The third patient presented with pneumonia and the doctor had to advise him to have an HIV test as he showed every sign of being HIV positive (which he was subsequently shown to be). The afternoon also proved to be difficult as he had to spend the best part of an hour with a patient who was distraught by the fact that she had genital herpes. When he was exhausted and about to go home, a patient turned up late for an appointment and intensely distressed. She had genital warts and showed signs of having been physically abused. This was, undoubtedly, a 'bad' day but it was striking that the doctor, in common with all other doctors and biomedically trained staff at the clinic, viewed the volume of difficult cases as 'all in a day's work'; and he did not convey any emotions about the volume of tragedy that he had been asked to deal with that day. In short, it would not be an exaggeration to say that a sort of machismo informed the way in which many staff talked about patients to other staff and, on occasions, the way in which they dealt with the patients themselves. The following case study illustrates this point further.

Case Study Theresa is a graduate in her mid-twenties and works at the clinic's drop-in centre for female prostitutes. She has an engaging, warm, open manner and spoke lucidly about the fact that (as with so many other units within the National Health Service) she and other staff at the centre felt under increasing stress as they were so understaffed and unsupported. Indeed, she described one particular occasion when she spent the best part of five hours trying to sort out accommodation and benefits for one of the prostitutes visiting the centre.

2. The fact that staff members' sexuality influenced the type of relationships they established with other staff members and the type of relationships they established with patients (see the following section for a detailed discussion of this point) reflects the growing influence of the gay lobby in influencing the provision of sexual health services, and particularly for men who are HIV positive and have been diagnosed with AIDS.

The woman had turned up in a dreadful state as she had been physically threatened by her pimp over the weekend. She described how he had threatened her with a large knife and, reliving the trauma, pulled out the knife he had used and started waving it around in the air. Theresa felt untrained and ill-equipped to deal with the situation. In fact she had been terrified by the woman's actions as no one else had been in the building at the time and she felt that the woman could easily have attacked her with it or attacked herself.

Distressed and concerned (both for herself and for other staff at the drop-in centre) Theresa made an appointment to see the business manager for GUM and HIV medicine as she wanted to use the opportunity to convey the importance of increasing the numbers of staff at the drop-in centre as well as improving security arrangements. But the manager's response was dismissive and she simply replied: 'Well, that's nothing! Jane Crowther was bitten by a patient last week!'

This, incidentally, was not a view shared by a psychiatrist who worked at the same hospital and saw Theresa for supervisory sessions once a week. He felt that an enormous load was being placed upon her (and on other staff at the centre) and commented on the fact that he saw about five such cases in a year whereas she was being asked to see about five in a week. Theresa was clearly heartened by his response as staff within the clinic rarely, if ever, responded in this way.

The intolerance of sensitivity My own experiences within the clinic also highlight this latter point. I arrived at the clinic having undertaken a large amount of research in Africa. I did not, however, have any previous experience of working within the National Health Service. I thus spent the first few weeks tracking health advisers in their day-to-day work with a view to acquiring sufficient experience to be able to see patients who had been diagnosed with gonorrhoea in my capacity as an honorary health adviser and researcher. Within six weeks of my taking on the role of a health adviser, however, a meeting was called by the health advisers with the senior management at the clinic. They were not happy about my presence within the clinic and wanted to remove my status as a health adviser. Their grounds for removal (and, therefore, the possibility of recruiting large numbers of patients to the study of sexual networks) were twofold: first, they did not think it was possible to combine the role of health adviser and researcher. They argued that patients found the dual role confusing as they were never sure where the health advice ended and the research began and this was unacceptable. The provision of quality health advice had to take precedence over everything else (though it has to be said that not a single patient had complained). Second, I was simply not the right person for the job. I was, as far as they were concerned, far too 'sensitive' and 'fragile'.

At the time, all the health advisers were female and gay and to help argue their case they drew upon the support of the business manager (a woman who was gay herself and professed to support other gay female staff at the clinic) and the

consultant responsible for clinical practice within the clinic (a heterosexual woman who loathed the consultant/professor responsible for research within the clinic and had never wanted to have this particular project in the clinic in the first place).

A fierce battle took place but, in the end, they won the argument and my status as an honorary health adviser was removed . . . at least for a while. These events were, of course, most unpleasant and confusing and it was only much later that I came to understand what had happened. Briefly, the health advisers felt resentful and threatened by my presence in the clinic. They were resentful because at least one of them would have liked to be appointed to my post (but they had not applied for it as they felt they did not have the appropriate academic qualifications); and they felt threatened as my presence played to their own insecurities: I was not a nurse, I did not have a background in sexual health, and yet within a few weeks of coming to the clinic I was perceived by many staff to be doing many aspects of their work as well as they themselves. To their minds, this only confirmed their precarious and lowly status within the clinic. None of the senior management appreciated just how experienced and good they were at their work (for if they did they would have wanted to have appointed a health adviser to the post) and since the funding of clinic posts was constantly under review, they were worried that as soon as there was a financial crisis their posts would be the first to be abolished.

They could not, of course, go to senior figures within the clinic with these concerns and thus they came up with something that everyone could identify with: I was too sensitive. After all, anyone who had worked in a clinic for sexually transmitted diseases for any length of time could recognize the dangers of being too open and sensitive to other people's difficult life situations.

With respect to these problems, it is interesting to note that they were eventually resolved in a classic way: one of the most articulate, thoughtful, and influential health advisers was paid, out of grant money, to work on a closely related research project on a part-time basis. In so doing, she became fully involved in the research investigating sexual networks and the transmission of gonorrhoea. It thus became possible to rely on the strength of her ties within the clinic to ensure that contact could be made with patients who received a positive diagnosis for gonorrhoea. In other words, the problems resolved themselves and I was gradually integrated back into the life of the clinic. Within a few months my status as an honorary health adviser was reinstated and I was allowed to see patients who had been diagnosed with gonorrhoea. In fact, by the end of the study, the very people who proved so hostile to me turned out to be some of my best informants and greatest allies at the clinic.

It is also important to mention that the health advisers' concerns about their lowly status and job security were fully justified. In the summer of 1996, for instance, there was a funding crisis and, disillusioned with their position in the clinic and the increasing likelihood that some of their posts would be abolished,

four of the five health advisers resigned together. The fifth health adviser resigned a few months later. These posts have not been reinstated as the view taken by the senior staff at the clinic is that health advice can be delivered just as well by doctors and nurses and that it is not necessary to fund posts solely for the purpose of handing out appropriate health advice.

Confidentiality and Anonymity at the Clinic

The third theme addressed in this section concerns the issue of confidentiality and anonymity at the clinic. The image projected to the public is that of a clinic which provides a confidential, non-judgmental service, and the staff are thus obliged to ensure that they do not discriminate by sexual orientation, ethnicity or gender. There is a substantial body of legislation as well as codes of professional conduct to support this outlook but the reality, not surprisingly, proved to be altogether different. One particularly vivid illustration of this was entered in my diary under the heading: 'Pets at the clinic' and it said:

Today I had lunch with some of the nurses and health advisers in the common room. A young, male nurse came into the room towards the end of our lunch hour and started to describe the morning's events at the downstairs walk-in GUM clinic: a man had presented himself at the clinic with an unidentified animal stuck up his anus. The tail was hanging out and he came to the clinic to ask someone to pull it out. The young nurse went on to say that he and another colleague had been in a room adjacent to the male patient when he was having it pulled out. They had managed not to laugh out loud (as they knew the walls were paper thin) but their shoulders had heaved with silent laughter.

There were at least 12 people in the common room when the nurse described this event. It was striking that amidst embarrassed, shocked, and horrified laughter, others joked. One of the health advisers, for instance, had said that the clinic should replace its current sign 'welcome to the Radcliffe Centre' with one reading 'pets are us'!

The nurse, encouraged by the laughter, went on to describe the man's clinic notes. Against the acronym LSI [last sexual intercourse] the doctor had written '?'; and against the acronym PSI [previous sexual intercourse] the doctor had written: 'chicken (dead)'. Reflecting on these notes, he then said: 'Does this mean he had sex with the chicken once it had died or that it had died after he had had sex with it?!'³ The laughter continued but as soon as it subsided, the nurse said: 'I tried really hard to like him . . . honestly I did . . . [but] he stank of urine.' He then went on to describe how other aspects of the

3. A female nurse said that he should write to Sainsbury's to let them know that people are using chickens for sexual purposes as it would give a whole new angle to marketing and Christmas. She went on to say, 'You can imagine him picking up chickens' and she gestured with her hands about how it might be done, including looking at the size of the chicken's anus. Another staff member, continuing on the theme of having sex with chickens, commented on the fact that 'baste at hourly intervals' now took on whole new meanings.

patient's behaviour had really annoyed him such as the fact that it had taken him ages and ages to move from one end of the room to another. By the end of the lunchtime break, the general consensus among the staff present was that the clinic's ethos had gone out of the window as not a single person in the room had managed to be non-judgmental.

The next example also illustrates the difficulties of maintaining a non-judgmental outlook. In fact, it suggests that the nature and extent of discrimination at the clinic varied according to the gender and sexual orientation of the patients. The example concerns people who repeatedly test positive for gonorrhoea. That is, people who receive a positive diagnosis for gonorrhoea as many as three or four times over a period of several years. These people are often referred to as 'repeaters' in the epidemiological and clinical literature; and the health advice they receive is sometimes delivered in a hostile and antagonistic way. This is on the grounds that they did not appear at all concerned about the consequences of repeat infections either for themselves or for their partner(s). Unfortunately, the majority of people suffering from repeat infections at this particular clinic were black, Caribbean, heterosexual, and male. The health advisers at the clinic (who, at the beginning of the study, were all white, female, and gay) were openly hostile and aggressive about these types of people when they talked among themselves, and any patient falling into this category was quickly labelled 'Mr GC'.⁴ One health adviser, elaborating on the 'Mr GC' stereotype said: 'You know the type . . . he's tall, black, carries a mobile phone, and sits with his legs splayed out so that he can trip you up whenever you walk past him in the corridor.' At this point she gestured to the crutch indicating that the shape of his balls are always revealed by the tight clothes that he wears. After a brief silence she went on to say: 'Oh yeah, and when you see him, he says, "Give us the drugs . . . I don't want no lecture".'

The health advisers' hostility, antagonism, and distress were sometimes palpable in a 'GC interview'. In January 1994, for example, I observed the following interaction between a white English, female health adviser and a black Caribbean, heterosexual, male patient: the patient had just been diagnosed with gonorrhoea for the fourth time in two years. He appeared to be fairly nonchalant about the diagnosis. That is, he shrugged his shoulders and did not articulate any concerns about whether he had transmitted this new infection to or acquired it from any of his female contacts. He also refused to be drawn about whether he would contact any of his female sexual contacts with a view to encouraging them to come to the clinic to be tested for gonorrhoea. The health adviser became increasingly angry and frustrated with his public persona and rounded on him at the end of the interview by saying: 'If you go on like this your dick will fall off.' He left the

4. 'GC' is the acronym used by staff at the clinic for the bacterial infection *Neisseria gonorrhoea*.

consulting room a few minutes later and, despairing, she turned to me and said: 'I hate patients like that.'

It would be quite misleading to suggest that health advisers were the only group of staff to struggle with codes of professional conduct requiring a non-judgmental attitude. It would also be misleading to suggest that black Caribbean, male, heterosexual patients were the only group of patients to be on the receiving end of staff hostility. Moreover, there were several occasions during my time at the clinic when the confidentiality of patients was breached on an individual and collective basis. For example, the following occurrence was reported by a female doctor at one of the clinic's monthly staff meetings in 1995: a patient with an HIV diagnosis was waiting in one of the consulting rooms of the HIV clinic for a nurse to come and carry out some routine tests and noticed a piece of paper on the ground. He picked it up and realized that it was a computer print-out listing all the patients who had died while registered at the clinic with HIV. Among other things the list specified their dates of birth and TCell counts at the time of their death. The patient, not surprisingly, was upset that such sensitive and confidential information was being left on the floor for staff to kick around and he was concerned by the disrespect implied by such carelessness. The doctor reassured the staff at the meeting that a formal complaint had not been made but urged everyone 'to take great care' and not to get 'too complacent'.

In sum, there were several indications to suggest that staff struggled to provide a confidential and non-judgmental service. The marginal and least articulate patients (including the deceased) were particularly vulnerable to breaches in professional conduct. This was largely due to the fact that staff were required to see a large number of patients facing difficult and often intractable health problems each day. The culture of work promoted a robust and emotionally detached attitude to work and the difficulty of sustaining this outlook meant that some of the most vulnerable were susceptible to professional misconduct. It is also likely that the desire to create a 'safe haven' for HIV patients to retreat to, and the difficulties – if not impossibilities – of talking about the difficulties of working with some of these patients (such as men who openly talk about infecting other men) meant that anger and hostility was often displaced onto others.

Money Speaks: the Clinic as a Market Place

The final theme addressed in this section concerns the increasing tendency by staff to view the clinic as a market place. This outlook primarily reflects the impact of political and economic changes imposed upon the National Health Service in the 1980s and early 1990s but, whatever the explanation, it would not be an exaggeration to say that considerable attention is now paid to monitoring the number and type of patients attending the clinic as this influences the amount of funding received and the type of services that can be provided.

That the clinic was best viewed as competing in a 'market place' with other clinics for patients (who, incidentally, were increasingly referred to as clients) could be seen at the monthly meetings held for all staff: the first item on the agenda was an overview of the number of patients who had attended the three clinics during the previous month. On those occasions where there was an increase in the number of new patients registering at the clinic, the clinic business manager and the head of clinical practice always made a point of congratulating everyone for successfully finding and recruiting these patients. This was particularly the case with patients who had been diagnosed with HIV, as each new HIV patient brings in at least £12,000 a year. In short, more patients mean more money and this, in turn, influences the quality of services provided. Moreover, clinical staff cannot contribute to research investigating the relative merits of different drug combinations unless patients with HIV are recruited to the clinic and a reduction in numbers means a reduction in research funding (which is another important source of income to the clinic).

Another indication of the importance of maintaining, if not increasing, the number of patients registered at the clinic is the fact that information about the number of patients (and particularly HIV patients) registered at a clinic is perceived to be extremely sensitive and confidential. For example, a senior registrar in HIV medicine at the clinic told me in November 1995 that he had recently been shown around a smaller hospital with a view to applying for a consultant's post. He had not asked anyone about the number of HIV positive patients registered at the hospital – even though the post entailed caring for HIV positive patients – as to ask would somehow cast aspersions on the unit. To his mind, the very question would send out the wrong message to the appointments panel as it would suggest that he was concerned that the consultant's post might be a low-status one.

The Hunt for Patients: Outreach Work The desire to increase the number of patients registered at the clinic (and particularly patients who are HIV positive) means that staff are increasingly asked to undertake outreach work in a variety of settings. This includes attending gay clubs to hand out free condoms and leaflets about the clinic; visiting schools to provide information about sexual health; attending family-planning centres to advertise the services provided by the Department of GUM; and attending freshers' fairs at universities and further education colleges to advertise the services provided by the clinic. Similarly, considerable time and effort is put into thinking about how to attract patients to the clinic who are HIV positive but do not, as yet, know their status. For instance, the business manager asked one of the gay male senior registrars whether or not he would be willing to set up a special clinic session for men into S&M sex. This suggestion was based on the idea that men into S&M sex would probably be having high-risk sex and if they could be attracted to a clinic catering for their specialist needs then this would be a promising way of identifying new cases of HIV infection. The doctor, incidentally,

was uncertain as to whether or not he should run the clinic. He felt that he was too well known on the S&M scene and that it was potentially awkward for him to see patients whom he might have met in very different contexts elsewhere. Other doctors were also concerned about the precedent that such a specialist clinic might set if this particular doctor ran the clinic, namely, that male, gay-identified doctors would feel that they could have greater claim over the gay patients and the 'het' doctors would be left with the 'het' patients. The 'het' doctors were particularly concerned by this – not because they particularly wanted to run a clinic for men into S&M sex (on the contrary, some of them could think of nothing worse) but because they wanted to have access to the gay, HIV-positive patients as their research output depended on them having access to sufficient numbers of HIV patients. To date, the clinic has not been established but it is an illustration of the thought that is given to attract new patients with undiagnosed HIV infection.

HIV at a Clinic for Sexually Transmitted Diseases

So far this paper has described the atmosphere and ethos of a clinic specializing in the treatment of sexually transmitted diseases. Wherever appropriate, attention has been drawn to the various ways in which the emergence of HIV in the 1980s has helped to create a culture of work that is unique to departments of genito-urinary medicine which have large numbers of patients registered with HIV and AIDS in Britain. This section adds to this contextual information with a view to suggesting the following: first, HIV has transformed the medical speciality of genito-urinary medicine in an extremely short period of time. Second, many of these changes have benefited the medical speciality of genito-urinary medicine but the benefits are not equally distributed. Indeed, there are several indications to suggest that the quality of care varies according to HIV status.

With respect to the first point, Allen and Hogg (1993) suggest that genito-urinary medicine was perceived to be a low-status, 'Cinderella' speciality by medics until the mid- to late 1980s. Many of my informants reiterated this point. In fact, one senior registrar expressed this view in the following way: 'The only branch of medicine that had a lower status was that which specialized in the psycho-social problems of geriatrics.'

This is no longer the case. The discovery of HIV in the 1980s and the concern that was subsequently expressed by governments and international agencies that HIV presented a major public health problem meant that substantial resources were channelled into the research, treatment, and diagnosis of this infection. With respect to the medical speciality of genito-urinary medicine this precipitated major and profound changes. Many clinics specializing in the treatment of sexually transmitted diseases were moved from old and dreary sites to modern ones and/or they were refurbished. Resources were also provided to create a large number of

new clinical and/or research posts including consultant positions in HIV medicine and research professorships in HIV medicine. Moreover, the roles and responsibilities of nursing staff and health advisers also changed and this has been usefully documented in the Monks Report (1988) as well as by Allen and Hogg (1993).

However, there is a growing body of evidence to suggest that patients who are HIV positive and/or suffer from AIDS receive a quality of care that is superior to that given to patients diagnosed with non-fatal, sexually transmitted infections. The differential care (and by implication the differential status) is reflected in the use of space within the clinic as well as the types of relationship staff establish with patients. The following descriptions of the walk-in GUM clinic and the HIV clinic at The Radcliffe Centre illustrates this point further.

The Walk-in GUM Clinic

The walk-in GUM clinic is on the ground floor. It is a rather depressing place and resembles a social security benefit office. There are very few consulting rooms with natural light or windows; the air-conditioning does not work properly and space is in short supply. The health advisers' room, for instance, is about twelve feet square. It has three desks crammed into it and several large filing cabinets. There are no windows and the room is used by up to six health advisers at any one time.

Patients attending this clinic walk into a large waiting area lined with hard black plastic chairs. They take a ticket from a large paper roll attached to one of the walls and when the number on their ticket flashes up on a red digital screen they walk to the far end of the room to see one of the receptionists. Here they take a seat and the receptionist records basic social and economic information about them (such as their name, age, nationality, and current address). It is possible for three people to be registered simultaneously by three different receptionists as hessian-covered partitions ensure that patients cannot be fully seen or heard by other patients registering at the same time.

The waiting area promotes a formal and anonymous atmosphere. The receptionists wear a uniform and a distance of about three feet separates patients from the receptionists at the registration desk. They also ensure that all patients are provided with a clinic number and, in common with other staff at the walk-in clinic, never refer to a patient by name in public space.

Consultation times with doctors are scheduled to run for ten to fifteen minutes and many staff described the service provided at this walk-in clinic as a 'conveyor-belt system'. A polite and distant atmosphere characterizes the majority of interactions between staff and patients. The following account of a patient's consultation with a doctor helps to capture the routine and, at times, de-personalized nature of

consultations at the walk-in GUM clinic.⁵ The consultation took place in a windowless, box-like room in March 1994. The patient was about 24 years old, white, English, and male. He was called into the consulting room by a white Irish female doctor in her 30s who opened the conversation in the following way:

Dr: I'm Dr Horton and this is Dr Parker who is one of our research staff. She would like to sit in so that she can get the hang of things around here. Now, what can I do for you?

Pr: I'd like to have a blood test . . . well, actually I'd like to know if I have AIDS. *(He then went on to say that he knew he was a bit of a hypochondriac but he had recently lost weight and was sweating a bit.)*

Dr: I have to take a full sexual history so that I can see if you are at a high or low risk for HIV . . . Do you have a regular partner?

Pr: Yes.

Dr: Girlfriend or boyfriend?

Pr: Girlfriend.

(The doctor wrote this down and without looking up asked:)

Dr: Roughly how many partners have you had in your lifetime?

(The patient hesitated, so the doctor continued:)

Dr: Tens? Hundreds? Thousands?

Pr: Hundreds.

Dr: Low or high hundreds?

Pr: . . . um, low.

Dr: You mean one hundred? Two hundred? Three hundred?

Pr: About a hundred . . . um, more maybe.

(The doctor made a note of this while asking in a routine manner:)

Dr: Have you ever had sex with a prostitute?

Pr: I've never had intercourse with a prostitute but I've had oral sex with one.

Dr: Have you ever had sex with anyone from South America, North America, Sub-Saharan Africa, or South-East Asia?

Pr: No . . . Oh, actually, I was in America last year and some of my partners were American.

Dr: Have you ever taken drugs intravenously?

Pr: Yes, I've had my wisdom tooth out.

(The doctor tried to muffle her laughter and then explained that she meant injecting drugs. The patient responded that he took drugs such as grass, ecstasy, and cocaine and then anxiously told her:)

Pr: You'll see it in my blood.

Dr: *(laughing)* We won't see it as we won't be looking for it. Now, I'd like you to drop your trousers and your pants to your knees.

(Inspecting a mark on his penis, she said:)

Dr: You have a wart.

5. It is based on notes that were taken about five minutes after the consultation ended.

Pt: Does that mean I'm more likely to have HIV?

Dr: It's much easier to get warts than to get HIV.

(The doctor then explained that he would be referred for treatment to the upstairs clinic where he would have it frozen off.)

Pt: Frozen off! (with panic in his voice) Isn't there any other way of getting rid of it?

Dr: You could have it cut out!

At this point the patient agreed that it was preferable to have it frozen off and he departed in the knowledge that he would now be referred to the upstairs clinic as well as to a health adviser to discuss the full implications of having an HIV test.

The rapid fire of questions and the speed with which patients such as the one mentioned above pass through the walk-in GUM clinic is viewed by some staff as a desirable state of affairs. Others felt that a 'crap service' was provided. Either way, it could not be more different from the HIV clinic.

The HIV Clinic

The HIV clinic has a very different atmosphere to that of the walk-in GUM clinic. It is on the first floor and the reception area is bright, airy, and spacious. It is carpeted and a large number of comfortable chairs (with arm rests) are provided for patients registered at this clinic. In contrast to the walk-in GUM clinic, a warm, friendly, and informal atmosphere is promoted. The receptionists do not wear a uniform and they are on first-name terms with the majority of patients. In fact, many patients kiss the receptionists hello on arrival and it is not uncommon for nurses to hug and comfort patients if they are distressed. The clinic sessions with doctors are also run in a friendly and informal manner. Patients are able to have longer consultations of approximately thirty minutes and, if problems arise that are beyond the remit of the senior registrar or consultant, they can be referred to a number of other professionals providing in-house services such as acupuncture and nutritional advice. Advice is also available from a welfare benefits officer and, where necessary, patients can be referred for free psychiatric and psycho-therapeutic care.

Other indications of the friendly and informal atmosphere promoted by staff working at the HIV clinic include the fact that patients often refer to the waiting area as a 'good cruising joint' and 'a nice place to hang out'. In addition, the clinic organizes Christmas parties for patients attending the HIV clinic. These parties are usually held at gay venues and patients are encouraged to bring their friends and lovers. These aspects of care and attention, in combination with the regularity and duration of consultation times, often means that a rapport develops between staff and patients. Indeed, it is not unusual for patients to leave staff gifts in their

wills in gratitude for the care they have received and the staff, in turn, sometimes attend their patients' funerals.

In sum, the physical environments of the walk-in GUM clinic and the HIV clinic are very different from each other. The resources available for patient care and the type of relationship that tends to develop between staff and patients also differs and there is compelling evidence to suggest that the quality of care varies according to HIV status.

Conclusion

This chapter has focused on a clinic specializing in the treatment of sexually transmitted infections. It has tried to convey the atmosphere and ethos of the clinic by focusing on four themes central to the organization and running of the clinic: sex and sexuality; machismo and the intolerance of sensitivity; the myths and realities of providing a non-judgemental and confidential service; and the increasing tendency to view the clinic as a market place. I have suggested that the identification of HIV in the 1980s and the geographical concentration of HIV infections and AIDS cases in this part of Britain have influenced every aspect of life in the clinic. A very specific culture of work has been created which, in all probability, is also found in other GUM clinics with a large number of patients registered with HIV and AIDS. I have also suggested that, within the clinic, the quality of care varies according to HIV status; and that patients with non-fatal sexually transmitted infections are sometimes treated in a hostile and dismissive manner which contrasts markedly with the high-quality care offered to patients suffering from HIV and AIDS.

References

- Allen, I. and Hogg, D. (1993), *Work Roles and Responsibilities in Genitourinary Medicine Clinics*, London: Policy Studies Institute.
- Donegan, E. van (1998), "'I wish a happy end": Hope in the Lives of Chronic Schizophrenic Patients', *Anthropology and Medicine*, 5: 169-92.
- Kayser-Jones, J.S. (1990), *Old, Alone, and Neglected: Care of the Aged in the United States and Scotland*, Berkeley: University of California Press.
- Krause, I.-B. (1998), *Therapy across Culture*, London: Sage.
- Modell, J. (1989), 'Last Chance Babies: Interpretation of Parenthood in an In Vitro Fertilization Programme', *Medical Anthropology Quarterly*, 3: 124-38.
- Monks Report (1988), *Report of the Working Group to Examine Workloads in Genito Urinary Medicine Clinics*, London: Department of Health.
- Parker, M. (1999), 'HIV Transmission in Urban Environments: London and Beyond', in L. Schell and S. Ulijaszek (eds), *Human Biology in Industrialized Countries*, Cambridge: Cambridge University Press.

Family, Health, and Welfare

- , Ward, H., and Day, S. (1998), 'Sexual Networks and the Transmission of HIV in London', *Journal of Biosocial Science*, 30: 63–83.
- Rapp, R. (1988), 'Chromosomes and Communication: The Discourse of Genetic Counselling', *Medical Anthropology Quarterly*, 2: 143–57.
- Sinclair, S. (1997), *Making Doctors: An Institutional Apprenticeship*, Oxford: Berg.

Part IV
Development and Politics

Social Research in Rural Development Projects

David Mosse

Introduction

In this chapter I will explore some issues and dilemmas involved in undertaking ethnographic research within rural development organizations. These reflections derive from experience working as an anthropologist within rural development in different organizations in India. My particular interest, however, is not in the conventional anthropological problem of how to produce an ethnographic description of another people or place, nor even in doing an organizational ethnography, but rather in the way in which social research is undertaken by actors in and for development organizations – both ‘aid workers’ and those the aid organizations are attempting to help – and in the way that research is used by the actors themselves in support of their own ends. The research methods discussed are therefore, to different degrees, intended to be participatory.

After explaining something of the context of my own interest in social research in development projects, I will introduce some recently popularized participatory research methods. The use of these methods presents some interesting problems which I will explore before looking at alternative approaches to information production in rural development. A number of these are closer to the way in which anthropologists normally work. Finally, I will return to some of the problems and dilemmas of attempting social research *within* organizations, and in particular the difficulty of balancing the demands of engagement in development agencies with critical analysis of their institutional processes.

Anthropology in Development Organizations

A development project, whether concerned with agricultural improvement, rural water supply, participatory irrigation, or any other activity is a particularly complex form of organization. A project is not a bounded entity formed around consensual goals and ideas (even though it may represent itself as such), but a ‘political’ system in which different perspectives contend for influence and authority. It involves multiple actors and stakeholders; not only project staff and participating villagers,

but also donors, consultants, bureaucrats, senior agency managers, and local government representatives, among others. Within each of these there are distinct interests, concerns, and priorities. These may be defined by gender, age, and position (field workers, project management, senior agency executives), or competing development agendas (e.g. gender equity vs. increasing production; poverty reduction vs. environmental protection).

Given these different views and the tensions between them, a project can be seen as a negotiated, but frequently unstable, operating consensus among the different players and perspectives, including those of 'villagers', farmers, or consumers – or in planning jargon, the 'primary stakeholders'. Unitary and official representations of projects often mask this complexity. A development project also involves a whole series of interfaces: farmers/project staff, fieldworkers/supervisors, staff/management, consultants/team, government/agency, among others. Each of these is a site for competing meanings. There is, then, no single perspective from which to undertake an ethnography of a development programme, or even to describe what is happening.

My own perspective comes from engagement as a development actor (manager, trainer, and consultant as well as researcher) within different types of organization and of project in India, including an international NGO, a bilateral aid agency, and various local NGOs. I have had various tasks, including the development of project design, implementation strategies, the assessment of impact, the development of policy, or the management of an office. In all of these I have been among those seeking to advance certain interpretations and meanings, specific readings of events and priorities for action, over others. Indeed, development practice involves constant negotiation, advocacy, and the strategic deployment of information as much as its production through research.

More particularly, the comments offered here derive from my experience as an anthropologist working in two different settings. The first is as part of a multi-disciplinary team supporting an agriculture project in the Bhil tribal region of western India, i.e. in the adjacent districts of Rajasthan, Gujarat, and Madhya Pradesh. The second setting is research on NGO- and state-managed irrigation development in Tamil Nadu.

My ethnographic perspective has depended considerably on my relationship to the organizations concerned. In the western India agriculture project, I have been a project 'insider'. This is an ODA/DFID-funded project with the stated objective of involving male and female members of Bhil tribal farming households in the improvement of livelihoods through work on crops, agro-forestry, small-scale irrigation, soil and water conservation, and savings and credit, and addressing the constraints of deforestation, deficit agriculture, rural indebtedness, and seasonal migration (see Jones *et al.* 1993; Mosse 1994, 1996; Mosse *et al.* 1997). I was directly involved in the design and negotiation of this project in 1990–1991. I also supported the recruitment and training of staff and the design of approaches to

participatory farming-systems development from 1993, and the early formulation of village plans. Subsequently my role has been in review, monitoring, impact assessment, and ancillary social research (e.g. on livelihood strategies, tribal debt, and seasonal labour migration).

In Tamil Nadu, my relationship to irrigation development organizations has been that of a more conventional researcher, in this instance pursuing ethnographic and historical work on indigenous tank-irrigation systems. Here I have been an 'outsider' researcher, with restricted access to the internal practices of the agencies involved, whether of government, universities, or NGOs.

It is probably true to say that most of my insights into organizational processes have come from the former 'insider' role rather than the latter 'outsider' role. In other words, as an anthropologist I have learned about organizations by working within them (as adviser, consultant, or manager). This means being a reflective participant – or perhaps engaging in 'observant participation' rather than 'participant observation'.

Even within a single organization I have found my role and perspective changing significantly. This has certainly been the case with my work over seven years with the tribal farming project. Initially I was deeply involved in the mechanics of *doing*: designing, recruiting, training, supervising, etc.; once the project was established, my role began to change. I became a critical observer of actions and effects, encouraging review and reflection, acting as an advocate for field-level perspectives, providing feedback to management and donor, and influencing changes in approach as the project learned from experience. This was a task shot through with tensions and the need to negotiate or bargain for desired changes, often through forming tactical alliances with my technical colleagues.

More recently, and in part through reflective writing, these two modes – participation in, and critical reflection on, project processes – have become more separate. Indeed I have become more conscious of the two discourses and practices of development and anthropology respectively as distinct. But I do not cease to move between them, and find this journeying fruitful. Like ethnography in other cultures and places, insights come from translation, from the juxtaposition of different perspectives on the same phenomena – in this case that of development actor and critical ethnographer – and a heightened consciousness of the processes that structure our own thought. In other words, through developing a critical separateness from the project, it is possible to view my own accounts and actions as a product of the project discourse.

Let me illustrate some contrasting perspectives. In forging a new participatory and poverty-focused *agriculture* project in the early 1990s it was necessary to link activities which promised to guarantee benefits for the poorest and for women to widely endorsed views of the nature of the problem – namely low production, land fragmentation, deforestation and soil erosion, and the isolation of tribal villages

from services of the state (e.g. education, health, agricultural extension, etc.). The focus of my work was on countering top-down, subsidy-driven programmes with community-based planning and action. There was a constant need to identify opportunities for farmer involvement (especially by women), to emphasize low-cost, low-subsidy interventions, and to focus on the needs and interests of the poorest, in order to ensure equity in access to benefits. In order to influence project design and strategy, it was necessary to be acquainted with local people's assumptions about the role of natural resources in livelihoods (e.g. the importance of farming, forestry, or horticulture as against artisanal activity or migrant labour), as well as knowing about the capacity of local communities to manage resources sustainably, and the need for participatory action.

But this operational perspective is one of several; it is also one which, from a different point of view, can be challenged. The poverty of tribal villagers need not be seen in terms of low production arising from pressure on land and tree resources or isolation from government services, but rather as the result of historical and political processes. Included here would be the effects of the registration of land for revenue purposes under British rule which established land as private property, and – together with stricter administration of forest land – ended an earlier tribal practice of allocating usufruct rights for unoccupied land by mutual agreement. A more sedentary form of agriculture, and the need to pay cash land tax, brought a range of intermediaries and middlemen, shopkeepers, and moneylenders into these remote areas. Today, tribal farmers are often involved in seasonal migration to urban construction sites which ties them into on-going relations of debt to moneylender/labour contractors. Rather than seeing migration as a consequence of pressure on resources, it is possible to view environmental degradation as the result of a historically specific set of political relationships which have ensured that tribal communities subsist in marginal regions. Today, labour migration is not a problem in itself. It is a central part of tribal livelihoods. What is problematic is the relationships through which farmers access informal urban labour markets, the linking of migration to debt, and the small proportion of the value of their labour which poorer migrants are able to retain. Such an analysis implies a different set of problems, a different type of strategic action, and work with different agencies (e.g. government labour offices, NGOs working in urban sites, trade unions, employers' organizations, legal aid organizations).

There is no one true perspective. Subsidies, for example, do indeed undermine local initiative, and do damage the long-term maintenance of soil and water conservation or forestry plantings, and so need to be minimized. But, from the other perspective, subsidies are to be maximized for their part in protecting a livelihood system which depends upon short-term seasonal wage labour. Viewpoints are institutionally grounded and adopted tactically in the pursuit of given goals. In fact, the role of an anthropologist in development organizations is often to combine

different perspectives, and so to challenge the tendency of closure within a given analysis or narrative. The result of such efforts in this western Indian project have been, for example, to refocus attention towards debt and migration, and to break up monolithic ideas of 'community'.

Ultimately, then, the ethnography of a development institution is an ethnography of the structuring of the thought and practice of its actors (including myself); it involves direct acknowledgement of the socially mediated nature of my own knowledge and practices. But this reflexive practice has to take place within the flow of events and commitments. An active engagement in meetings, discussions, debates, arguments, or conflicts potentially involves a parallel process of self-observation, note-taking, and writing. But this is unmediated by an explicit research agenda. Indeed, it is in practice very difficult to do two things at the same time: actively to push forward an agenda, and simultaneously to reflect critically on this.

Towards the end of this chapter, I will return to the question of the tensions involved in taking (or articulating) an anthropological perspective within an organization. But these comments are really by way of personal background. What I want to focus on next are some more specific approaches or methods of social research and information production employed by organizations involved in rural development.

Social Research *by* Actors and *for* Organizations

Ethnographic research methods have been adapted for and used within, or more particularly *by*, development organizations to serve their own agendas. In these situations the role of the anthropologist changes. Rather than producing data and delivering an analysis, the researchers facilitate actors in documenting and reflecting on their own practices, whether these are the institutional practices of development workers or the agricultural practices of villagers. The aim of research is to encourage innovation and improve learning. This involves significant departures from academic modes of research.

In this section, then, I will refer to methods which have evolved largely within development rather than in academic settings. There are two clusters of methods here, which are captured under two labels: 'Participatory Learning' (also known as Participatory Rural Appraisal or PRA); and 'Process Documentation Research', or 'Process Monitoring'. These are two rather different approaches to social research in development which, for convenience, I will refer to as 'participatory' and 'process' research respectively. The first is rather better known than the second, but both potentially shift ethnographic practice in new directions. Six related points need to be made about them.

1. These research methods in different ways stress the *direct involvement of actors* (project staff, participants, or 'beneficiaries') in the production and analysis of data.

2. To different degrees, they involve *continuous information production* over a period of programme work rather than a 'snap-shot'. The focus is not on the development 'output', but on the dynamics of development processes, different perceptions of relationships, transactions, decision-making, or conflicts and their resolution in a context of change.

3. There is often an *orientation to the present*, 'an intimate relationship with what is happening right now' (Gilbert, personal communication, cited in Mosse 1998a: 10). In project-cycle terms, process research methods focus on programme *implementation* and depart from the dominance of planning or evaluation discourses in development.

Relatedly, PRA and 'process research' are explicitly *inductive* rather than deductive in orientation; that is to say, they have an open-ended concern with project contexts and happenings rather than the modelling of expected change from known inputs. While close to ethnographic common sense, this is a significant departure from those many project information systems which are structured around the monitoring of predetermined indicators. In other words the concern is with the complexity of the social life of a project rather than focusing attention on the question, 'Has input *x* brought about change *y*?'

4. 'Participatory' and 'process' research methods both have a strong *action-orientation*, which means that their outputs are, in the first instance, directed towards participants who are in a position to react to them whether 'beneficiaries' or programme managers. The feedback is often rapid, rather as the strategic adjustments during a football match. But the significance of learning by doing is not only in the immediate utility of information. There is also a methodological premise that intervention and change make visible certain structures underlying social systems which are otherwise invisible. The axiom is: '... if you want to know reality you must try to change it' (Volken *et al.* 1982, quoted in Uphoff 1992: 275). In other words, action-orientation makes for a better interpretative social science of practice. However, it should also be pointed out that strongly action-oriented research can also narrow the frame of reference, as indeed can planning-oriented PRA. This is one point at which (as I will explain below) 'participatory' and 'process' research diverge. The intention of the latter is not appraisal for planning, but rather to focus on events, relationships, and diverse impacts on the particular or improbable individual person, idea, or event, as well as more regular influences of contexts, roles, interests, and constraints.

5. While engaging institutional actors, research (especially process research) is situated outside of normal structures and the routine flow of programme activities and monitoring information. Indeed it is intended to by-pass the usual filtering and packaging involved in hierarchical organizations. (I will comment later on the degree to which this is possible.)

6. Finally, both participatory and 'process research' methodologies give recognition to the *inter-subjective* nature of the research process and (at least potentially) treat the perspectives and judgements of researchers, monitors, or participants as data in their own right.

Several of these developments in 'applied' social research correspond to contemporary 'post-positivist' trends in anthropology, addressing issues of both representation and relevance. There is no place for invisible, omnipresent, and unquestioningly authoritative anthropologist-narrators.

There are also several reasons why development organizations themselves are interested in more detailed knowledge of their own processes, and are increasingly willing to make use of participatory and semi-ethnographic methods. Briefly, various turns in development policy and practice make for new demands for information and qualitative analysis (cf. Mosse 1998a). In a complex series of changes in development discourse, five trends stand out. First, there has been a shift away from narrow technology-led projects towards the more complex objectives of social and institutional change. Secondly, these involve more complex management situations. Instead of the expatriate-managed technology-transfer project, international development today more commonly involves inter-organization collaboration and local networks. Thirdly, there has been a shift away from simple input-output project models and greater acknowledgement of the unpredictable and complex nature of 'development'-induced social change. Fourthly, awareness of the limits of 'blue-print' planning has led to a shift towards more flexible and iterative (repeatedly reassessed) planning approaches. Fifthly, there is now widespread support for participatory 'bottom-up' approaches to development.

These complex interrelated shifts cannot be discussed further here. It is enough to know, first, that there is growing interest in the use of qualitative and ethnographic methods within development contexts, and second, that at least some of these are employed reflexively to understand better the workings of development organizations themselves.

Let me now turn to the research methods themselves. First, I will provide a very brief introduction to currently popular participatory research methods. Then I will consider the application of these to social research in development organizations. This will raise certain critical issues which serve to introduce, 'process documentation research' as an alternative approach to reflexive organizational practice.

Participatory Learning and Action as used in Development

Participatory Rural Appraisal (PRA) has been described by one of its leading proponents, Robert Chambers, as a 'growing family of approaches and methods

to enable local people to share, enhance and analyse their knowledge of life and conditions . . .' (Chambers 1997: 102). PRA methods have principally been developed for generating information for planning with rural communities. However, they can (with adaptations) be used as a means by which any group of people analyse their situation, including members of an organization.

PRA is based on certain key assumptions or principles. The first principle is that social actors are themselves the 'experts' in understanding complex inter-linkages involved in their life-situations and therefore should be involved not only in generating information (for outsider analysis and presentation) but in actually doing the analysis (using local categories). A second principle is that research techniques can be adapted for different situations and especially for the recording and representation of information by non-literate men and women. Outsider researchers need only have a limited, facilitating role. A third principle is the visual sharing of information often in the form of maps, models, or diagrams and the generation of information which is public, correctable, manipulable, and owned/verified by participants themselves. Information is retained by participants or circulated close to its point of generation through participatory record-keeping, monitoring, etc. Fourthly, information is regularly cross-checked by using more than one method to verify accounts (the principle of triangulation). A key assumption (fifthly) is that participatory research is an empowering process involving, as it does, role-reversals between the researchers and the researched. In short, PRA, as Chambers puts it involves 'rapid, progressive learning, is iterative, flexible and exploratory'.¹

PRA Methods Themselves

So, what sort of methods are involved? As Chambers and others are quick to point out, there is no fixed set of PRA methods. Methods are rapidly evolving and innovations are constantly being made. Core methods commonly used include semi-structured and group interviewing; the recording of local histories or 'timelines'; participatory mapping and modelling (e.g. of settlements, agricultural landscapes); seasonality diagramming; and various techniques for estimating, ranking, and comparing (including 'wealth ranking', a technique to establish local criteria of social differentiation). PRA methods emphasize the use of local materials (e.g. stones or fruits as counters). The context of PRA varies enormously depending on the purpose of the exercise but often involves researchers working together with local residents through a structured set of activities, the results of which are used as a basis for planning development activities. The outputs of PRA exercises

1. Taken from loose sheets handed out during a PRA workshop, Seeganhalli village, Karnataka, 1989.

are important, but often the discussion and debate that surround the exercise are particularly informative.²

None of the research techniques used in PRA are, in themselves, fundamentally new. Most have been developed and used in other contexts first. Visual representation and informal mapping and diagramming, for example, were developed as methods for agro-systems analysis at the University of Khon Kaen in Thailand (Khon Kaen University 1987; cf. Chambers 1997: 112). The involvement of local people as researchers and experimenters and the exploration of the complexity and diversity of local farming systems was a part of farming-systems research in the 1970s and 1980s. More radical 'participation' streams within PRA derive from traditions of action-reflection research among activist NGOs in Latin America and South Asia (notably the influence of Paulo Freire's ideas of 'conscientization'). More directly influential were the approaches of the more extractive precursor to PRA, namely RRA or 'rapid rural appraisal' (which made use of group interviews, key indicators, group walks, aerial photography, chronologies of local events, portraits, and case studies) and the idea of 'indigenous technical knowledge' which was popular among development agencies in the mid-1980s. Finally, the qualitative elements of PRA – work in indigenous categories and the ideal of local residence – derive from the older tradition of anthropological participant observation (Chambers 1997).

PRA and Research in Organizational Settings: Practice and Problems

Let me illustrate with an example from Western India the kind of development context within which PRA is used. The Kribhco Indo-British Rainfed Farming Project (KRIBP) (begun in 1992 and still ongoing) is a participatory farming-systems development project situated in the Bhil tribal region of Gujarat, Madhya Pradesh, and Rajasthan. The project aims to improve the livelihoods of poor farming families by actively involving them in generating location-specific natural-resources development plans (focusing, *inter alia*, on crop improvement, soil and water conservation, agro-forestry, minor irrigation). In the early stages of this 'participatory planning', villagers and project staff jointly used PRA techniques to identify

2. There is now a large literature on different PRA methods and readers interested in details of technique or records of the experience of use of PRA are referred to the *Participatory Learning and Action Notes (PLA Notes)*, published by the International Institute of Environment and Development (London), and to the PRA/PLA information packs produced at the Institute of Development Studies, Sussex University (Brighton). There is also a critical literature which has provided a challenge to some of the claims made for PRA, or examined its practice in a little more detail. (For critical accounts see Mosse 1994, 1996; Pottier 1997; PLA Notes 1995.)

livelihood problems and workable solutions, and to negotiate project interventions. Typically, initial PRAs are rapid-research events. Small teams of project staff stay in villages for four or five days guiding villagers through a structured set of group exercises and interviews. For example, villagers develop maps or models of their neighbourhood and landscape on the ground, identifying attributes of their social and physical environment. These may be accompanied by 'transects', discussions on the constraints and opportunities in relation to natural resources while walking in different directions from a high point in the landscape. Maps and 'transect' diagrams are used, in a preliminary way, to identify problems (such as areas of soil erosion or tree loss) or opportunities for resource protection. Techniques of seasonal diagramming are used to indicate periods of shortage, cycles of debt and seasonal migration, variable availability of food or work, or the seasonal patterns of ill-health. Visual representations of trends enable discussion of changes over time in diet, yields, prices, or types of wage labour. Such discussion of trends links to the representation of local histories as 'timelines'. Techniques of matrix ranking are used to explain (to outsiders) the different types and qualities of tree or grass species, and to express preferences, and a rather specialized form of ranking based on card-sorting is used to identify local criteria of wealth and well-being. This 'wealth ranking' provides a basis for provisionally grouping households into broad socio-economic categories (e.g. 'deficit', 'self-sufficient', and 'surplus' households).

Since, as I have argued elsewhere (Mosse 1994), women face constraints in participating in PRA 'research', it proves important to work with them separately in smaller groups, often in domestic settings. Bhil women faced practical constraints to participation in PRAs in terms of the demands on their time which restricted their ability to be available collectively, at central locations, for continuous periods of time. Women (depending upon age and status) were also constrained socially in their ability to participate in activities in public spaces in the presence of outsiders. It was also rare that women were able to articulate needs or concerns which deviated from socially accepted gender roles. Finally, some of the PRA techniques themselves may have excluded women's perspectives. In early KRIBP PRAs 'the representation of knowledge and experience in maps, tables, charts and so forth involved a formality which appeared to mark it out as the province of men' (Mosse 1994: 513).

In the KRIBP project, therefore, initial general PRAs were followed by work with smaller groups of women. There were also focused PRAs dealing with specific issues (e.g. the availability of livestock, health, or fodder) which contributed to the formulation and budgeting of a programme of action.

While PRA has commonly been used by development workers in order to facilitate community-level problem analysis and planning, it has also been used for participatory research within organizations. Indeed, some techniques such as the diagramming of institutional linkages are intended to enable members of

organizations (as much as 'villagers') to represent and analyse the social and work relationships in which they are engaged, spheres of influence, social proximity, distance, hierarchy, competition, and so forth. In training exercises it is common for students of these techniques to model their own institutions and their place within them.

In the teaching session on which this chapter is based, for example, a heterogeneous student group including BA, MSc, and MPhil students from the Human Sciences Department at Brunel University used linkage and Venn diagram methods to explore different perspectives on student-department relationships. The simplest form of this exercise involves representing individuals/institutions by different-sized or -coloured circles and placing them (on the ground or on a sheet of paper) at different distances to determine importance (size) and social distance (relative position). Since the intention is often to establish significant differences of perspective, the group divided themselves up by type of degree. (Other distinctions such as age or gender could also have been used.) The different small groups did, as expected, produce very different representations of the structure and relationships of the Department and their place within it. These different representations then provided the basis for discussion both of the research process and of the organizational dynamics of the Department.

Whether carried out in Indian tribal villages or among UK students, participatory research takes place in a particular social context and produces a distinctive type of information. By virtue of being participatory, these research exercises are also unusually visible: individuals work with a large group of their peers, in the presence of persons in authority (teachers, heads of department, or village leaders) and in front of outsiders (who in development settings are often resource-bearing outsiders to whom the right image has to be conveyed). In this sense participatory research is often a very *public* type of activity. The public nature of the research has a strong bearing on who participates, what is said (or drawn), and what is not said (or not drawn). (At Brunel the output of one student group was influenced by the presence of staff among them.) There may be unspoken ground rules for safe discussion, or official models of organizational behaviour from which it is difficult to depart (cf. Pottier and Orone 1995).

More generally, the way in which a group (villagers or students) represent their situation (its charted output) is a product of its own social dynamics. These dynamics are themselves produced by existing social relationships. In the case of KRIBP, the exclusion of women was only one of several ways in which PRAs came to be orchestrated locally in such a way as to exclude divergent opinions and the interests of non-dominant groups, whether these were factions, minor lineages or clans, or distant hamlets. Public expressions of community interest were capable of concealing private interests. A striking example was where – as it became apparent in retrospect – a community desire for education (expressed though the PRA event)

disguised the individual interest of the village headman who wanted a hand-pump near his house. The need for education meant a school, schools came with hand-pumps, and the placing of both could be influenced locally (Mosse 1994).

With the advantage of hindsight, it is clear that, in almost all cases, initial project PRAs took place under the control of key villagers and their supporters. PRAs took place on their land or by their houses, or in public spaces or social contexts over which they held sway. These were often the same 'brokers' through whom the project had gained entry into villages, and whose interests (and those of their client groups) featured prominently in the early record of community needs and priorities. I remember being impressed in 1992 by the way in which, in one village, KRIBP fieldworkers had incorporated *bhajans* (informal devotional singing sessions) into the PRA event in order to place it within a locally understood informal context. Later it became clear that the *bhajans* had a more important social effect. It served to mark the proceedings of the PRA (and subsequent project activity) as the province of members of a *bhajan mandal* (devotional group) in the village, a group which was dominated by older men and which represented a clan-based social group (headed by the leader of one village faction) who, identifying themselves as *bhagat Bhils*, claimed a measure of separation and status superiority over other villagers. The link between PRA and *bhajans* only served to underline and exclusion of other interests in the village.

If representation of livelihood needs are shaped by local power relations they are, in an even more profound way, shaped by the assumptions, preconceptions, and interests of the development agencies and projects involved which promote them. The way in which KRIBP objectives came to be articulated through PRA has been discussed elsewhere (Mosse 1996). An important question is, how effective are PRA techniques at elucidating the very social/power relationships which structure their own outputs?

In general, participatory research has been more successful in some areas than others. In the KRIBP project in Western India, PRAs often proved very effective at generating agro-ecological information related to farming systems and watershed development. However, they were far less useful in generating an analysis of social relations: village-level patterns of dominance or dependence, credit relations, factions, conflicts, or spheres of influence. Such social knowledge was indispensable to fieldworkers who had to integrate activities aimed at enhancing production (irrigation, soil and water conservation, agro-forestry, etc.) with principles of gender equity and long-term sustainability. Indeed, a basic grasp of the social dynamics of a community – as many anthropological fieldworkers quickly realize – is necessary in order to reside and work effectively. Project fieldworkers, in their notes and diaries, often recorded their shifting perceptions of the villages in which they worked, sometimes synthesizing into diagrams their analysis of sub-groups, alliances, leadership, and influence. While these had a superficial resemblance to

the linkage diagrams of PRA, they could never, in fact, have been generated *in the course of* a participatory research-event.

The strong desire of communities or organizations to represent themselves as harmonious, well integrated, or functional, means that public PRAs involve a 'micro-politics' of consensus which obscures rather than reveals local social relations. This presents a major constraint to analysis of the social relations of villages, projects, or institutions more generally through PRA-type research methods. Indeed, in work on the West India tribal project, it was through the *participant observation* of PRA events and their contexts (and *not* the direct application of PRA techniques) that elements of local power relations, and the relationship between villagers and project staff, came to be understood (as in the case of the *bhajan* group mentioned above). The best material for analysis of relationships of the 'project setting' were not found in the consensual output of chart, map, or diagram but in the absences, the gaps, the corrections, the staff interpretations, the different receptions given to male and female workers, the rewards, disagreements, conflicts, and inconsistencies (Mosse 1998a). PRAs provide critical events in which to observe social relations. This sort of joint reflection on events is where 'process' approaches to research in development agencies come in.

Process Monitoring and Documentation Methods: Examples

Conscious of the biases contained with early PRAs, in the KRIBP project, together with a colleague, Mona Mehta, I began to facilitate structured and critical reviews of PRAs to improve their usefulness for planning. At the same time we began to encourage the field teams to recall, discuss, and review other project events, working through various significant happenings, creating a history of actions, and describing these from as many dimensions and points of view as possible. Patterns of participation, the identities of prominent actors, the rationale of decisions were all considered. The events selected for discussion could be anything from the visit to an agricultural research centre, to the planning and implementation of a programme, to the theft of saplings from a project tree nursery. In what loosely resembled 'situational analysis' (van Velsen 1967), the structured review of key events/interventions helped reveal local social networks, highlighting for the project staff the significance to outcomes of factors such as clan difference, religious difference, patronage, factional conflict, and leadership struggles. As far as the staff were concerned, reflection on action generated a kind of knowledge about social relationships which was concealed in PRA events. It also focused attention on the *processes* of intervention and the role of staff themselves.

Although every case was different, there were some common patterns in the effects of early project work which helped field staff in developing entry strategies in new villages. For example, a key component of success in villages had been the

development of strategies to avoid or neutralize disruption from village leadership, by identifying individuals or groups within a village having the right balance of *authority* and yet *independence from patronage*, as the starting point for programme development. In some cases, the KRIBP staff shifted away from the village leadership to more independent hamlets or clans; they found support from returned migrants; or they shifted from the older to the younger generation, finding alternative more supportive persons among sons or more often nephews of village leaders (Box 1).

Box 1: Influence and Opposition

In one Rajasthani village the headman or *tadvi*, through whom KRIBP had entered the village, began to oppose project activities, such as a crop loan scheme and the new *anganwadi* (child-care centre). He inhibited villager participation in the otherwise popular credit scheme and forbade his son from becoming a signatory of its new bank account. The crop loan scheme, unlike earlier community initiatives led by the *tadvi* (getting an electricity connection or bridge for the village), did not support his style of patronage and leadership. Indeed potentially it threatened his own money-lending (and land-mortgaging) activities. But, while the *tadvi* was able to control his son (and his daughter-in-law who had run the child-care centre before it collapsed) and indirectly to influence other villagers dependent upon him to withdraw from the programme, he was opposed by his brother's son, Lalji. This young man had the tacit support of his father, who was a man of stature in the community but was not prepared openly to come into conflict with his brother. Lalji led a group of younger men supportive of the project's interventions. Work at the hamlet level resulted in the formation of three kin-based neighbourhood groups for the purposes of crop loan repayment. These were headed by Lalji and other younger men (his cousins) normally excluded from prominence in village-level meetings. With the tacit support of older leaders, including their fathers, these young men successfully pressed ahead with the credit programme without the *tadvi* (Mosse *et al.* 1995).

As Appadurai (1989: 271–2) has pointed out, conventional interview-based research techniques (including PRA) usually attempt to capture the outcomes of events, the identifiable net outcomes of social processes: organization and leadership, structures, new linkages, input supply lines, community decisions, etc. However, many important data are manifest not in the outcome but in the quality of the transaction, in the relationships implied, and in the aspirations and expectations, as well as in the post facto outcomes (*ibid.*). The reviews of project activities and processes undertaken by the project focused on transactions, but unlike PRAs did not involve the

villagers concerned. In that sense they arose more from participant observation than from participatory research. Despite their fruitfulness in understanding the dynamics of project-community interfaces, the monitoring of development events in the project never continued beyond the initial few facilitated sessions largely because they generated a type of information not easily assimilated to institutional interests of the project as a whole (see p. 175–6).

Elsewhere, however, the observation and recording of events by project staff or outsiders has been more systematically undertaken, in a method which has come to be known as Process Documentation Research.

Process Documentation Research

This term was first coined at a workshop in the Philippines in 1978 as a label for research into field-level implementation of a programme to improve communal irrigation by developing farmer institutions for irrigation management, which was part of the broader objective to transform the National Irrigation Authority from a government bureaucracy to an autonomous agency self-financed through water charges (Veneración 1989). Full-time social scientists took up residence in selected villages along with project staff. They attended all project activities and meetings, interviewed staff, analysed records and began detailed observation and documentation of the workings of the programme and the processes of 'water user group' formation and functioning. Detailed and daily diaries of events record the interactions of villagers and project staff and other agencies, the significance of wider processes – party political contests, leadership struggles, resistance from contractors, caste conflicts, etc. – to institutional development, the opinions, and attitudes and judgements of key project and non-project actors (notably absent from PRAs). Process documenters synthesized their observations into regular narrative reports, as well as contributing to manuals on field intervention methods (how to organize meetings, manage association funds, promote effective leadership, etc.).³

In the later 1980s and 1990s similar research has been initiated within programmes of 'participatory irrigation development' in Tamil Nadu and Gujarat in India and in Thailand, as well as in forestry programmes in the Philippines and Gujarat (e.g. Parthasarathy and Iyengar 1998). The precise combination of methods varies. Sometimes the 'process documentors' are members of the project team (Tamil Nadu), while at others they report separately to a research institute. In each case the primary objective is to enable the programmes as a whole to learn from implementation experience and in this light to modify strategy and policy (Mosse 1998b; Bagadion and Korten 1991). This involved programme decision-makers at project,

3. Documentation methods are not limited to monthly reports, but also include subject papers, working papers, newsletters. Networks and fora are important parts of some process-monitoring programmes.

national, and international (donor) levels coming together to form working groups to review information on processes. This represents a significant departure from the normal upward flow of information in development organizations.

But beyond immediate programme-implementation uses, process documentation (PD) reports allow for analysis which can challenge aid agencies' own narrow instrumental perspectives on development. The PD reports from the Tamil village of 'Nallaneri' between 1989 and 1994, for example, provided a rich record of events in the establishment of a local water-users' association (WUA) and the repair and improvement of a local tank-irrigation system under a European Commission and Ford Foundation funded programme. Among other things this record could be used to trace the shifting pattern of alliances and disputes around the new irrigation association, showing the importance of new institutions in the articulation and contestation of leadership and upper-caste honour.

The long-term process documentation showed that the significance of the water-users' association extended way beyond irrigation management and wetland agriculture attracting the attention of urban-based kin who had no interest in cultivation at all. In fact, for a long period the WUA proved far more capable of mobilizing funds for extensive litigation (over rights to leadership) than for tank repairs, demonstrating that this, like many such associations, was an institution of village politics as well as of water management. The process documentation also traced a parallel process in which the NGO- and state-backed Association provided a major focus for low-caste social mobility. Upper-caste Mudaliar and low-caste Harijan members of the village could be seen to evoke and manipulate development ideals of 'community' in pursuit of quite separate strategies of social change. Mudaliar leaders sought to retain some measure of status and dominance by building the new WUA on an old system of privileged 'shares' (*pankus*) in tank resources clearly indicative of their caste power. Harijans, on the other hand, perceiving that this intent behind the new 'public service' roles and institutions challenged their own restricted participation, withdrew their support and labour, or bargained for new privileges (including office-holding in the WUA and rights of access to tank resources) (Mosse 1997).

The Process Documentation Research employed in this case is, in fact, one of several efforts to use ethnographic methods *within* development organizations, including Laurence Salmen's approach of 'participant-observer evaluation' developed in 1982-4 for work in urban Latin America (Salmen 1987). Salmen used ethnographic methods (residence and participant observation in project areas for five months plus small, focused surveys) to identify different and contrasting perceptions of achievement between beneficiaries and project professionals in World Bank urban upgrading projects. 'Process research' also bears on the concerns of a body of actor-oriented sociological work which focuses on the 'interface' between communities and development agencies (e.g. Long and Long 1992) and which further develops the 'situational analysis' of the 1960s. Equally relevant

for the development of 'process research' is the renewed interest in the anthropology of organizations (Wright 1994). But what distinguishes Process Documentation Research is, first, the close and continuous recording of local programme activity and, second, its integration into development organizations and policy processes.

Process Documentation Research does, however, present a number of difficulties both as a device for influencing policy/project management and as a tool for social research. I will just comment on the latter (on the former, see Mosse 1998b). On the positive side, PDR has made policy-makers aware that development effects are socially complex and locally diverse. It has contributed understanding of matters such as leadership in villager organizations, the complexity of local-agency interfaces, the shifting bargaining power of different groups and genders, and the functioning of resource user groups as dynamic and evolving political institutions.

But there have also been problems. Principal among them is the ambivalent status of the 'process documentor'. As external agents, process researchers meet resistance from agency staff, while as insiders they present too close an identification with the purposes of the project and lack a critical independence from the project studied. A research strategy based on long-term (six to seven years) recording of events as they happen is anyway expensive, and, even more importantly, depends upon field researchers possessing exceptional skills. They are required to have intimate knowledge of the project, good rapport, documentation skills, critical independence, and sufficient local and subject knowledge to enable interpretation of events and the attribution of significance. These are rare skills. More often there is an overemphasis on note-taking and report production as against selective observation, discussion, and analysis. (Oral reporting might in many situations provide a more useful feedback.) In Geertz's terms (1973: 7), the lack of interpretation means a predominance of 'thin' over 'thick' description. The notion of 'documentation' itself gives a spurious sense of objective reporting. In fact 'process documentation' is anything but a 'clear window into the rich detail of uncensored experience' (Korten 1989: 14). Selection and interpretation is the more difficult to handle because it is not explicit. Certainly there is often a narrow focus on project activities which can miss relevant local events.

Institutional Ethnography

Recently, 'process research' (of a slightly different kind) has extended beyond individual projects and programmes and been used in more complex inter-agency settings to analyse how partnerships (an increasingly important part of development practice) actually work. Lewis (1998), for example, describes attempts to undertake an 'institutional ethnography' of donor-government-NGO collaborative links involved in Bangladesh fisheries development. This involved examining inter-agency relationships through, *inter alia*, tightly structured problem-solving workshops

followed by semi-structured interviews with participants, the use of tape and video to record discussions, and content analysis to mark shifting attitudes and experiences.

This institutional ethnography had two sorts of output. First, there were a number of practical effects: better adapted fisheries technology 'packages', improved inter-agency (NGO-GO) communication, and revitalized NGO networks. But second, and more critically, research began to question the whole 'partnership idea' by analysing the different motivations involved. As Lewis concludes, research indicated that the principal collaborating technical agencies 'need each other far more for the individual institutional survival of each agency than the average low-income farm household in Bangladesh needs new technology for aquaculture' (Lewis 1998: 104-5). As elsewhere, the ethnography was able to suggest how institutional needs perpetuated misperceptions of development problems (in this case portraying technology constraints rather than socio-economic issues of resources access and tenure as the principal bottlenecks of fisheries development).

In drawing attention to problems, distortions, and institutional interests underlying the discourse on 'partnership', the ethnography hit sensitivities which rapidly undermined its legitimacy. The research project was resisted and eventually closed prematurely.

Power and Information

The experience of tension in undertaking independent ethnographic analysis of development organizations is commonly reported, especially where research draws attention to the weakness of prevailing models, and points to contradictions or the gap between intention and action. But these tensions are unsurprising. The difference between ordinary ethnographic work and the sort of client-oriented ethnography I have been describing is that the latter is research which takes place *within* and *for* organizations. As such it has to contend with the fact that organizations – and especially development organizations which exist in a nexus of information, evaluation, and external funding – are, among other things, systems for the production and control of information. Development organizations have highly evolved mechanisms for filtering and regulating flows of information. Indeed it is particularly evident here that information generation and its use is inseparable from specific interests. These interests conspire to decide which versions of reality are legitimate in that, for example, they give legitimacy to chosen courses of action or existing structures. In short, contrary to the tenets of academic research, in organizational settings information is rarely viewed as a 'public good'.

There are several issues here. At the most general level very few, if any, organizations are conducive to the free flow of information. Where it falls within the domain of management control, research is likely to serve to reinforce existing perceptions, assumptions, and prevailing consensus models, or meet the organizational imperative

to report success. As such it is tolerated and supported. More commonly, however, ethnographic or process documentation research modifies existing flows of information or creates new ones beyond management control. In this case, it quickly loses legitimacy, is treated as suspect, resisted, undermined, or, as in the Bangladesh fisheries case, terminated.

Moreover, there is the general problem that ethnographic work – typically exploring and revealing complexity of social relationships and the untidy business of practice – runs up against organizational needs for simplicity and to reduce complexity. Much resistance stems from the perception that research is unnecessarily wasteful of staff time and serves only to reduce the desired manageability of the social world.

It is not only those in management positions who seek to control information flows. Middle-level and field-workers often systematically withhold or distort information from senior executives and conceal poor performance. This is not only to place their own work in a good light, but also to support the claims and demands of the communities or leaders for whom they work. In short, information gets lodged in different parts of an organization, its flow is controlled, guarded, and restricted by individuals holding conflicting priorities (cf. Edwards 1994). More generally, within agencies (as in social life), information is often very clearly a private good, part of an actor's private endowment and a source and instrument of power in negotiating one's position in organizations (Baland and Platteau 1993). This means that there can be powerful individual and collective resistance to any exploration of the sorts of everyday practice in which ethnographers might be interested.

Ethnographers or 'process researchers' are not themselves independent from the relations of power within which information flows are nested. As researchers we operate within frameworks which align us to certain perspectives rather than others, we have points of entry to negotiate and confidences to maintain; and in organizational settings as 'observant participants' we may find ourselves using information strategically to pursue particular ends. The overall point is that ethnographic work in organizations makes the link between power and knowledge unusually clear, endorsing the post-positivist point that knowledge is not made valid by its relation to its object (its objectivity), nor by the consensus underlying its assertions 'but by its relation to our pragmatic interests, our communal perspectives, our needs and our rhetoric' (Baumann 1996: 4, citing Cahoon 1996; cf. Mosse 1998a: 25-6).

The loss of legitimacy that comes from separating information flows from the domain of management control is a common experience in organizational ethnography. In several cases this has ultimately led to the closure of process-monitoring programmes. This loss of legitimacy is expressed in various ways: process work is undermined, for example, when researchers face non-cooperation; it may be neutralized by lowering the status of process monitors, by denying them access to

meetings or documentation, or by carefully circumscribing the areas in which they can work. Indeed, the mere fear of information leaks can make access to organizations by social science researchers extremely difficult. Arguably, this is particularly so in development organizations in which success depends so much upon interpretation and representation.

Does this make an ethnography of organizational practices in development impossible, unless pursued by a reflective 'insider', or are there alternatives? In the context of development programmes, recognition of the problems of the power-knowledge nexus has led some to abandon externally oriented analytical objectives altogether. Instead, social 'research' is used as a means for *engagement* in consensus-building *within* programmes rather than to analyse institutional performance and relationships. As the idea of independent synthetic accounts is abandoned, 'process research' takes on the role of improving performance, enhancing inter-agency understanding, advocacy, facilitation, or nurturing. Process researchers – whether they focus on the processes (meetings, etc.) of village-level user groups (e.g. forest-protection groups), of project-level teams, or of high-level steering committees – produce information in order to facilitate agreements, to hold officials to verbal agreements, to validate policy changes, or to resolve differences (cf. Mosse, Farrington, and Rew 1998). These recent experiences significantly expand the scope of social research in development.

This type of research involves work *within* existing organizational domains and attempts to shape and mould existing flows of information rather than to cut across them. In practice this means being willing to allow agencies to set the agenda, abandoning certain analytical research objectives, and being able to respond to local needs for problem-solving or the production of promotional material. Such an approach undoubtedly has some power in advancing development initiatives, in creating the necessary consensus, resolving differences, and validating progressive change in organizations. But these achievements have their price in terms of the loss of critical reflection. As such they may equally allow the perpetuation of misconceived models, may foster self-serving institutional collaboration, or may contribute to the production of usable fictions and sellable products covering over the gaps between intention and action.

Conclusion

Research in organizations tends to face in one of two directions. On the one hand, it is oriented towards critical analysis, producing an 'institutional ethnography', or an understanding of the 'architecture' of a project, including the structure of interests and motives. Research concerns here tend to be defined outside of the programme where the ultimate consumers of the information lie. On the other hand, some forms of action-research are firmly embedded in the institutions and

processes they reflect upon. Their orientation is towards resolving problems, the working out of consensus, collaboration, or change within the programme setting.

While not entirely incompatible, these two orientations pull in different directions. We end therefore by revisiting the tension between analysis and engagement. This brings a dilemma. The more that the study of project processes are independent, critical, and unrestricted by organization concerns (the more ethnographic it manages to be?), the greater will be its loss of legitimacy and practicality, while the more the analysis is instrumentally focused and tied to project concerns, the less interpretative power it will have (cf. Alvesson 1993: 30, 33). Finally, this tension finds a parallel in the distinction between participatory and critical research perspectives. The more 'participatory' the analysis of communities and institutions is, the less it is likely to reveal about the social dynamics of the participants themselves.

References

- Alvesson, M. (1993), *Cultural Perspectives on Organizations*, Cambridge: Cambridge University Press.
- Appadurai, A. (1989), 'Small-Scale Techniques and Large-Scale Objectives', in P. Bardhan (ed.), *Conversations Between Economists and Anthropologists: Methodological Issues in Measuring Economic Change in Rural India*, Delhi: Oxford University Press.
- Bagadion, B.U. and Korten, F.F. (1991), 'Developing Irrigators' Organizations: A Learning Process Approach', in M.M. Cernea (ed.), *Putting People First: Sociological Variables in Rural Development* (2nd edn), Washington: Oxford University Press for the World Bank.
- Baland, J.M. and Platteau, J.-P. (1993), 'Are Economists Concerned with Power?', *IDS Bulletin*, 24(3): 12–20.
- Baumann, P. (1996), 'A Review of the Literature on Information Generation and Exchange: Implications for Process Documentation and Process Monitoring', unpublished ms., Overseas Development Institute.
- Cahoon, L. (ed.) (1996), *From Modernism to Post-Modernism: An Anthology*, Oxford: Blackwell.
- Chambers, R. (1997), *Whose Reality Counts? Putting the First Last*, London: Intermediate Technology Publications.
- Edwards, M. (1994), 'NGOs in the Age of Information', *IDS Bulletin*, 25(2): 117–24.
- Farrington, J., Gilbert, E., and Khandelwal, R. (1998), 'Process Monitoring And Inter-Organizational Collaboration in Indian Agriculture: Udaipur District and Beyond', in D. Mosse *et al.* (eds).
- Geertz, C. (1973), *The Interpretation of Cultures: Selected Essays*, New York: Basic Books.

- Jones, S., Khare, J.N., Mosse, D., Smith, P., Sodhi, P.S., and Whitcombe, J. (1993), 'The Kribhco Indo-British Rainfed Farming Project: Issues in the Planning and Implementation of Participatory Natural Resource Development', *KRIBP Working Paper No. 1*, Centre for Development Studies, University of Wales, Swansea.
- Khon Kaen University (1987), *Proceedings of the 1985 International Conference on Rapid Rural Appraisal*, Rural, Rural Systems Research and Farming Systems Research Projects, University of Khon Kaen, Thailand.
- Korten, D. (1989), 'Social Science in the Service of Social Transformation', in C.C. Veneración (ed.).
- Lewis, D. (1998), 'Partnership as Process: Building an Institutional Ethnography of an Inter-Agency Aquaculture Project in Bangladesh', in D. Mosse *et al.* (eds).
- Long N. and Long, A. (eds) (1992), *Battlefields of Knowledge: The Interlocking of Theory and Practice in Social Research and Development*, London and New York: Routledge.
- Mosse, D. (1994), 'Authority, Gender and Knowledge: Theoretical Reflections on The Practice of Participatory Rural Appraisal', *Development and Change*, 25(3): 497-526.
- (with the KRIBP Project team) (1995), 'Social Analysis in Participatory Rural Development', *PLA Notes*, 24: 27-33.
- (1996), 'The Social Construction of "People's Knowledge" in Participatory Rural Development', in S. Bastian and N. Bastian (eds), *Assessing Participation: A Debate from South Asia*, Delhi: Konark.
- (1997), 'The Ideology and Politics of Community Participation: Tank Irrigation Development in Colonial and Contemporary Tamil Nadu', in R.L. Stirrat and R.D. Grillo (eds), *Discourse of Development: Anthropological Perspectives*, Oxford: Berg.
- (1998a), 'Process-Oriented Approaches to Development Practice and Social Research', in D. Mosse *et al.* (eds).
- (1998b), 'Process Documentation and Process Monitoring: Cases and Issues', in D. Mosse *et al.* (eds).
- , Ekande, T., Sodhi, P., Jones, S., Mehta, M., and Moitra, U. (1995), 'Approaches to Participatory Planning: A Review of the KRIBP Experience', *KRIBP Working Paper No 5*, Centre for Development Studies, University of Wales, Swansea.
- , Farrington, J. and Rew, A. (eds) (1998), *Development as Process: Concepts and Methods for Working with Complexity*, London and New York: Routledge.
- , Gupta, S., Mehta, M., Shah, V., and Rees, J. (with the KRIBP team) (1997), 'Seasonal Labour Migration in Tribal (Bhil) Western India', *KRIBP Working Paper* (mimeo), Centre for Development Studies, University of Wales, Swansea.
- Parthasarathy, R. and Iyengar, S. (1998), 'Participatory Water Resources Development in Western India: Influencing Policy and Practice Through Process Documentation Research', in D. Mosse *et al.* (eds).

- PLA Notes: Notes on Participatory Learning and Action*. No. 24, 'Critical Reflections from Practice', October 1995, London: International Institute for Environment and Development.
- Pottier, J. (1997), 'Towards an Ethnography of Participatory Appraisal and Research', in R.L. Stirrat and R.D. Grillo (eds), *Discourses of Development: Anthropological Perspectives*, Oxford: Berg.
- and Orone, P. (1995), 'Consensus or Cover-Up? The Limitations of Group Meetings', *PLA Notes*, 24: 38-42.
- Salmen, L. (1987), *Listen to the People: Participant-Observer Evaluation of Development Projects*, New York: Oxford University Press.
- Uphoff, N. (1992), *Learning from Gal Oya: Possibilities for Participatory Development and Post-Newtonian Social Science*, Ithaca and London: Cornell University Press.
- van Velsen, J. (1967), 'The Extended-Case Method and Situational Analysis', in A.L. Epstein (ed.), *The Craft of Social Anthropology*, London: Tavistock.
- Veneración, C. (ed.) (1989), *A Decade of Process Documentation Research: Reflections and Synthesis*, Quezon City: Institute of Philippine Culture, Ateneo de Manila University.
- Volken, H., Kumar, A., and Kaithathara, S. (1982), *Learning from the Rural Poor: Shared Experiences of the Mobile Orientation and Training Team*, New Delhi: Indian Social Institute.
- Wright, S. (ed.) (1994), *Anthropology of Organizations*, London and New York: Routledge.

‘Among Professionals’: Working with Pressure Groups and Local Authorities

Simone Abram

Introduction¹

This chapter concerns the ethnography of policy. Policy is formed within an organization – in this case local government – and it ought, therefore, to relate to anthropological discussions of organizations. The ‘anthropology of organizations’ has tended to imply a parallel with organizational studies – that is, the study of businesses, public bodies, governmental offices, and so on – in response to the use of the notion of ‘culture’ in organizational studies literature (see Wright 1994: 1). Work on local government ‘organization’ has tended to focus on service provision, and rarely on policy processes, despite ethnographic work from other disciplines on policy activities such as planning. However, rarely is the ‘public’ role in planning policy preparation brought into these ethnographic analyses, and this is the key area where anthropological experience can inform debates on ‘public participation’ in policy formulation. The lack of proper public voice in policy processes, particularly regarding international development, has long been of concern to anthropologists (and remains so: see Sillitoe and Bicker 1998, and Mosse, Chapter Eight of this volume). However, as I have pointed out elsewhere (1998), in cases where the lives of social groups are overwhelmed by the forces of international capital coming from outside, particularly from other countries, it is easy to feel justified in defending ‘indigenous knowledge’. In the case of the local government of housing policies, the differences in local knowledges and forms of knowledge become more difficult to analyse, especially because the anthropology of development has not made a major contribution to the debate on forms of democratic government per se (with the notable exception of Robertson 1984; cf. Gardner and Lewis 1996: 68–75).

This chapter introduces an ethnography of a policy that appears in a Local Land-Use Plan for a district in the south-east of England. The focus on a specific

1. The fieldwork research referred to in this chapter was funded by the ESRC (Research Award No. 000222057). I would like to thank Jonathan Murdoch, David Gellner, and Eric Hirsch for their comments on earlier drafts.

policy allows a multi-perspective ethnography that encompasses not only the planners preparing policy, but the politicians responsible for making decisions, and multiple, competing, local residents' groups and landowners or developers attempting to influence decisions. The chapter begins with a very brief introduction to planning in England. (A similar system applies in Wales; the legislation regarding Scotland and Northern Ireland is separate.) It then moves on to introduce the setting of the ethnography, indicating the complexity of relations between competing groups, and the transformation in the expression of resistance throughout the period of policy debate. The chapter demonstrates the tension between the politics of policy-making and the technical competencies of 'experts', such as professional planners. We will explore the efforts of local pressure groups to negotiate the relationship between the politics and the techniques of planning, in the context of local competition over the future of a particular settlement under pressure for growth.

Why an Ethnography of Planning?

The planning system for housing in Britain is the arena for decisions on what gets built where. The legislation and procedures change constantly, but currently the system is hierarchical and can be described as 'plan-led'. In very simple terms, this means that the government, through the Department of the Environment, Transport, and the Regions (DETR), uses the census to forecast housing requirements over twenty-year periods, as the basis for a decision on how many new dwellings will be required in that period. These figures are passed down to county-level authorities, who generally negotiate with each other on how regional projections will be distributed. Counties then distribute their own household figures between districts.² Local decisions on individual applications for development permissions must then be made in accordance with prepared plans. In other words, if a plan indicates that a certain amount of development will be permitted at a certain site, permission will normally be given to whoever applies to carry out that development. Particularly in south-east England, where the pressure for development has been increasingly intense for several decades, local pressure groups are beginning to realize that if they want to retain some control over local development, or if they wish to prevent building on particular environmental sites, they must ensure that the plan does not indicate this site for development, otherwise any local enquiry will be obliged to follow the plan, often despite strong local objections. Hence, more and more local groups are getting involved in local and strategic plan-making in the name of local environmental protection.

2. Since local government reorganization in the late 1990s, a new and less systematic system puts districts and unitary (mostly urban) authorities together into strategic planning groups very similar to the prior counties, to produce joint strategic plans (see <http://www.planning.detr.gov.uk/ppg12/7.htm>).

Ethnographic research on planning is beginning to appear from within planning theory, where there is recognition of the importance of planning policy for environmental management and of the social implications of settlement policies (e.g. Yiftachel 1997), although this work owes more to the geographical than the anthropological tradition of ethnography. Foucauldian analyses of ethnographic experience are currently popular in Planning Theory debates, and some excellent ethnography focuses on the roles of key powerful actors within planning 'networks' (e.g. Flyvbjerg 1998). A few anthropologists have begun to consider the local politics of planning (e.g. Vike 1996), and this chapter suggests that it constitutes an important area for ethnographic study, since the issues under debate concern the future of not only the physical but also the social environment that many of us inhabit. We need only glance at the British local or national newspapers to find concern over new housing development, or travel around the country to see new housing estates appearing at a rapid rate on fields around all the towns and cities, and many villages as well, while in other areas houses are lying empty. Sites of Special Scientific Interest are often destroyed to build new roads, or for open-cast minerals mining, and alternative 'environmentally friendly' settlements are threatened by landlords who want to build executive housing estates for commuters. (For a particularly vivid example of this, see Holtsfield Residents' Association 1998.) All these developments are governed by planning laws, but much of the application of the law is organized through local government planning officers who subscribe to particular rationalities which make them deaf to appeals from certain quarters. This renders some citizens voiceless, not necessarily through lack of knowledge or even education and wealth, but it ensures that planning is distanced from general concerns raised by objectors through the use of limiting discourses.

Earlier research on the use of public consultation in the preparation of strategic policy raised some questions on the activity of planning (see Abram *et al.* 1996, 1997; Murdoch and Abram 1998). It was very clear that a great deal of effort was required from local residents to have even the smallest influence over local government decisions on the future of settlements through the statutory procedures for public consultation in planning-policy formulation. This raised a number of questions about participative and representative democracy. For instance, why was so much stress placed on public participation in planning when so little attention was paid to the views of people who did participate? Why did people feel the need to participate when they had elected local politicians to represent their interests? Why did planning policies appear to go against the interests of many residents? Answers to these questions might also reveal why so many more people did not participate in the preparation of planning policy, a problem that concerned many planners and politicians alike.

In any case of local authority practice, the pragmatic management of public participation, or consultation, has its roots in some much deeper questions about

local democracy and political activity. Local authority officers, in this case planners, are the intermediaries between local politicians, the public and the central state authority (government), and therefore the relations between these individuals and groups are crucial to the effectiveness of any policy process. What became clear during this research project was that each of these sets of people had different understandings of development and of the environment, as well as of democracy, and that this deeply coloured both relations between them and their approaches to planning. In order to look at this more closely, I embarked on a detailed ethnography of local planning, where I examined the different perceptions of development among the various participants in planning negotiations.

Studying the Policy Process

This later project focused on the production of a new district-wide plan for a district in the county of Buckinghamshire, north-west of London. However, the research project was based neither solely in the planning office nor in a single 'community', if such a thing can be said to have substance. The focal point around which the ethnography revolved was related to a set of policies which identified a field in a village as a suitable location for the development of a housing estate whose size altered through the policy process from 300–400 houses down to around 100. The conceptual 'target' of the ethnography was binary, being both the preparation of the plan itself and notions of villageness held by residents in the village. In other words, while part of the research was recognizably 'village-based', the most important aim of the research was to record and analyse the processes by which policy was formulated by the many different, often competing, actors involved (see Wright 1995).

One of the difficulties of this ethnography, therefore, was the need to try to make contact with many different, often conflicting groups. Initially, I expected most resistance from the local authority officers, because I anticipated that their official positions would make it awkward, first, for me to persuade them to give up sufficient time to talk to me and, second, for them to present anything other than the 'official line' to me about their work. In the event, however, these fears were not fully realized, possibly influenced by the fact that my employer at the time was a well-known school of Planning. Several, but not all, of the planners in the District Council offices were glad of the opportunity for reflection on their roles and activities, one not often presented to them amidst the routine everyday round of their work.

However, time spent with planners was organized formally in terms of meetings, and I was not offered access to council offices.³ It was possible to hold informal discussions with planners on other occasions, though. A public exhibition of the

3. This contrasts dramatically with more recent fieldwork in Norway (2000), where full and open access was offered by a district council, which also made an office available for me as guest researcher.

draft local plan held on a bitter January day attracted only a handful of visitors, leaving plenty of opportunities for me to chat with planners and participate in their discussions about their lives and roles, as they described how they had decided to become planners, and compared careers. It was clear that contrary to what many villagers believed about planning the planners held to a belief in a 'greater good': they made clear their belief in the need for more housing for 'concealed households' and for provision to be made for changing living patterns. A striking commonality of purpose was evident among these professionals, who expressed a belief in their role as providers of housing, a basic human right, for those who needed it. Striking, also, was a level of recognition of the limits to their ability to achieve this, expressed as resignation, perhaps cynicism and a degree of ill-ease with their roles as servants to political groups they sometimes personally disagreed with.

A certain form of rationalization of this problem was also discussed. The planners explained the limitations on them as to what they could achieve. Much of this was steeped in planning discourse that could be distilled from governmental Planning Guidance Notes. For instance, I asked one planner why they should want to develop a large amount of new housing in the district's main town, and he explained that it was necessary because the government wanted four million houses built and all the other places round London are already built-up and under severe constraints, whereas the town can expand. The town has long been identified in regional guidance as a sort of 'sacrificial site' for expansion which keeps the pressure for development off the exclusive southern areas of the county (see Allen, Massey, and Cochrane 1998: 87), and the district planners began their planning approaches from this starting point.

The planners discussed the difficulty of separating professional roles from personal politics, and characterized the problems they faced in working with politicians of different parties. They joked about pompous local 'squires', recounted horror stories of right-wing bigots demanding preferential treatment over the unworthy poor, and referred to influential episodes in their personal development, including an enlightening encounter with Greenpeace, an eye-opening training course with the RSPB, and the responses from local residents to one planner's ANC and 'support the miners' car stickers. This fortuitous meeting formed the basis for continuing discussions with some of the planners who felt able to discuss planning policies with me openly and frankly. It also offered me an initial insight into the lives of the planners as whole people who often remain hidden behind their professional personae as disinterested local government servants. This contrasted strongly with the characterization of planners by many local residents as alternately misguided, incompetent, anti-environmental, or as being pawns trapped in a bureaucratic system. Here was a demonstration of the tension between the politics and techniques of planning made visible through the difficult relationships between professional planners and politicians.

Local Responses

The District Local Plan (DLP) emerged from a changing statutory planning process. Until 1995 districts were obliged to produce separate plans for urban areas and rural areas, so when new requirements appeared for district-wide local plans, the planners began to merge their existing documents with the requirements of the strategic County Structure Plan for Buckinghamshire. Plans are repeatedly updated, and new plans are often based on revisions of former plans, which makes it difficult to identify a discrete policy process. However, the preparations of the new plan for the district began when district council planners prepared a set of issues papers introducing the idea that planning for the district required an overall strategy. Much of the strategy was prescribed by central government, suggesting that new development should be concentrated in existing settlements that were well served by public transport and public facilities (shops, health and education centres, and leisure facilities). The first draft version of the plan, which went out to public consultation in 1996, contained an interpretation of that strategy which proposed to concentrate development in the main town and in the larger settlements in the rural areas. Political input to this early plan was quite understated. The council had recently ceded from Conservative to Liberal Democrat control, and many of the councillors on the planning committee were quite inexperienced, with little knowledge of planning. The full council had agreed a strategy that appeared to be environmentally orientated, promoting 'sustainable development'. Once the plan was published and awareness was raised in the district of the proposals contained within it, the politics of planning began to appear increasingly significant to the physical and social future of the District and the communities within it.

As the District Plan of the settlements proposed for further development became known to residents, a number of groups began to coalesce around certain responses to the plan. Most reaction came from one large village in the rural area of the District, which had experienced approximately a ten-fold growth in population since the 1950s. Villagers reacted in various ways to the proposals for a new housing estate on one edge of the village, and their responses could be summarized into three main positions, which we can label 'pragmatic planning', 'old villagers', and 'no growth'.

'Pragmatic Planning'

In this view, villagers with many years of experience of responding to planning proposals and enquiries recognize that the district has a large allocation of housing to find sites for. Given the district's proclaimed desire to follow a 'sustainable' planning strategy, interpreted as the preference for development in places with good public services and public transport links, it appeared inevitable that the

village would be identified for further housing over the next twenty years. It was therefore in the villagers' interests to make sure that the figure allocated should be limited to a 'reasonable' amount and that it should be well situated and well designed. This view was promoted by the Village Society, some of whose members were professional planners working in other districts.

'Old Villagers'

This term was often used to describe members of families long-established in the village. As well as kinship, the implication was that 'old villagers' had personal or collective memories of life in the village prior to the massive expansions during and after the Second World War. Rather like Zonabend's description of the war forming a juncture between the endless past and the modern present (1984), villagers reflected on an earlier period when it was imagined that village families were identifiable, all villagers knew each other personally, and villagers built new houses for their growing families within the village, from local materials, particularly 'witchert' (mud and straw), a building technique used well into the twentieth century. For many of these 'old villagers' the latest round of growth was simply another step in a long process, and often they accused those protesting against it of being hypocrites: had the old villagers objected to growth in previous plans, the new protesters would not be living in the village to protest against further development.

'No Growth'

A large body of village residents responded to the prospect of further expansion with horror. The idea that a new set of green fields (currently used for crops) should start to be built on threatened their notion of living in a rural area. They imagined the countryside being 'concreted over', and the picturesque English village transformed into a suburban landscape. The site was bounded by houses on two sides and roads on the other two sides and, once part of the site was released for housing, there would be little 'rational' argument against releasing the rest of the site for housing. At stake, therefore, was not a question of a hundred houses on a few acres, but the potential for up to a thousand houses on the whole site. It would only require the council to release land for business purposes elsewhere in the village, thus creating more employment, to justify large-scale housing development, until the village became just the kind of large, impersonal town that those who had moved in over the previous ten or twenty years had consciously wanted to avoid. This argument held a powerful sway over many residents who also recognized that some of the village's services were already stretched. The health centre was in financial difficulties and was in need of another partner, although

there was no room to expand its present accommodation. Waiting lists for GP appointments were increasing, with no prospect of improvement. The village primary schools were also having difficulties in accommodating certain classes with large age-cohorts (particularly at the more fashionable 'church' school) so that some class sizes were larger than normal. The idea that one hundred new families could be added to the GPs' lists and their children be included in the bulging classes seemed nonsensical. Villagers subscribing to this view began to organize into a pressure group, which adopted the title of 'Protection Society'.

Official, state-sponsored representation for the village consisted of a Parish Council and two District Councillors. The village also had a (Conservative) County Councillor, whose seat also covered some small neighbouring settlements. The Parish Council, while normally an elected body, had not had sufficient candidates to hold an election, so its members were unelected. However, far from being a list of village noteworthies, as it had been in the 'old village' days, its members represented each of the above views, including 'old villagers', incomers of long standing, and people who had moved to the village more recently. They also represented a range of party political standpoints, although all members insisted that party politics should not inform local Parish politics. Two members of the Parish Council were also local landowners, who were therefore unable to participate in any debates over the local plan, since they potentially stood to gain financially from the release of land for housing. The two district councillors, though, were both Liberal Democrats, although only one of these councillors was active during most of the preparation of the plan, the other having moved away. The latter resigned only some months later, leaving the village with only half its representation on the District Council in the interim. This appeared to be a party strategy to minimize the chances of losing the seat.

In response to previous plans, only the Parish Council and Village Society made collective objections. When the County Structure Plan was being prepared, a few years previously, the Village Society sent a delegation to the Examination in Public (a sort of informal public enquiry) to press for the removal of the mention of the village as a 'Key Rural Settlement', which they felt would lead to massive development pressures in the local plan and which had, in fact, been specifically recommended against during an earlier planning dispute by the Secretary of State (the government minister who has the final say on contested planning decisions). They were successful, although most of the other villagers were unaware of this since general awareness of structure planning was low. Broad interest in the plans only arose with the specific suggestion that a new housing estate might be built on a green field in the village, when a small 'ginger' group (i.e. a group of villagers trying to provoke interest in the issues) distributed leaflets suggesting 'thousands of houses' would come to the village, prompting a large turnout to a village meeting, at which the Protection Society was formed.

However, before it appears too neat to suggest that three groups held three different views, we must note that many of the Village Society members also joined the Protection Society. Furthermore, members of each society were on the Parish Council. As Howe has suggested (1998), resistance to powerful institutions is rarely singular, and in this case, among the 'powerful' as well as the 'weak', there was commonality and conflict. While many of those making objections could hardly be described as 'weak' in terms of education, wealth, or access to public institutions, they were still rendered relatively powerless in the planning process by the exclusivity of the planning discourse. In fact, what distinguished the groups was partly their familiarity with this discourse and associated practices, and what became evident through the plan-preparation process was the transition of pressure groups' arguments from 'common sense' to 'planning discourse' in their quest for effectiveness.

The Protection Society, beginning from a position of outrage and emotive resistance, prepared statements for villagers to send to the District Council planning office, and distributed them throughout the village. These letters suggested that the village was already bulging, and that an additional round of housing estates would urbanize the village beyond acceptability. The efficiency of this method of public-consciousness-raising was such that of some 3,000 responses to the publication of the Draft District-wide Plan, around 1,800 were from villagers, protesting at the policy to build on a green field. This action effectively put a great deal of pressure on the local politician to act in the interests of his constituents, and in doing so highlights one of the major confusions among local people over how planning policies are arrived at.

Channelling Emotions

The objections sent by village residents, along with comments on the plan from all respondents (including business groups, developers, Parish Councils, etc.) were received and collated by District Planners, whose duty was to present them to councillors with a set of recommendations as to how the Council should respond to them. It is particularly clear at this point in the consultation process how planners attempt to transform political pressures into technical arguments, a process they articulate through a specific discourse on the limitations of planning powers.

Many village residents first assumed that if 'the whole village' were to object to the policies, on the grounds of not wanting further development, then this would persuade planners that they had made a mistake in identifying the sites. However, planners were keen to point out that public consultation was not supposed to act as any kind of 'opinion poll'. One planner explained, attempting to redefine the boundaries between politics and technical limitations, as follows:

Say a thousand people wrote in and said, 'This will destroy our property values' (and that's what a lot of people have said). That will have next to no value in planning terms. And one person can even write in and say, 'You can't get access on this site because I've measured it and you can't get access because you'll have to knock down half the street' or something; that one comment will have far more weight than a thousand about property values.

This forms almost a truism in British planning: that is, first, that planning is a technical activity rather than a vehicle for public representation, and, second, that few 'members of the public' are aware of this. With this discourse, planners attempt to duck political pressures, and exclude broad social problems from their technical 'solutions' by invoking governmental limits to planning's powers and domains. Technical 'knowledge' holds inherent superiority in planning over emotional, social, psychological, or political knowledge, and the planner's explanation rationalizes and reproduces this continued preference.

This preference for 'technical' 'planning' knowledge also upholds a common planning perception of a generalized, planning-ignorant, and selfish 'public'. The notion of 'educating the public' plays an important role in the conceptual vocabulary of planning, legitimating planners' 'expert' knowledge in contrast to widespread ignorance, and justifying a common refusal, or perhaps inability, to listen to people's responses to plans by categorizing them as planning-unaware. However, many planners do recognize this as a categorization that is not universally applicable, since, as Forester (1989) argues, technical planning is only one of a range of techniques that planners employ; and, as one of the planners said:

In fact, in those thousand comments, you will get a lot of planning-related comments – people aren't that stupid, they have a lot of planning-related concerns.

Despite this acknowledgement, the planner reiterated the notion that individual residents as 'members of the public' are self-interested, with reference to one of the guiding principles of planning:

Planning is for the public interest. It's the public interest in the widest possible sphere. It's not about the residents today, with their back garden, it's about everybody's living in the future and private amenity has some part to play in that, but it can't be an over-riding concern against everything else.

The planner then backed up this assertion with a reference to Governmental papers:

The new PPG1 [Planning Policy Guidance notes issued by the Government] – there's stuff in there about how you evaluate substantial numbers of objections and the duty of the politicians and the duty really of evaluation of proper terms so it's worth reading, that's a good document to understand where we're coming from.

This suggestion encapsulates a very powerful ideology within planning, that of the 'public interest' being served by 'strategic' decisions. This is the notion, stressed in recent planning legislation (Planning and Compensation Act 1991) that at each level of planning – national, regional, county, or local – individual planning policies and decisions are guided by 'strategic' general principles, from which good planning results will flow in logical progression. However, planners rarely stress that strategic decisions are made by politicians, quite possibly not in the interests of the 'greater public'. For instance, a strategy to concentrate development in existing urban areas could be said to be simply protecting exclusive, wealthy areas occupied mainly by people who vote for a particular party, at the expense of 'town-cramming' for those who cannot afford to move into, or to remain in, more desirable areas. However, all political decisions must be examined by government-appointed planning inspectors, and can be challenged by the Secretary of State, who can, in turn, be challenged in the High Court if decisions appear not to comply with planning legislation. The politics of planning, therefore, operate within the technical limitations of planning legislation, but within those limits (themselves set by political choices since, effectively, government makes legislation) political courses can be followed. What is clear about the relationship between planning and politics is that there is a complex interaction between the two whereby each reins in the other and, in turn, follows the other's leads.

Learning Planning Speak

Given the complexity of the relationship between politics and technical expertise in planning, and their constitution of an ignorant and self-centred 'public', any form of local resistance requires a high degree of both technical knowledge and political influence, and a great deal of perseverance. Different groups within the village demonstrated different degrees of effectiveness in this process, not merely reflecting their experience of planning. Indeed, those most experienced in planning had often become incorporated into planning discourse, thereby limiting their ability to contradict strategies and tacit assumptions implied in the technical jargon of planning.

The Village Society's approach had been to put forward sound planning principles to the planning committee, to be considered also at a public enquiry on the plan, accepting the need for the District to find housing sites, and regretting their decision to site it in the village. However, having admitted that they accepted the strategy, they had then to argue that some housing must be accepted in the village, leaving them only to argue that it must be minimized and strictly controlled. Their detailed experience of planning procedures (their advisers including a retired government planning inspector) drew them in to planning discourses concerning

the demands on government to accommodate development pressure, and the factors that suggested that the village was an appropriate site for growth. It could be said that they were, to some extent, doing the planners' work for them, and translating their own feelings about the future of the village into technical planning terms themselves. This contrasted sharply with the initial broad and emotive reactions of the Protection Society.

However, since the preliminary consultation on the draft plan, the Protection Society committee had spent a great deal of time and effort (and money) investigating the rules of the planning system. They had read government guidance notes and legislation, taken advice from a planning solicitor, and joined the national Association of Small Historic Towns and Villages. They had talked to other pressure groups and canvassed support from the other villages that had raised resistance to the plans. They had also spoken with District Council planners to discuss the alternatives for the allocation of housing. While the planners had attempted to convince the villagers of their reasons for putting forward certain proposals, the Protection Society members had used the opportunity to calculate how they could reformulate their arguments to make them carry more weight. The Protection Society group began to focus on the necessity for the plan to go to a public enquiry before it could be formally 'adopted' by the District Council, and in the year between responding to the first Draft plan and the publication of the 'deposit draft' (the version of the plan that would go to public enquiry), they had developed a great deal of planning knowledge, and a new strategy. They had seized on the government's preference for the re-use of previously occupied land (so-called Brown Field Sites) over land that had not been previously built on ('Green Field Sites'). They began to argue that the District had a duty to use Brown Field sites before any Green Fields were breached, and that this applied equally to the rural parts of the district as to the urban areas. They also argued that developing the site in the village would increase traffic, since it was nearly a mile to the railway station from the site, which was at the opposite extremity of the village. They began to acknowledge that they were not completely 'anti-growth', as they accepted that 'infill' would allow an increase in village housing whether or not the new site was allowed, and that they were happy to see continued, small-scale growth. They also acknowledged, as they had from the beginning, the need for low-cost housing in the village. This latter factor was almost universally recognized in the village, as housing costs had spiralled since the 1960s, and the children from low-income homes were all obliged to move to cheaper areas to find their own accommodation.

This gradual transformation from an emotive, 'felt' argument into a calculated, 'planning' argument was noted by the local planners. For them it represented a reining in of 'emotional' response into a technically informed discourse. One commented:

If you look at the Protection Society, early on, a lot of it was irrelevant but gradually it evolved into planning-speak. They've taken on the jargon and to a degree they've taken on the thinking and they're using that, so they've matured that as an argument.

As the villagers began to translate and rationalize their arguments into a planning discourse, planners found themselves more able to 'work with' them and negotiate potential alternative policy proposals. However, in adopting this discourse, the Protection Society had to drop references to their concerns over health provision, urbanization, or the potential increase in vandalism from a growth in the number of young, unemployed residents with no meeting places or leisure facilities.

Paradoxically, planners also recognized that part of their own duty was to 'educate and inform' the public and to consult them over their future plans, and this duty must be carried out with some minimum effectiveness. Planners also have to respond to all sorts of public reactions, and they need to channel emotional responses through technical filters in order to redirect often aggressive or angry respondents. Their occasional experiences of blatant racism and snobbery lead them often to a level of cynicism over the responses they receive at public meetings. Another planner explained these two points:

If we are inconsistent in the way we consult people, then the validity of the process can be challenged in law, and maybe the plan eventually made null. So we've had that sort of fairly intelligent tactic played upon us. Individuals get very, very angry when they approach us. They find it very difficult to speak; one has to be patient in dealing with them. There are others who make threatening gestures, physically poke you. Their behaviour is basically threatening. But there are others who acknowledge that you've got a job to do, your job is to inform them and they will exploit that to the full -- they will cross-examine you, but in a good-natured way, in order to get as much information. Indeed, one man said to me last week, 'Do you think I now have sufficient information to enable me to make the case against this proposal? What more can you supply that might enable me to?' There's a very telling question!

Planners' recognition that residents' views must be tutored does not prevent them from using pacifying tactics. In one instance, an angry resident came to a village planning exhibition, saying 'I know it won't make a blind bit of difference what I think about this. You're going to do it anyway aren't you? What can I do about it?' The planner quoted above let the woman rage, then stepped back towards her, saying, 'Here's what you can do: you can take one of these forms that we have brought with us today, and you can write there exactly what you think about the plans, and you can return the form to my office before the end of the consultation period. That is what you can do to have your say, and the council have a statutory duty to consider whatever you say.' This graphically demonstrates the planner's tactics in trying to channel emotional anger into technical discourse.

He was well aware that such emotional responses would not be useful in justifying the council's plan to a public enquiry, or to a planning appeal made by frustrated developers, and would therefore do little to change the policy to build in the village. While her response might possibly influence local politicians, planners are often uncomfortable with such outbursts, and try to redirect them into the technical arguments which form their professional expertise.

Planners occupy an ambiguous role in the preparation of forward plans. While they are responsible for preparing technical details and offering alternatives for politicians to choose between, they are also cast in the role of presenting the politicians' decisions to the public through these 'public consultations'. Occasionally, politicians turn up to public planning displays, known as 'surgeries', which are held in the main sites identified in the plans. As the planner explained, again:

We have had local members [councillors] attend what we call surgeries, these local exhibitions-cum-meetings. They're not formal meetings where we make a presentation, we just make ourselves available to be questioned, to provide information and to equip people to respond to the consultation as best we can. So local members tend to turn up because they are the people, the councillors representing that particular ward. I think they're there not to promote the plan or to defend the plan, I believe they're there to gauge public opinion. Indeed, to do their job as representing those local people. But – and they play it different ways – some people say, 'Yes, I will defend you. You tell me what you want to say and I will say that at council, I will be your local representative, I will speak up for you at council.' There will be others who will say, 'I've got to hear what you've got to say, that is my job, but I'm also a member of council and a member of a corporate decision-making body, and I have to try to incorporate what you say with the interests of the greater majority.' My responsibility is not to any one community, whereas members do have responsibility to individual communities, the communities they represent. I am employed and appointed by the council to represent the council as a whole, and my interest is what I believe to be the interests of the whole of the council, trying to weigh everything, the interests of all the communities at the same time.

In this short speech, the planner reiterates his belief in 'public interest', while also disowning the plan and showing that politicians often do not assume responsibility for the 'corporate' decisions of the whole council. It is the planners' duty to justify the council's decisions through planning arguments, and they do this at surgeries and at public enquiries, even, sometimes, when they know the arguments to be, at best, 'weak'.

Politics Bite Back

In this discussion of the power of technical discourse at the expense of wider social or political interests, it is easy to imagine a technocracy dominating policy-making, against which politicians are rendered powerless. However, this is not a

fair representation of the policy process. Although politicians' decisions can and are judged by judicial enquiries based on planning law, they do hold a great deal of control over detailed site allocations and individual policy. Politicians also have to play a number of roles in policy-making, attempting to take control of technical decisions, but also having to negotiate the interests of their constituents with those of other politicians from their own party groups, and those of other parties. Furthermore, District Councillors are under pressure to adopt a 'corporate' or 'strategic' Council perspective to consider the interests of the 'whole district' over their narrow sectoral interests, and adopt a role described by Councillors and officers as 'responsible' (as opposed to an 'irresponsible', localistic approach). In other words, councillors are under pressure to agree a general strategy and stick to it, whatever the consequences for their own wards. This, in particular, can lead to cynicism and mistrust from constituents who may feel that their councillor is failing to represent their interests sufficiently well, and therefore not carrying out the duties he or she was elected for. The strategic view is very problematic in local politics, precisely because it is often seen as contrary to the interests of particular sectors of constituents, and because it makes local decisions difficult for pressure groups to alter.

The adhesion to strategy effectively means that once a political approach is agreed, it is very difficult for local objectors to alter decisions, making it more important that objectors respond to strategic policies before they can know what the implications of these policies will be. The difficulties of getting people to respond to 'abstract' strategic policies is often cited by planners as an example of the public ignorance of the importance of planning, and of residents' narrow-mindedness in only caring about their locality once it is threatened, and showing no interest in broader issues. At stake here is the construction of a notional 'public' with poor planning qualities. As shorthand for this approach to planning, the acronym 'NIMBY' (Not In My Back Yard), and variations on it, have become common currency in planning debates. The tendency of strategists and developers to dismiss any local or site-specific objections to their proposals as 'NIMBY' has reached such a level that at the beginning of debates on the plan at the local council, the chair of the planning committee requested that the term not be used, since it undermines proper consideration of all views.

In fact, the real import of a large body of objection is the pressure felt by politicians whose position as local representatives is threatened by unpopularity. Only when a large body of objections to development in the village was received by the council did the local councillor, who was at that time also standing for parliament, begin to try to dissuade his colleagues from continuing with their proposals. In an act of political bravura, he put forward a vote during a planning committee meeting to redistribute the allocation of housing for the village among several other, smaller villages, also minimizing the allocation for a small town

under Conservative control, and thereby ensuring support for his proposal. Members of his own political group felt obliged to support the proposal since he was at that point the leading member of what was the leading political group. The planners were despondent: the consequences, they knew, would mean an extra year added to the timetable for preparation of the plan since the new proposals would have to be drafted with appropriate background papers and sent out for public consultation, any responses collated and debated by council, and a new draft plan prepared to reach the same position in the plan-preparation process that they thought they had already reached. While the politician may have considered the move would increase his popularity, in fact it proved to have been a highly risky manoeuvre. Many of his party colleagues were angry that he had turned away from the strategy they had spent two years preparing, that of concentrating development in the existing largest settlements, and were disappointed in the new policy which, to them, was 'unsustainable', since it proposed building houses in villages where residents would have no alternative to private car transport.

The planners were disappointed, not merely because of the extra work (which they were prepared to accept as part of their job) but because they would find difficulty in justifying this turn of events to a planning inspector at public enquiry, as it was such a transparently political move which would be almost impossible to dress up as sound planning policy, and this would make their plan vulnerable to pressure from developers to build on what they considered to be 'less sustainable' sites. They also knew the policy would be extremely unpopular in the smaller villages, and as they would be the public face of the policy, they would 'take the brickbats'. Many villagers, also, were far from satisfied with the change in allocation, since 150 houses remained in the village allocation, and were now to be considered on more than one site, which represented more sites for conflict and the dissipation of resistance. However, the amended policy was passed, written into the draft plan and taken to public consultation despite the technical objections of the planners and other politicians.

In conclusion, we can see that preparing a plan is an obstacle course through legislation, politics, and public representation. Roles are cast which are difficult to influence, and 'public consultation' is an extremely partial activity. This effectively allows the central state to effect change at the local level, by directing citizens either to adopt or struggle with the roles they have been allowed. If they argue from outside the planning discourse, they must have resort to a great deal of political influence; otherwise they must become expert in planning procedures in order to 'catch out' the planners on technical issues. While this is possible, and does happen, it requires a massive dedication of effort from citizens to become highly informed about planning techniques, for them to face professional planners whose full-time occupation is the preparation of planning policy. For developers, both tactics are adopted, but most importantly (contrary to popular opinion), the latter tactic of

catching out local planning committees on technical procedural details is a common ploy. Planners know that many competing actors, including extremely articulate and experienced planning barristers acting for particular interests (especially developers or landowners), will be attempting to pick holes in their technical arguments, and they must therefore try to ensure that the politics of planning are constantly drawn back into the realm of technical detail.

Conclusions: Positioning the Ethnographer

In contrast with classic ethnographic studies, the sort of study described above requires the ethnographer not only to understand one particular group, and then represent them to the rest of the world (on a model of 'advocacy'; see Huizer 1996), but to understand the encounters between different groups. Anthropology has paid increasing attention to the encounters between groups, such as that between bureaucrat and citizen (Lipsky 1980), or between medical officer and patient. These are well-recognized examples of the encounter between forms of knowledge, but recent work has considered the relations between sub-sets of these groups, such as between groups of welfare officers (Cullen 1994) or the individuals whose work is to mediate between such groups, such as Housing Aid workers (Edwards 1994), or the relations between welfare officers and politicians (Vike 1996). In the context of ethnography of policy, there may be many different forms of social organization operating in reference to a single policy, some of which may be better defined than others. The idea of a policy network – attempting to 'map' all those who take a part in preparing a policy – indicates how complex merely identifying such actors can be (see Marsh and Rhodes 1992).

The ethnographer must become familiar with each of the different groups separately, in order to understand encounters between the groups. This presents particular problems of trust and loyalty. It has traditionally been the case that ethnographers have worked hard to gain the trust of informants from one social group, and have retained some loyalty to that group. In development encounters, this has often proved to be problematic, since anthropologists who act as advocates for a group to a development agency may find themselves without significant influence, but those who gain influence with development agencies may find their loyalty to 'their' group comes under strain. This may also be the case in studies of organizations, where ethnographers working at the lower echelons of an organization may often have to address the perception that they are 'management spies'. Indeed, when a study is commissioned by senior management, this perception may have some truth, and efforts to gain the confidence of workers may require a laying down of boundaries about what may or may not be divulged in the final report.

For anthropologists who wish to study encounters between groups, there may also be a requirement to suspend loyalty, or to retain a more neutral position.

It may or may not be possible to gain the trust of all parties to a dispute or conflict, or an anthropologist may need to split their loyalties, but the individual must decide how to respond to the tendency to be pushed into the role of mediator between groups. The role of anthropologist of policy in between groups echoes that described in the ethnography of science and technology, as the quest to conduct a 'symmetrical' analysis. It is important to distinguish between the notion that an individual can represent all sides of an argument, and the idea that they might be 'apolitical' or, more unrealistically, 'objective'. Even the most disinterested observer cannot be described as apolitical. Recent French ethnography of science has chosen to adopt a stance described as 'symmetry', which has two meanings. In relation to the ethnography of laboratories or other forms of scientific research, ethnographers like Latour (1993) have argued that scientific arguments must be incorporated into the ethnography as problematic statements, rather as beliefs or myths are in classic anthropological texts. As Latour indicates, the early sociology of knowledge only attempted to explain deviations from reason, whereas truth needed no explanation (ibid.: 92). Latour follows Serres' argument that if you want to explain flying saucers, you must be able to use the same explanation for black holes: in other words, we can only understand why some forms of explanation fail if we understand how others succeed; that is, truth and falsehood should be treated equally. Latour pushes this further, and adds another dimension to the idea of symmetry, whereby society and nature should also be incorporated equally into our analyses of the world, and the anthropologist should consider both human and nonhuman actors within the ethnography. He notes that the tendency of the West to define everyone else as having authentic cultures, whereas the West supposedly does not, is a reflection of the distinction between nature and culture central to Western self-understanding. In response to this, he argues for a symmetrical ethnography which attributes equal weight to all the different arguments of the participants to any debate. In order to represent fairly any situation, the anthropologist must not apportion weight to one argument over another, but must only trace the networks of power that lead to one argument winning out over another. There is some similarity here with the methods of Foucault, whose historical, genealogical method attempted to achieve a similar goal (see Davidson 1986). Latour therefore stresses the equality of voices between the analyst and the field, which creates a hybrid text, 'a mixture of "them" and "us", of "our" ways of talking and "theirs"' (Murdoch 1997: 751). The anthropologist is thus not apolitical but can adopt a disinterested stance, and try to follow through the process by which one particular view becomes dominant over others.

At the beginning of my study of local planning, the planners, politicians, and three residents' groups were in all conflict with each other. However, I explained the aims of my study and my desire to understand all the points of view to people in each group; all my informants took it upon themselves to offer a righteous

version of the situation. In other words, most of my key informants took upon themselves the role of tutor, and by posing as an interested student, I put my informants in a position of authority within my encounters with them. However, before I conducted any interviews, I familiarized myself with each of the groups, and thereby allowed them to become familiar with me. Through participation in various local social activities, and through my presence at council meetings, my interest could be seen to be genuine, and my willingness to participate became apparent. In fact, by doing voluntary work and joining village groups, I acted as a 'good villager', and by attending council meetings, I acted as a 'good citizen', rather than simply a nosy outsider, and gained some respect from the people I wished to learn from. Moreover, through some level of shared experience, as Judith Okely has noted (1994), I was able to understand some non-verbal communication, and to appreciate those sentiments and arguments voiced by my correspondents.

As with all fieldwork, ethnographers necessarily present themselves as wishing to learn from informants. In the case of fieldwork among professionals, it is important to differentiate professional knowledge acquired during training from professional practice. Ethnographers who are qualified professionals in the field that they study have the advantages and disadvantages of the ethnographer 'at home' in that they have a depth of insight into the cultural resources of the professionals, but may be so familiar with the profession that it could be difficult to problematize some of those deep assumptions. In fact, it is often some time after fieldwork that these become apparent, since explaining your experience to other people often prompts you to rethink this experience and reformulate it according to different questions.

In most fieldwork situations, we are looking for symbols and signs of communication, which act as shorthand for more complex concepts or systems. Often these symbols are so familiar to those who use them that they do not even notice them, although they articulate a fundamental belief or practice to those people. One of the jobs of the anthropologist is to recognize these symbols or metaphors, and to problematize them, and to ask people to reflect upon them in order to make them explicit. This is in itself a political activity, since it causes people to be reflective about their own lives and language, and it may be welcome or quite offensive to the people we ask. If knowledge is power, then different kinds of knowledge offer different forms of power, including our own anthropological forms, and it is crucial that we both address this issue and acknowledge it in our ethnographic practices.

References

- Abram, S. (1998), 'Introduction', in S. Abram and J. Waldren (eds), *Anthropological Perspectives on Local Development*, London: Routledge.

- Abram S., Murdoch, J., and Marsden, T. (1996), 'The Social Construction of Middle England: The Politics of Participation in Forward Planning', *Journal of Rural Studies*, 12(4): 353–64.
- Abram, S., Murdoch, J., and Marsden, T. (1997), 'Planning by Numbers: Migration and Statistical Governance', in P. Boyle and K. Halfacree (eds), *Migration into Rural Areas: Theories and Issues*, Wiley: Chichester.
- Allen, J., Massey, D., and Cochrane, A. (1998), *Rethinking the Region*, London: Routledge.
- Cullen, S. (1994), 'Culture, Gender and Organizational Change in British Welfare Benefits Services', in S. Wright (ed.), *Anthropology of Organizations*, London: Routledge.
- Davidson, A.I. (1986), 'Archaeology, Genealogy, Ethics' in D.C. Hoy (ed.), *Foucault: A Critical Reader*, Oxford: Blackwell.
- Edwards, J. (1994), 'Idioms of Bureaucracy and Informality in a Local Housing Aid Office' in S. Wright (ed.), *Anthropology of Organizations*, London: Routledge.
- Flyvbjerg, B. (1998), *Rationality and Power: Democracy in Practice*, tr. Steven Sampson; Chicago: University of Chicago Press.
- Forester, J. (1989), *Planning in the Face of Power*, Berkeley: University of California Press.
- Gardner, K. and Lewis, D. (1996), *Anthropology, Development and the Post-Modern Challenge*, London: Pluto Press.
- Holtsfield Residents' Association (1998), *Holtsfield: It's Where We Belong – And Where We Plan To Stay!* <http://iip.co.uk/www/holtsfield/>
- Howe, L. (1998), 'Scrounger, Worker, Beggarman, Cheat: The Dynamics of Unemployment and the Politics of Resistance', *JRAI* (N.S.), 4: 531–50.
- Huizer, G. (1996), 'Anthropology as Advocacy', *Etnofoor* 9(2): 31–50.
- Latour, B. (1993), *We Have Never Been Modern*, tr. C. Porter, Cambridge, Mass: Harvard University Press.
- Lipsky, M. (1980), *Street-level Bureaucracy: Dilemmas of the Individual in Public Services*, New York: Russell Sage Foundation.
- Marsh, D. and Rhodes, R.A.W. (eds) (1992), *Policy Networks in British Government*, Oxford: Oxford University Press.
- Murdoch, J. (1997), 'Inhuman/Nonhuman/Human: Actor-Network Theory and the Prospects for a Nondualistic and Symmetrical Perspective on Nature and Society', *Environment and Planning D: Society and Space*, 15: 731–56.
- and S. Abram (1998), 'Defining the Limits of Community Governance', *Journal of Rural Studies*, 14 (1): 41–50.
- Okely, J. (1994), 'Vicarious and Sensory Knowledge of Chronology and Change', in K. Hastrup and P. Hervik (eds), *Social Experience and Anthropological Knowledge*, London: Routledge.

- Robertson, A.F. (1984), *The People and the State: An Anthropology of Planned Development*, Cambridge: Cambridge University Press.
- Sillitoe, P. and Bicker, A. (1998), 'Development's Demand for Indigenous Knowledge': workshop for the 5th Biennial EASA conference, Book of Abstracts, Frankfurt.
- Vike, H. (1996), 'Conquering the Unreal: Politics and Bureaucracy in a Norwegian Town', University of Oslo, Unpublished PhD thesis.
- Wright, S. (1994), 'Culture in Anthropology and Organizational Studies', in S. Wright (ed.), *Anthropology of Organizations*, London: Routledge.
- (ed.) (1994), *Anthropology of Organizations*, London: Routledge.
- (1995), 'Anthropology, Still the Uncomfortable Discipline?', in A. Ahmed and C. Shore (eds), *The Future of Anthropology*, London: Athlone Press.
- Yiftachel, O. (1997), 'Nation-Building or Ethnic Fragmentation? Frontier Settlement and Collective Identities in Israel', *Space and Polity*, 1(2): 149–69.
- Zonabend, F. (1984), *The Enduring Memory: Time and History in a French Village*, Manchester: Manchester University Press.

Understanding the Working Environment: Notes Towards a Rapid Organizational Analysis

Stella Mascarenhas-Keyes

Introduction

In recent years, anthropologists have increasingly elected or have had little choice but to obtain research or research-related work in a variety of organizational settings. Furthermore, as the sites of knowledge production and consumption have changed (Loder 1992), anthropologists will increasingly be located within organizations or funded by non-academic institutions which have their own agenda that constrains the autonomy of the researcher. Such research is invariably policy- and practice-oriented and, therefore, has to lead to recommendations for action. In order to carry out research successfully in such working environments, it is essential to understand the organizational context. I arrived at this view after a large research project called Computer Access, which I was co-ordinating, failed to achieve most of its aims because of wider problems within and between the multiple organizations involved in the project. I had not foreseen these problems partly due to lack of experience and appropriate academic training.

Prior to taking up this post, I had undertaken two research projects. The first one involved a sociological study using self-completion questionnaires of an urban population living in London. This was an independent study funded by a large, well-known, elite philanthropic organization whose role was restricted to financial oversight of the project. I undertook the study under the auspices of a small, ethnic community organization and, although I worked to a steering committee, I had a considerable degree of autonomy. My second piece of research was for my doctorate and involved fourteen months' overseas fieldwork as an autonomous researcher. I was funded by the then Social Sciences Research Council. While in the final stages of writing up the doctorate, I obtained the job of research co-ordinator of the Computer Access project in a large non-governmental organization (NGO). I was told that I was successful in the national competition because of my research background. However, as I came to realize, training and experience as an autonomous,

academic researcher does not sufficiently prepare one for working within a non-academic organization. The more one understands the organizational context in which one works, the more effective the outcomes, whether this is writing a proposal for funding, implementing a research project, or negotiating the take-up of recommendations arising from social research. A detailed ethnography of an organization is only necessary if that is the major aim of the research. However, if the research is focused on a particular topic or issue which is of interest to the organization in fulfilling its overall mission, then a rapid organizational analysis of the working environment is both necessary and sufficient.

In this chapter, I briefly describe the Computer Access project and then provide an organizational checklist which researchers may find useful in order to understand the context in which they are working. I illustrate how the checklist would have helped me by reference to the Computer Access project.

Case Study: The Computer Access Project

The project was the brainchild of a non-governmental organization which I shall call Voluntary Services Council (VSC) who successfully managed to obtain a grant of half a million pounds from a borough council. The three-year Computer Access project was focused on 'disadvantaged' people who were defined as black and ethnic minorities, women, and the disabled. It aimed to look at their existing access to training and employment in the computing field, to provide, on an experimental basis, different types of computer training and to make recommendations for increased and equitable access. VSC set up its own co-ordinating team at its headquarters which I was to manage. It was agreed that five voluntary organizations who worked with particular constituencies of 'disadvantaged' people should be selected to participate in the project. In order to choose these organizations, VSC held an open competition. It asked voluntary organizations to submit a research and development proposal which would give them substantial computer hardware and software and three staff. There was tremendous competition and eventually five organizations were chosen, each focusing on a specific 'disadvantaged' community as follows:

- Roots Trust (RT) whose constituency was Afro-Caribbean
- Women and Work (WAW) whose constituency was women
- Indian Organization UK (IOUK) whose constituency was South Asians
- Star Information Technology (SIT) whose constituency lived in a specific geographical area and which comprised people from a cross-section of ethnic backgrounds
- Disability Focus (DF) whose constituency was the disabled.

In addition, two computer companies were selected to provide equipment. The organizational chart for the project is given in Figure 10.1.

Each of the five organizations had a contract with VSC which disbursed funds at regular intervals to them. As the chief executive officer, I was responsible for the design and management of the project and worked to a steering committee. This committee comprised various members of VSC staff and council, as well as representatives from each of the five organizations. The central co-ordinating unit at VSC had three staff: myself, an assistant with expertise in computing, and an administrative worker. All the five projects also had the same complement of two professional and one administrative staff and a more or less equivalent skills constellation and background. I was the first person appointed and, subsequently, was involved in the recruitment and selection of all the remaining seventeen staff. The local staff were housed at the local NGO, and were jointly managed by myself and the director or a designate of the local NGO. The framework I had planned was that the research would have a general component that was common to all local projects, and a specific component which took account of local project specificities. The computer-training component I envisaged would be undertaken within an action research model and therefore would be research-based. The selection of computer hardware and the type of training courses set up would be based on research to identify constituency 'needs'. The training programme would run in phases and be evaluated after each phase, with the results being incorporated in the next phase.

My mistake was to focus solely on the research and development side of the project: what methods to use, how to identify sample populations, how to analyse the quantitative and qualitative data, what to do with the findings, etc. I had envisaged that the project would operate on the model of autonomous research, each researcher working separately but to a common timescale and plan within a larger framework. The model was naively premised on an autonomous researcher working

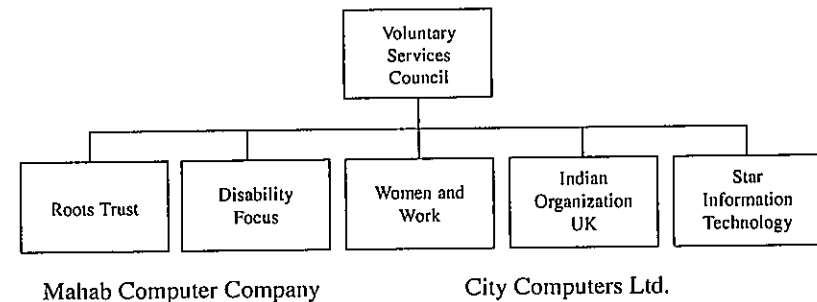


Figure 10.1 Computer Access Project

in a politically neutral context. I came to be increasingly frustrated by the personal, and inter-community and intra-community micro-politics, and the often irreconcilable, competing demands of the multiple organizations. I wrongly, and in hindsight arrogantly, thought that such behaviour was deviant and the fault of ignorant people who did not understand what constituted 'proper research'. In order to understand the institutional and political complexity and work effectively within such a dynamic context, I should have raised certain questions and undertaken a rapid organizational analysis using a checklist of the type I am now going to discuss.

Type of Organization: What Types of Organization are Involved in the Project?

There is considerable confusion generally as to what is meant by the label NGO. In its widest sense it includes: relief and welfare agencies, technical innovation organizations, public service contractors, popular development organizations, grassroots development organizations, advocacy groups and networks. Part of the problem is that the classification does not fully differentiate between the function, ownership, and scale of operation as part of a subcategorization of these organizations. As a result, everything from a neighbourhood organization concerned with better lighting through to an organization operating globally, such as Oxfam, is equally labelled NGO.

It may be helpful, therefore, to distinguish between two types of NGO:

- membership organizations staffed and elected by the people they are meant to serve and represent (such as farmer organizations or parent-teacher associations in schools).
- non-membership organizations staffed by people who are socially, professionally, and at times ethnically different from their clients.

In the VSC project, all the NGOs were non-membership organizations staffed by middle-class professionals. Some of them worked with membership organizations: for instance, WAW worked with a women's group – a membership organization – from a deprived housing estate.

Organizational Structure: What are the Structures of the Organizations I am working with and what is my Location within them?

Information about the structure of an organization may exist in publicity brochures. It will give some idea of the scope and complexity of the organization, and, for instance, whether there are specialist departments dealing with different activities (see Figures 10.2a-d). Role, task differentiation, chain of commands, and reporting arrangements vary considerably between organizations.

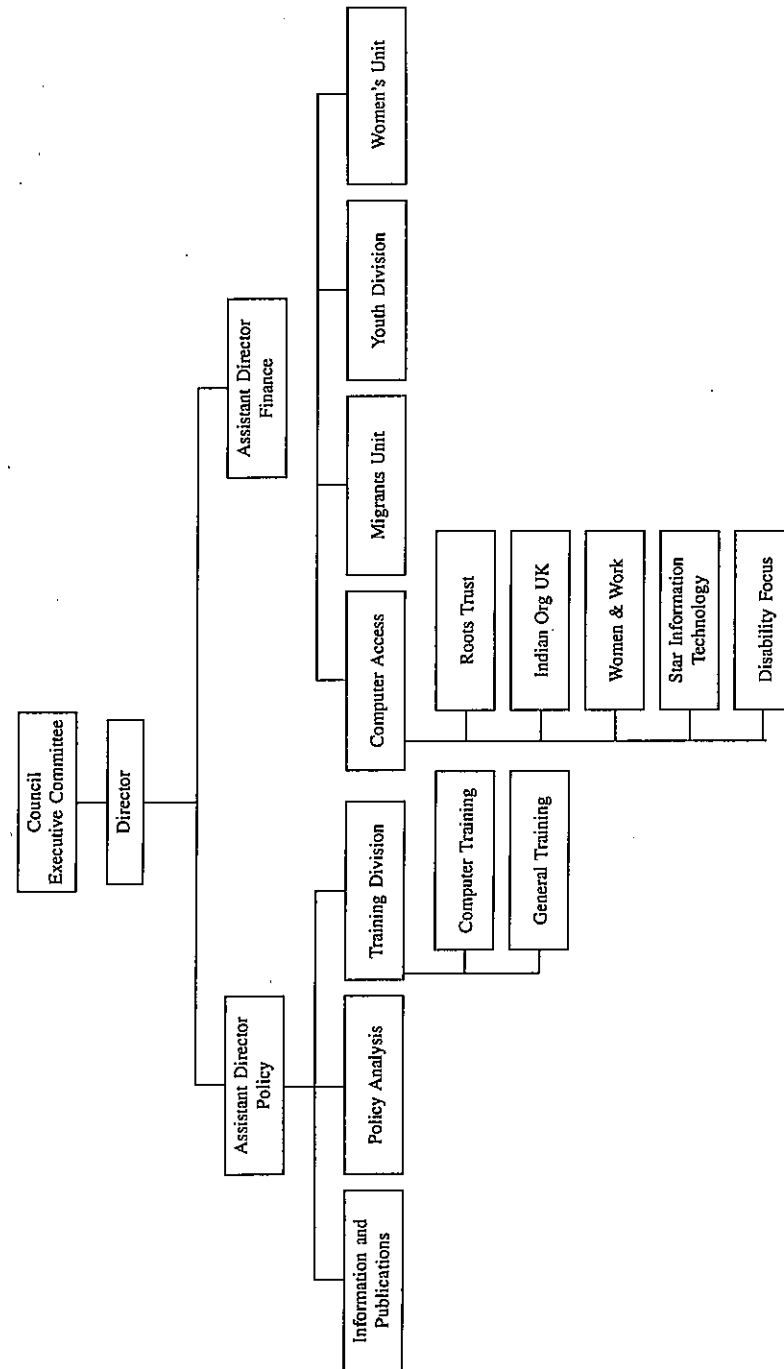


Figure 10.2a Voluntary Services Council: Organizational Structure

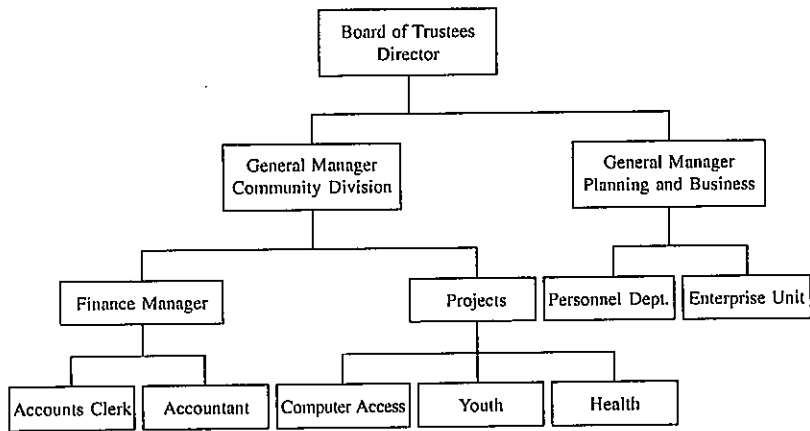


Figure 10.2b Roots Trust

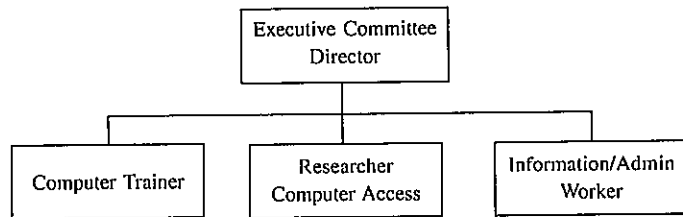


Figure 10.2c Indian Organization UK

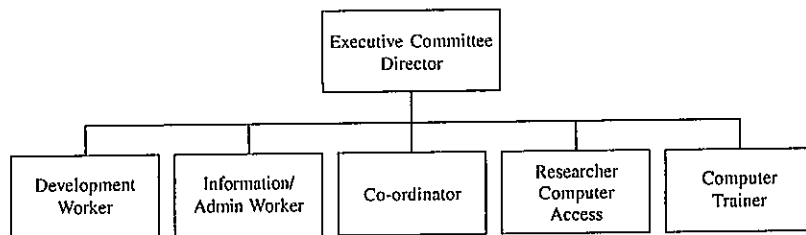


Figure 10.2d Women and Work

Thus, my unit within VSC was one of several constituent units each with a separate focus (see Figure 10.2a). As a member of VSC, I was expected to attend staff meetings and contribute to various organizational activities. At these meetings, I would give reports on my project and receive feedback from colleagues, many of whom did not have a research background. My line manager was not a social scientist nor had he undertaken research of any kind. He was the Assistant Director of VSC, responsible for finance. Out of the three members of the Directorate he was, at the time, the only one who had some knowledge of new computer technology and was assigned as my manager. He was motivated by economic rationality and therefore concerned with efficiency and effectiveness. Within a few weeks of my taking up the post, both the senior officer in the funding body responsible for the project and my own line manager at VSC asked for detailed bar charts with clearly identifiable performance indicators, such as number of interviews and training sessions which would be undertaken by specified dates. Regular research reports were required showing that there was progress in the project, and including recommendations for action even if these were based on circumstantial evidence rather than what I considered to be a solid body of systematically collected data. My manager tended to eschew social factors and issues of rigour and relevance of social research. His background was a direct contrast to that of the line manager I would have had as a researcher in an academic institution.

As the co-ordinator of the Computer Access project, my focus was almost exclusively on my project, whereas VSC and the local NGOs located the project within the context of the portfolio of projects and other activities for which each was responsible. For VSC, the Computer Access project was distinctive in three respects. First, it was the only research-based project amidst all the practice-based projects and activities handled by VSC. Second, it provided the first experience that VSC had of working simultaneously over a sustained period of time with multiple voluntary organizations. Third, the project had a very high status in VSC's portfolio of activities because its high level of funding had considerably exceeded the funding other projects had attracted, and it was hoped that it would be a precursor to further funding in the future. The combined result of the three factors was that the Computer Access project was perceived as VSC's 'flagship' – which placed considerable pressure on me and my team to deliver specific outcomes.

Within each of the local NGOs, the Computer Access project was differently located and its significance for the organization also varied. This meant that the interest in the project and its outcomes varied. Indeed, one NGO gave the impression that it was primarily interested in acquiring computer equipment to meet the other needs of the organization and only went along with the research and development objective because it was a vehicle for obtaining equipment.

Management Structure: How are Organizations Managed?

Various management styles were evident in the Computer Access project. Thus, RT had an autocratic management structure with individuals at each level of the hierarchy having specific decision-making powers; SIT had a participatory management structure requiring consultation with various people prior to decision-making; WAW was a collective, working on a consensus model. The implications of these diverse structures was evident in the research process, particularly when a questionnaire was being drafted. I sent out a draft to all the five organizations. RT and IOUK returned theirs with comments within a couple of weeks as their researchers had the authority to make decisions on content and format. SIT returned the questionnaire within a month, as its management style required the project researcher to consult not only the other team members in the Computer Access project but various colleagues and committees in the organization. When the comments were all received, the SIT researcher made the final decision. A similar approach was adopted by DF.

However, WAW consulted everyone in their organization. Their ideological approach did not allow for distinctions to be drawn between professional and white-collar workers. This was reflected in the fact that all staff, irrespective of qualifications, type of job, and level of responsibility, earned the same salary. Furthermore, they were not ideologically comfortable with the distinction between paid and unpaid staff and, therefore, sought to involve unpaid workers in decision-making processes as much as possible. Consequently, as many people as possible were asked to comment on the questionnaire and several meetings were held to reach a consensus. Not surprisingly, the time required was over two months. As my research model was that all projects would work with the same basic questionnaire, none of the other four projects could commence work until there was agreement within and between the five projects.

Funding and Accounting Systems: How are Financial Matters dealt with in an Organization?

An organization's annual accounts will indicate the size of its budget and the allocation of funds for different activities. It may also indicate the number of people involved in various financial tasks. The five NGOs involved in the Computer Access project all had different accounting systems and the significance of this was brought out in the purchase of equipment for computer training. Thus, RT had a separate finance section who were able to process the accounts for the purchase of computers in a short space of time. IOUK took considerably longer because financial matters were the responsibility of one staff member who was also in charge of a number of other tasks. Furthermore, he had to obtain authorization from the executive

committee who worked in a voluntary capacity and were not always immediately available to deal with financial matters (see Figure 10.2c). Consequently, all the organizations did not have the computer equipment at the same time, and therefore could not simultaneously begin the computer-training activities which were to be subject to monitoring, review, and evaluation in accordance with the agreed research protocol.

Human Resources: How Many People are Involved in the Organization?

The number of people working for the organization, in either a paid or an unpaid capacity, gives an indication of its size. Furthermore, job titles give a clue to how human resources are deployed in the organization. However, common titles, such as 'manager' are not necessarily comparable between and within organizations.

The autonomy and authority of individuals in an organization also varies. Thus, in WAW, where there was no concept of a 'line manager' or of staff with certain levels of authority, decisions were made by consensus. In DF and SIT, role boundaries were blurred under an ideology of equality, and authority was invoked only when disputes arose. At RT, where there was a hierarchical management structure, line managers had to be kept informed of decisions taken by staff.

The staff working in the Computer Access project were managed jointly by the local NGOs and the central unit at VSC. This combined management of human resources was put to the test when a dispute arose between the RT researcher and his line manager, and the aggrieved worker was suspended from work. He rallied the support of colleagues at RT as well as the support of all the Computer Access staff who were located in the different NGOs on the basis that they were employees in a common project. The senior management of VSC and RT also became involved as they were jointly responsible for payment of salaries. As the Computer Access project was concerned with equal opportunities, the RT researcher, who it later emerged belonged to a left-wing political activist group, used the rhetoric of equal opportunities to frame his grievances. This strategy helped to mobilize a large number of workers as he was seen to stand for 'higher ideals' and, therefore, was a worthy cause to support. During the dispute very little research was done by any of the project staff as they engaged in 'strike action' against the alleged hypocrisy of senior management supposedly committed to a project whose principles were being violated in practice.

Communication Systems: How do People Communicate within and between Organizations?

Hardcopy, oral, and electronic communications play various and different roles in organizations. In some organizations, there is an expectation that everything will

be in writing. In others, personal contact and oral communication – at both individual and group level – are more common. I began to realize that it is important to be aware of the way in which cross-cultural communication works: for instance, how people expect to be addressed; their preference for oral/written and electronic communication; norms about who should be kept informed, and whose views solicited; expectations about enquiries about their family, and other personal matters. It is a mistake to assume, as I found out, that if a report is sent to one person in an organization, it will be circulated to other relevant people who may have views or think they should be consulted. Often papers are not circulated and if a response is expected from a number of people in an organization, then copies should be sent to all. Failure to do so resulted in delays in the Computer Access project. In one case, a particular research approach had to be completely re-examined because one important individual on a committee claimed he had not been sent papers nor had his views been canvassed.

Expectations relating to the private and public domain are also significant. For instance, with IOUK, I always began telephone conversations with Indian staff with enquiries about their health, family, etc., as this provided a personal touch and demonstrated that I was thinking about an individual as a person rather than merely as a worker. On the other hand, the white British staff at SIT expected telephone calls to be restricted to business matters. While I do not wish to perpetuate stereotypes, it is important for researchers to be aware of varying cultural and sub-cultural norms regarding interpersonal communication.

Key Individuals: Who are the Key Actors in the Organization?

Key individuals may be the same as the names cited in the organization's literature. Alternatively, some people may be mere figureheads, and the power brokers not formally identified. It is important also to remember the gatekeepers, such as secretaries, who play an important role in facilitating or inhibiting access to those in positions of power. Key individuals may be identified by observing committee meetings and public events, and by listening to whose names are cited with greater frequency in informal conversations. It is important to cultivate key individuals because they may be important conduits to a network of contacts which will facilitate the research process and also play a role in the take-up of recommendations arising from research.

Power and Influence: What is the Basis for Power and Influence in the Organization?

At the risk of stating the obvious and for heuristic purposes, one may say that power is distributed in organizations in various ways and may emanate from:

- position: people in senior posts usually have more power;
- resources: the magnitude of resources under a person's control contributes to power;
- gender: women often have less power;
- race/ethnicity: non-white ethnic/racial minorities generally have less power. Expatriate white researchers/consultants, for example, often have, or behave as if they have, or are perceived to have, more power;
- personality: people with charisma often have more power.

It is important to pay attention to the 'official' or designated loci of power as well as to the capillaries of the organization through which power may be 'unofficially' dispersed. In the Computer Access project it was the honorary chair of the Trustees of RT, not the full-time paid Director, who had a great deal of power because he was a charismatic personality and because, by virtue of his position in the wider society, he had a network of influential contacts through which he was able to acquire public and private funds for community projects.

Intra-organizational Dynamics: How does an Organization Work in Practice?

The preceding points in the checklist will give some idea of the formal structure. However, organizational structure is the outcome of compromises and balances between conflicting but perhaps equally valid considerations. It is important to try to understand the balances and to work within them. It is worthwhile doing a rapid stakeholder analysis (see Mascarenhas-Keyes 1997: 173–83) in which primary and secondary stakeholders are identified, their interests, power, and influence ascertained, and their motives for participation in project activities explored. This should ideally be done at the beginning of the project and at regular intervals, as the dynamics of the situation rarely remain static.

Circumstances change fast and on-going awareness is needed of the internal and external political and economic climate in which research is being undertaken. It is important to understand and analyse the interests, the perspectives, and the constraints under which staff and other people connected with the organization work. Problems may be masked by individual personalities and styles of management. No organization is a monolith, however much it may be portrayed as such in its literature. Different and conflicting values may co-exist and the balance between these is probably constantly shifting. Power struggles, evident in confrontations and conflict, are inevitable in organizations. Goffman (1967) distinguishes between 'frontstage' and 'backstage' behaviour, and it is useful, to observe closely but unobtrusively, the backstage behaviour of individuals during informal gatherings

over lunch, in the pub, or at office parties. These often reveal the power struggles which may not be mentioned in open conversations but which can be discerned from subtle verbal and non-verbal inter-personal communication.

Awareness of power plays and ideological dispositions, and of the knock-on effect outside the organization, for example, is useful when judgements have to be made for selecting recommendations for action. For instance, I suggested that students from the anthropology department of a local university could be involved in conducting interviews and undertaking other aspects of data collection and analysis. I saw this strategy as being of mutual benefit to the project and the students. While some of the staff at VSC agreed with this, others saw it as a way of depriving unemployed people from paid work, and as the exploitation of student labour. The students and supervisors saw the strategy as a useful opportunity to gain practical research experience. However, because of various ideological resistances, the idea had to be discarded.

Inter-organizational Politics: How do Various Organizations Relate to Each Other?

There are many different kinds of organization. It is therefore important to consider inter-organizational politics and to seek a variety of perspectives on this. This may help to determine which organizations are likely to favour or resist recommendations. In the Computer Access project, VSC and all five voluntary organizations had a stake in the project but the stake kept changing over the duration of the project. Short-term cliques emerged when there was a common interest, and there were continuous alignments and realignments throughout the project.

This organizational complexity was further complicated by two factors. First, about a year into the project, the borough's funds and the projects it was responsible for were taken over by a residuary body (RB) which had a different political agenda and a different way of managing projects. RB tried, eventually unsuccessfully, to terminate the project on the grounds that VSC's contract with its predecessor was *ultra vires*. VSC took legal advice and I was involved in briefing solicitors and lobbying through speaking on local radio programmes. Second, in addition to the fierce competition among voluntary groups to be chosen to participate in the Computer Access project, there was a great deal of competition among a variety of commercial organizations to obtain contracts to supply the computer hardware. Two main contenders were Mahab Computer Company and City Computers Ltd. They were owned respectively by Asian and African businessmen who argued they should be chosen because the Computer Access project focused on 'disadvantaged' people.

Organizational Time Frame: What Concept of 'Time' does each Organization Work to?

Organizations concerned with policy and practice want research findings to be translated into practical outcomes. Furthermore, these outcomes, such as new training courses, development activities, policy guidelines, articles, and research reports are expected at regular intervals. This approach to research was at complete variance to the one I had been trained for: the research exercise is undertaken over a long period of time, is completed first and subsequently written up. If I had adopted this approach, nothing substantial would have been produced in the Computer Access project until some time in the second year. The concerns at VSC regarding outcomes were replicated at the local level. The project had a different strategic location in each local NGO whose time-frames also varied. This meant that my model of synchronous research activities between the projects was very difficult to implement and was naive in its conception. My academic training had taught me not to be premature with publications but to wait until the findings had been analysed and digested, but this time-frame means that tangible evidence of research activity is not visible on an on-going basis to the diverse consumers of the research. This led to the erroneous view among some potential consumers of the research that the project funds were contributing to the 'fat cats' lifestyle of a chosen few staff, while the 'real' beneficiaries – that is, 'disadvantaged' people – were not reaping the rewards of the research. In hindsight, a better strategy would have been to organize the research into a number of discrete, clearly focused but interrelated components, each to be undertaken in a short time-frame of two to three months with specific policy and practice outcomes to be disseminated in a series of reports and workshops accessible to diverse consumers.

Research Outcomes: How can I ensure that Recommendations for Action are taken up?

Any report based on research work submitted to the organization should be in a format that is easy to read and which contains the summary and recommendations at the beginning of the document (Mascarenhas-Keyes 1997: 236–9). Busy executives do not have the time or the interest to read through detailed academic arguments eventually leading to a conclusion at the end of a lengthy document. The first research report I wrote at VSC was in the format of an academic essay of 4,000 words. It was 'torn to shreds' by the management committee who had a number of agenda items to get through at the meeting and had no time for a 'long' academic paper. Therefore brevity, limited use of academic jargon, an easy-to-read style and well-focused recommendations for action are the hallmarks of a research report for non-academics.

It is important to find out which officer in an organization is accountable to senior management for one's project, and build up a shared understanding with that person. The location of that person in the organization also needs to be determined: for instance, which section is he or she in? What are the strengths and weaknesses of that section? Is it under threat? Where does one's project fit into the officer's personal vested interests and intra-organizational politics? To whom does the officer report? How are actual decisions made, and by whom?

It is useful to identify the key players and keep in touch with them to discuss their ideas, as a project will have a history of accumulated agendas, only some of which will appear on the project description and terms of reference. It is worthwhile to have regular meetings with key individuals to share the progress of the research so that there are no surprises at the end. In the meantime, the officers with knowledge of one's project will be able to lay the paths for the reception of the research when it is completed.

If at all possible, it is advantageous to talk through the research results with the people whose support is needed to get the recommendations accepted and acted upon. It is better to do this orally and informally before officials see anything of the final report on paper. This means that when the hard-copy version is submitted it confirms their expectations and they are ready to act on it. It is advisable never to give officials or politicians a shock by submitting the written report without a prior oral briefing session. They need to be familiar with the ideas before they see them written down. Furthermore, prior oral sessions enable one to test out orally which wordings are acceptable in order to communicate in a language that will not set up resistances.

Conclusion

It is useful to bear in mind three main points: first, the autonomy of the researcher; second, the purpose of the research; and third, the audience it addresses. These can best be illustrated by drawing on a contrast between my doctoral research and the work done at VSC. First, autonomy of the researcher: while researching and writing up the doctorate on a studentship, I was virtually my 'own boss' and free to pursue my own research interests. On the other hand, while working at VSC, my interests had to be balanced with those of my employers. Thus, there was a constant tension between reconciling my interests and views on how a research project should be run with that of the organization and the various constituencies that it served.

Second, the purpose of the research: my doctorate fitted in with the concept of 'pure' research in that it was a work of scholarship to extend the frontiers of knowledge for its own sake, and I could dictate the pace, theoretical and methodological approach, analysis and interpretation of data. In the Computer Access

project, on the other hand, the research fitted into the concept of 'applied research' in that the knowledge generated was to be used for policy and practice purposes. This had implications for the pace and scheduling of research activities. It required developing an awareness of the optimum moments within and outside the organization to channel the findings in order to promote changes in policy and practice.

Third, the audience for the research: with my doctorate, verbal remarks and written texts produced in a university setting were subject to comment from academics with a scholarly interest in the work. On the other hand, in the Computer Access project, the audience was very heterogeneous. It became necessary for me to cultivate an acute sensitivity to multiple interests and the often nuanced and subtle ways in which they were expressed. As I came to realize, my oral and written texts would be used by different sectors of the audience to drive their own agenda. If the texts did not meet individual requirements, then the strategy usually adopted was 'shoot the messenger'. The most common form this took was for criticisms to be voiced about the quality of the work. 'Quality' is a contestable concept. Whereas in a university environment there tends to be a fair amount of agreement about 'quality' and 'standards', outside the academy 'quality' is defined in multiple ways. Criticisms can be levied on a number of grounds, for instance personal vested interests; bias towards particular sections of the audience; questionable research methodology, usually related to whether informants constitute a representative sample; doubts about whether the data is genuine and reliable; and recommendations for action not being properly supported by 'real' evidence. In such situations, it is easy to be either completely dismissive of negative feedback or to become demoralized by such comments. While one develops resilience with experience, a strategy that I have found very useful, both when doing doctoral research as a native anthropologist, and when working in the Computer Access project and subsequently, is to adopt a reflexive stance. This means regarding Self as Informant (Mascarenhas-Keyes 1987), a method which involves constituting the Self as Other, and systematically and critically analysing feedback from other people and one's own responses to such comments. This approach has parallels with the psycho-analytical concepts of transference and counter-transference which Krause (1999) recommends that anthropologists may usefully adopt.

The strategy may reveal interesting information about the micro-politics operating within and between organizations, and may prompt one to exercise extra vigilance before engaging in oral and written communications. Furthermore, by subjecting one's own reactions to analysis, one is able to gain a deeper insight into how one's values and beliefs relating, for instance, to political ideologies, gender, sexuality, ethnicity, and the substantive content of one's research, all affect the way in which engagement in different aspects of the research process occurs. Thus the strategy both dispels anxiety and at the same time generates information which can be used for beneficial purposes in the research. Undertaking research and

research-related activities within and for different organizations during one's career is extremely challenging, both personally and professionally, but the reward is that one may contribute to improving the quality of life of some people.

Acknowledgements

This chapter is based on an actual project, but all the names have been changed to preserve confidentiality. Certain sections have appeared in Mascarenhas-Keyes (1997). I am grateful for contributions from Sue Wright and Teresa Creswell.

References

- Goffman, E. (1967), 'Where The Action Is', in E. Goffman, *Interaction Rituals: Essays on Face-to-Face Behavior*, Chicago: Aldine Publishing Co.
- Krause, I.-B. (1999), 'Learning How to Ask in Psychotherapy and Ethnography', in D.N. Gellner and E. Hirsch (eds), *The Ethnography of Organizations: A Reader*, London: Department of Human Sciences, Brunel University.
- Loder, C. (1992), *Support for Social Science Research: Setting the Scene*, Institute of Education, London University: Centre for Higher Education Studies.
- Mascarenhas-Keyes, S. (1987), 'The Native Anthropologist: Constraints and Strategies in Research', in A. Jackson (ed.), *Anthropology at Home*, London: Tavistock.
- (1997), *Professional Practice in Anthropology: A Curriculum Resource Manual for University Teachers*, Birmingham: National Network for Teaching and Learning Anthropology.

Part V

An Ethical Case Study

Participation or Observation? Some Practical and Ethical Dilemmas

Martin O'Neill

It is my intention in this chapter to outline some of the practical and ethical considerations that I have encountered while carrying out ethnographic fieldwork with a Welsh ambulance service. In doing so I hope that it will give rise to some wider considerations of the ethical position of the individual while conducting ethnographic enquiry. The purpose of this account of my experiences is that I believe, as Beales (1978) has argued, that although a formal code of ethics is of use to researchers, what is often more useful is the recounting of cases and their outcomes. Such accounts may be used as the basis of discussion, instruction, and assistance in the practical resolution of the ethnographer's dilemmas.

The fieldwork I describe here was the basis of my PhD (O'Neill 2000). When I first commenced my studies I was fresh from completing my first degree in anthropology and sociology and, as a diligent student, I had paid attention in my research-methods class during the lectures on participant observation. I had heard about Malinowski, Roy, Humphreys, and Bourdieu and felt that I had some understanding of the method. Later experience, however, was to teach me that although I may have had some grasp of the theoretical aspects of fieldwork, the practical implications of one's actions in the field were something that had to be learned on the hoof, as it were.

In order to understand the ethical dimensions of what I am to recount, a little personal history is necessary. Prior to going to university to study for my first degree I had worked in the ambulance service for some seven years. Therefore, within the environment in which I was conducting my research, I was known. Many of those who were to be the subjects of my research I had worked with 'on the road'. When I started out it was my aim to employ what one could call a classic qualitative approach, that is, unstructured and semi-structured interviews and observation of staff as they went about their daily routines. These were to be my main tools of enquiry. I approached the ambulance service with my research outline, detailing my approach and methodology, and they very kindly gave me free access to the organization.

As other studies of ambulance workers have identified (Hutchinson 1983), and as I knew from my own previous experience, ambulance work is characterized by high degrees of uncertainty and visibility. When an ambulance crew are dispatched to a call they have little idea of what they will actually encounter on arrival. Also, unlike the doctors or nurses at the hospital, they are unable to pull a screen around the patient as they administer treatment, often having to do so in the full public gaze. For crews to perform their duties and avoid negative consequences they are aware that they need, at the very least, to appear competent. Therefore, the Goffmanian (1967) concepts of 'front stage' and 'back stage' are particularly pertinent to the way that the crews cope with the uncertainty and visibility inherent in their duties. 'Back stage' behaviour is for the station or when they are alone on the ambulance; 'front stage' is when they are in the gaze of their audiences, be they the public or other healthcare or emergency workers. To do this successfully they have to project an identity as competent, which needs to be accepted by the audience. Aspects of this identity, particularly for the lay public, derive from the portrayal of the ambulance service and the 'paramedic' in popular media forms such as the television programmes *Casualty* and *999*.

In studies carried out in the USA, Mannon (1992), Metz (1981), Hutchinson (1983), and Douglas (1969) all found that management of the front stage was a central feature of ambulance work. My own research also supports this observation. From the individual members of staff who crew the ambulances on a day-to-day basis to the organizational displays of public scrutiny, such as Trust Board meetings, the ambulance service needs to appear competent at all times. They are aware that the consequences can be serious if they fail. For example, there was national public outcry after the failure of the London Ambulance Service control system on 26 October 1992, which resulted in questions being raised in the House of Commons and an NHS inquiry.

This culture of front management generates a procedure which Hutchinson (1983) has categorized as 'covering'. By this she is labelling that self-protective process which anticipates consequences, which staff themselves refer to as 'watching your back'. To avoid the possibility of personal and organizational negative consequences staff are socialized into this notion from their earliest induction training course. This, in turn, presents particular problems for a researcher in this type of setting. Douglas (1972) believes that parallels can be drawn between the study of ambulance workers and the study of deviant groups, as they construct such elaborate fronts to exclude the outsider. Indeed Metz believes that Douglas failed to get behind these fronts due to her ascribed categories of female and nurse. Hutchinson, another female nurse, also suffered problems in her research, in that the crews engaged in activities to intimidate and hinder her.

I was in a different position from that of any of the researchers so far mentioned. I had been, up to a few years previously, an insider. I still lived in the same town

where I had worked in the ambulance service and had remained in contact with many of my former colleagues as friends and neighbours. However, this relationship that I had with my 'subjects' led to different dilemmas. In relation to the construction of fronts, one of my informants said to me early on in the research process: 'Well, it's pointless trying to hide anything from you as you've seen it all in the past.' I also knew the strategies that crews employed to obfuscate the appearance of any feelings of uncertainty or fear on their part, as I had done it myself in the past. But, there were now subtle nuances at play concerning my own self-identity, both internal and external. Although I had returned to an organization within which I used to be a true insider, I now had a new modified identity. It had aspects of being an insider but it also had aspects of being an outsider. This was to lead to multi-faceted dynamics within the research process for which no amount of classroom education could fully prepare me. What I want to consider in the next part of this chapter is how my state of mind and understanding, of myself as participant and observer, evolved at the very beginning of the fieldwork.

In the first few weeks of entering the field I carried a very naive conceptual model in my head: that I was now no longer an ambulance man; that I was now in the role of the 'researcher'; and that my subject was the ambulance service. The subtleties of the fieldwork situation were to teach me that the position could never be that simple. Early on in the fieldwork it became obvious that I could not just observe: I had to participate. What was I to do if the ambulance that I was on arrived at the scene of an incident with casualties and people in a state of distress? Surely, according to the classic theoretical model I should remain a detached observer, but as a human being I could not just stand by and watch: I had to become involved.

There were a number of dimensions to this. From the staff's point of view they knew me and knew I could 'do the job'. If I did not get involved they would resent me, not only because I hadn't helped them out, but what would they have thought of me as a person if I had stood by when someone was suffering and taken notes or spoken into my tape machine? What would the public have thought of me in the same situation and also, what would I have thought of myself? Several incidents in my first few weeks of fieldwork made me consider my ethical and legal position as both a citizen and as a researcher. One incident in particular compounded these concerns and caused me to retreat from the field for a while to rethink my strategy.

The incident happened during one day shift when I was with an emergency medical service (EMS) paramedic crew. We had arrived at the hospital and the crew were waiting for a piece of equipment to transfer the patient onto a trolley for transfer to a clinic. As we waited in the corridor an ambulance liaison assistant came running after us and said that a five-hour-old baby had stopped breathing some two miles down the road. The paramedic ran to take the details of the call

from ambulance control, while the emergency medical technician (EMT) and I transferred the patient on to the trolley by hand and grabbed the stretcher.

Mannon (1992), in his study of emergency medical service ambulance crews in the United States, although acknowledging that it could be problematic, advocates that the participant observer in this situation should never let participation become so engrossing as to forget his or her analytic stance. On this occasion, in the heat of the moment, I completely forgot my 'analytic stance' and suspended my role as observer. It may be relevant here that Mannon had no background as a health-service worker.

The EMT and I ran through the corridors of the hospital toward the ambulance. I was in front, in civilian clothes, shouting at members of the public to get out of the way. When we got to the ambulance we threw the stretcher in the back. As various other studies of emergency medical workers have identified, incidents involving children elicit greater urgency, and on this call, all the stops were out. The journey to the incident was a very silent affair with none of the jokes or banter that often characterizes the journey to emergency calls. I felt sick; my heart was pumping and my mouth dry. The EMT and I prepared the equipment that we thought might be needed at scene. He, I, and the paramedic worked as a team of three; I was no longer 'the professional stranger' but was acting as a member of the ambulance crew. This may have been some sort of coping mechanism on my part to deal with the situation that I now found myself in.

On arrival at the scene of the incident, the EMT and I jumped from the vehicle and ran into the house, while the paramedic turned the ambulance around in case there was a need to depart quickly. Inside the house the EMT ran into the kitchen where the baby was, while I stayed near the door of the house, ready to run to the ambulance to get any equipment that he might need. As I stood there the grandparents of the baby came out of the kitchen, obviously very distressed, and I tried to calm them down. At this time the midwife also arrived and went into the kitchen. It was very tense for everyone concerned. The paramedic stayed near the vehicle with the doors open. All three of us knew from past experience that if it were not possible to get the baby breathing again quickly at scene, the other option was to run with the baby as fast as possible to the hospital, which was why we had adopted the positions we had. I stayed with the grandparents trying to reassure them. We all looked to the door of the kitchen expectantly. When the EMT did emerge he told us that the baby was breathing again. This was a source of great relief to us all. The paramedic and the EMT then went into the kitchen to attend to the baby with the midwife. I stayed outside comforting the grandparents. Obviously they were now a lot more relaxed and they asked me who I was and what was I doing there. We had just been through a very stressful time together, probably for them one of the most stressful events of their lives. When I explained that I was a researcher from Swansea University they seemed confused at first, perhaps because

I was one of the first people that they had seen arrive at the house. I then said that I had in the past worked in the ambulance service and this seemed to explain matters. Although the baby was now breathing, it was not breathing normally so it was conveyed to the hospital for a check-up. The journey back, although a lot less fraught, was still full of tension. The EMT and I sat in the back. The baby was continually monitored in case there was a need to start resuscitation.

When we arrived at the hospital the baby and mother were taken into Casualty. I sat in the back of the ambulance with the EMT and talked about the incident. By this time I had my researcher's hat back on and my head was full of ethical and legal concerns. What if I had dropped the patient when transferring from the stretcher to the trolley? In gaining access I had told nobody that I would be touching patients, as at that time it had not been my intention to do so. What if I had prepared a piece of equipment incorrectly en route to the call? When I worked in the ambulance service that had always been a concern, but now I didn't work for the ambulance service and felt that I should not have been doing such things. What right did I have to be in these people's homes observing them at times of intense emotion and grief? As far as the ambulance staff were concerned I had their informed consent but the patients had not invited me into their houses. What if the baby had stopped breathing in the back of the ambulance? I knew I would have got involved in the resuscitation, if I felt I could have helped. I knew the ambulance crew would have expected me to, and that if I had not, they would have been disgusted with me. But I was now supposed to be the researcher; I felt that I had no right to be attempting to resuscitate people's babies, probably the most precious thing in their life. As I sat there in the back of the ambulance sharing these rantings with the EMT he said something that made me put my concerns about research ethics back into perspective. He said, 'We worked as a team and who gives a fuck as long as the baby is all right?' I realized then that that said it all. Whatever my role or responsibility as a researcher these were secondary to my roles and responsibilities as a human being. If I could help, I had a duty to help, regardless of any arguments concerning research ethics; the ethical issues in this situation were a lot deeper.

This realization, however, had various implications for my research strategy. I had been given permission to observe ambulance crews, not to lay my hands on and treat patients. This raised new ethical problems regarding the informed consent of patients to be treated by a researcher. Until I resolved these issues I decided to retire from the field. I felt it was time to consider my ethical position reflexively and then to approach the medical research ethics committee of the health authority. Although the focus of my research was not the patients, events had shown that I couldn't help interacting with them on many levels. I consulted the literature of others who had been involved in this type of research to see if they had had to resolve such dilemmas. Although there were no similar studies from the UK, there were three studies that I could draw upon of emergency medical services in the

USA. As stated earlier, however, I felt that Mannon was in a different position, as he had no medical background. Metz (1981) also was in a different position, as he was employed as an EMT when he conducted his study, and therefore was in the position where it was his main role to provide medical care with the research role being secondary. The only researcher in a similar position to my own was Hutchinson (1983), as she had trained as a psychiatric nurse prior to conducting her research. However, her main concern in this area was how she should deal with what she believed to be bad practice in the treatment of psychiatric patients by ambulance crews.

In the end it turned out to be Metz who gave me the most guidance. He found that there were many times when being a participant on an ambulance got in the way of observing. The demands of the moment can prevent the researcher from casually surveying the surroundings, or reflecting on the behaviour seen. One's actions are being judged by others who are involved in the situation and the researcher's emotions come into play and influence judgement. What is central to any incident is that the patient's well-being is at stake; the focus necessarily is on that goal, because one is human (Metz 1981: 225). This I took in conjunction with the observation made by Reinharz (1984: 183) that if the researcher does not accept the members' perspective then he or she will appear indifferent, hostile, or ignorant and will be tolerated only briefly. These observations and my own previous experience helped me to consolidate in my own mind what role I had as a participant and what role I had as an observer.

Due to the area my research covered I had to attend two research ethics committees and both posed the same hypothetical question. What would I do if I arrived at the scene of an accident and there were three casualties? What extent of care would I give? What equipment from the ambulance would I use? My answer to such questions was that I would provide first aid as any citizen who had a knowledge of first aid would. I would not use any equipment from the ambulance unless directed to by a member of the ambulance crew. That is how I resolved the situation both for myself and to the ethics committee. I felt that I was no longer qualified to offer anything more, but due to the fact that I knew some first aid I was under an obligation to use those skills. Relating it to the position I recounted earlier, as a parent myself I felt if there was anyone who could help in keeping my child breathing, be they researcher or anything else, I would want them to do so.

Normally in the presentation of aspects about my research I do try to resist the gory or dramatic story, as I feel that these often contribute to the misrepresentation of what the ambulance service is about. However, on this occasion the purpose of using a dramatic story is that in this incident, what was of concern was very precious, a child's life. As mentioned already, however, I had begun to have these concerns for some time in my fieldwork, often while involved in more routine ambulance duties. I found I was in the position, as every ethnographer is, of being

involved in social relations with others who occupied the field, not only ambulance staff but patients, hospital workers, and members of the public.

This incident compounds a concern that I had during the whole research process and one that I feel that other researchers should consider. We, as researchers, are parasites on our subjects. As this incident illustrates, I was using a period of intense emotional trauma as data for my PhD thesis. This, in turn, poses both practical and ethical concerns that should be addressed. The question that researchers need to ask themselves, reflexively, is: What's in it for them? If we just stand back and observe for our own purposes then we are failing to address these concerns. Lofland (1971: 98) talks of the need for 'immediate reciprocities': the need for the researcher to make a contribution to the informal social network in order to be accepted and to be able to observe and record what people are doing. In other words, reciprocity is necessary in order to achieve one's practical ends. However, I would suggest that ethnographers, in considering such reciprocities with their 'subjects', should address the question of ethical reciprocity as well.

Over the years there has been much debate as to how much the anthropologist should get involved with his or her subjects; this is not, however, the main focus of this chapter. What I have recounted here is an ethical dilemma as to how much to get involved, which in turn is similar to the arguments that surround the question of anthropological advocacy. Ethical decisions made during ethnography are often problematic, as they are in everyday life. Akeroyd (1984: 154) has stated that in an 'increasingly pluralist discipline, consensus about ethical behaviour and research practice is unattainable and compromise seems inevitable. The social researcher must make compromises . . . between roles as scientist and citizen, commitment and impartiality . . . The onus for making such decisions rests on the individual researcher.' I do believe that my experiences in the field have shown this dichotomy between the role of the individual as a researcher and as a citizen. The experience has made me believe that it is essential that researchers consider the humanitarian implications of their actions in the fieldwork situation, although addressing this problem is rarely straightforward. All I feel that we as researchers can do is to try to resolve these ethical dilemmas as best we can. To do this we need to consider these decisions reflexively and discuss them openly, not only with our colleagues, but also in a wider arena. What I hope to illustrate by using this concrete example from my own research is that there are times, as Hammersley and Atkinson (1993: 286) have identified, where the researcher should stop being a researcher and engage in action that is not directed towards the goal of producing knowledge. By its very nature ethnography forces us into relationships with people: this in turn has an impact on how we behave; or, more germane to this case, actions arise through obligations in another role, in my case not only as a former ambulance worker, but I think more basically as a citizen and as a human being.

References

- Akeroyd, A.V. (1984), 'Ethics in Relation to Informants, The Profession and Governments', in R.F. Ellen (ed.), *Ethnographic Research: A Guide to General Conduct*, London: Academic Press.
- Beales, A. (1978), 'Ethical Dilemmas in Anthropological Fieldwork', in G.N. Appel (ed.), *Ethical Dilemmas in Anthropological Enquiry: A Case Book*, Waltham, Mass.: Crossroads Press.
- Douglas, D.J. (1969), 'Occupational and Therapeutic Contingencies of Ambulance Services in Metropolitan Areas', unpublished PhD thesis, University of California Davis.
- (1972), 'Managing Fronts in Observing Deviance', in J.D. Douglas (ed.), *Research on Deviance*, New York: Random House.
- Goffman, E. (1967), 'Where The Action Is', in E. Goffman *Interaction Rituals: Essays on Face to Face Behavior*, Chicago: Aldine Publishing Co.
- Hammersley, M., and Atkinson, P. (1993), *Ethnography: Principles in Practice*, London: Routledge.
- Humphreys, L. (1975), *Tearoom Trade: Impersonal Sex in Public Places*, New York: Aldine de Gruyter.
- Hutchinson, S.A. (1983), *Survival Practices of Rescue Workers: Hidden Dimensions of Watchful Readiness*, Washington: University Press of America.
- Lofland, J. (1971), *Analyzing Social Settings: A Guide to Qualitative Data and Analysis*, Belmont, California: Wadsworth.
- Mannon, J.M. (1992), *Emergency Encounters: EMTs and Their Work*, Boston, Mass.: Jones and Bartlett.
- Metz, D.L. (1981), *Running Hot: Structure and Stress in Ambulance Work*, Cambridge, Mass.: A.B.T. Books.
- O'Neill, M. (2000), 'Carriage with Compassion: An Ethnographic Study of a Welsh Ambulance Service', unpublished PhD, University of Wales, Swansea.
- Reinharz, S. (1984), *On Becoming A Social Scientist: From Survey Research and Participant Observation to Experimental Analysis*, New Brunswick: Transaction.
- Roy, D. (1973), 'Banana Time', in G. Salaman and K. Thompson (eds), *People and Organisations*, London: Longman.

Afterword

**Afterword:
Natives 'R' Us: Some Notes on the
Ethnography of Organizations**

John Van Maanen

This chapter is designed to wrap up this collection of readings by suggesting in broad terms what readers might take away from the preceding writings and what might be made of them all in terms of the promise and prospect of organizational ethnography. This is a field growing fairly rapidly in both North America and Europe and the work presented here provides a reasonable sample as to what anthropologists are up to in the area and thus tells us something about the current state of the art (and science) of organizational ethnography.¹ The chapters mix story and explanation with theory, critique, and advice but most tell, in some detail and with bite, about what it is like to be someone else. The natives of these tales are not, however, the alien and exotic others of faraway lands put forth in the classic monographs of cultural or social anthropology but, rather, oddly familiar domestic others who might be our cohorts at work, our next door neighbors or even, gasp, ourselves.

This shift in locale is at least partly a consequence of the relatively recent spread of the specialized and formerly insular disciplinary aims of anthropology (and, to a lesser degree, of sociology). Growing interest in the distinctly modern idea of culture – as something constructed both thick and thin by all self-identifying human groups – has put ethnography in play across the intellectual map. No longer is ethnography ordered by familiar categories that cut across times, places, and peoples; nor is it confined to single-site studies of supposedly isolated or at least

1. The Atlantic, it seems, separates rather than connects the organizational research worlds of Europe and English-speaking North America. Unlike organization studies in Europe, organization studies in the United States stands on its own as a rather autonomous field, complete with internal rifts and divisions between and among those who are called macro- and micro- organization theorists. It is a field that represents a growing assembly of rather diverse scholars frequently working in business schools and held together by convenience, will, and a few summer meetings. For better or worse, organizational research in Europe, notably in Britain and France, is tied more closely to reference disciplines, especially sociology and psychology. Useful recent discussions of these matters are found in Koza and Thoenig (1995), Child (1995), Chanlet (1994), and Hinings (1988).

conveniently restricted ways of life. With market globalization, enhanced communications, the rise and spread of vast human migrations, the increased ease of transport, and so forth, comes an inevitable and yet largely unprecedented – but closely observed and theorized – shuffling and interpenetrating of modes of thought and action as people, ideas, and goods move back and forth across the world.² Ethnographic studies reflect such breaching and blending processes, mixing parasitic and novel elements with traditional interests such that the trade is shaped as much by timely, topical, and pragmatic concerns as by disciplinary goals or theoretical worries.

This eclecticism is neither a scandal nor (necessarily) a strength. But, for better or worse, ethnography is now being carried on in a seemingly limitless number of organizational settings. In this volume alone, we have learned something about corporate worlds, lab science, museum work, medical practice, rural development projects, community planning and more. For most organizational researchers – few of whom claim anthropology as their reference discipline – ethnographic work of this type represents travel over a field of study. Travel is of course not enough (even for an anthropologist or, for that matter, a geographer).³ But, students of

2. Although I will not touch much on these matters, globalization refers to the apparent intensification of worldwide social relations which link distant localities in such a way that local happenings are shaped by events many miles away (and vice versa). In both anthropology and sociology, the idea of the whole, integrated, self-contained social group and way of life gives way to a flexible, open view of culture that operates in a continually changing environment. Clifford (1997), among others, argues that the world has changed and changed dramatically as a result of globalization. Culture (and therefore identity) is not longer stable but is now fluid and more or less consciously constructed. Ethnography must then offer a variety of discordant voices, never come to rest, and never 'essentialize' (a favorite term of abuse) a people or a culture. Just how globalization is playing out in different parts of the world is of increasing interest to anthropologists generally and to organizational ethnographers specifically. Examples of how various aspects of globalization might be treated ethnographically include Appadurai (1997), Passaro (1997), J. Watson (1997), Hannerz (1992), Kahn (1995), Zabusky (1995), Lamphere (1992), Fox (1991), and Featherstone (1990).

3. I do recognize that a prestige of place has long been associated with anthropological work. What Gupta and Ferguson (1997) refer to as a 'hierarchy of purity' still obtains within the trade such that some field sites are taken as more anthropological than others. The question 'Where are you going to do your research?' remains an important one in anthropology and the reply 'across the street' is not likely on its own to generate an enthusiastic response from colleagues. Fieldwork at home is sometimes seen as an avocation – akin to tending one's garden – rather than a vocation and some notable figures in anthropology have gone out of their way in the past to stigmatize the practice. Geertz (1995: 102) for example remarks that ethnography in one's own society was once known derisively as 'gas station anthropology'. This has of course created a good deal of opportunity for the ethnographically inclined without proper anthropological credentials – myself among them – to contribute to various interdisciplinary fields such as organizational studies. Anthropologists have never been totally absent from the field, of course, but their numbers have been few and the status of their at-home work within the discipline rather suspect. This is changing as the ethnographic literature associated with organizational life grows.

organization at home or away who have never – even vicariously through reading – been exposed to everyday work in a clinic, a business firm, a research lab, a divorce court, or a civil service agency have probably missed something. Their generalizations about organizations are quite apt to be based on far too narrow a selection of the field. Taking readers to places where they presumably have never been is then very much a part of organizational ethnography as currently practised and understood.

Yet, as anthropologists know only too well, the burden of ethnography – to represent culture, fragmented or whole, at home or away – has over the past several decades become heavier, messier, and less easily tied to a specific set of analytic categories and narrative conventions. To suggest in writing what it is like to be someone else has of course never been a simple matter, but the task appears almost Herculean these days given the problematic of personal and social identity associated with the conditions we so nervously call postmodern. Moreover, the theories, practices, and rhetorics associated with ethnography continue to evolve (or, at least, shift) in recognizable and presumably irreversible ways. Although I have no cranky epistemological fits to throw on these pages, I suspect that most of us would now agree, for example, that all ethnographies owe whatever persuasive power they can muster to contingent social, historical, and institutional forms and no meta-argument can question this contingency.

But, as the dust settles on this ethnography-as-text argument, most of us are also coming to realize that if we are to continue to do our work, this sublime contingency – and other similar ones put forth by the annoying but necessary epistemological hypochondriacs among us – matters very little since any particular ethnography must still make its points by roughly the same means that were available before the contingency was recognized. These means include the hard work of securing access to study sites (by whatever sneak means possible), becoming saturated with first-hand knowledge of the research setting, gathering and putting forth evidence, providing fresh interpretations, inventing and elaborating analogies (crisp or otherwise), invoking authorities, working through examples, calling out theories, and, in general, marshaling the tropes such that attractive work is produced. This is simply to say that while the nature of ethnographic work, evidence, explanation, metaphor, authority, example, and rhetoric may shift over time, the appeal of any single work remains tied to the specific arguments made within a particular text and referenced to a specific, not general, substantive domain, analytic approach, and narrative style.

The work of ethnography, then, goes on in much the same way as it did before textuality came into fashion. To some, perhaps, the discourse surrounding the textuality of ethnographic facts and the factuality of ethnographic texts is paradoxical (discomfortingly so). But I think the paradox vanishes with the realization that ethnography – like any other form of social research – does not soldier on because

its facts, methods, genres, theories, and so forth all survive the passage of time (and, most assuredly, they do not) but because in the midst of change an audience still looks to it for the performance of a given task. In the case of ethnography, what we continue to look for is the close study of culture as continuously worked out and lived by particular people, in particular places, doing particular things at particular times.⁴

This mandate has, if anything, grown in recent years. Cultural self-consciousness is on the rise and everyone, it seems, now has a culture well worth presenting if not defending (or challenging) – including, for instance, nation-building groups of Hawaiians or Québécois, dedicated corporate tribes of Motorolans or Decies, and spreadsheet warriors of Anderson Consulting or the London Business School.⁴ Such worlds of meaning and apparent solidarity are not only felt but increasingly inscribed in both popular and scholarly forms. Cultural insiders create engaging lay ethnographies such as Lewis's (1990) treatment of Wall Street traders in *Liar's Poker* while cultural outsiders write scholarly ethnographies such as Albolafia's (1998) examination of these same creatures from Planet Finance in *Making Markets*. The ethnographic mandate has spread well beyond any neatly ordered disciplinary borders and now accommodates a rather astonishing amount of topical, theoretical, and stylistic diversity thus serving to question if not subvert what Malcolm Chapman, in Chapter One, rightly calls the 'vain hope' associated with intendedly authoritative definitions and portraits of culture. While the anthropological market share in the cultural portrait business may be down, the market itself is booming.

For ethnographers trained in either (or both) anthropology or sociology, the rise of interest in culture ever so broadly defined means that more subject matter is opened up, more attention is paid to neglected or marginalized areas of social life at home and away, more opportunities are created to cross traditional disciplinary and topical boundaries, more play with forms of reporting is possible, and more ways to frame research questions are available today than has been the case since Ur-anthropologists like Bronislaw Malinowski took up residence in the Trobriands or W. Lloyd Warner landed a job on the Hawthorne Studies research team at the Western Electric plant in Cicero, Illinois. Moreover, the audience for ethnographic narrative and the pleasure, puzzlement, and provocation such writings provide continues to grow in various applied and multidisciplinary fields such as

⁴ This spread of culture shows up occasionally in the most unlikely places. For example, Milton Friedman, the patron saint of free-marketeers and hard-line economic rationalists, when asked by a reporter why he thought the University of Chicago economics department had been so productive and prominent over the years, responded without a trace of irony that it was not the hardy individualists who were attracted to the department nor the rewards put in place to motivate them but rather, in his words, 'the culture of the place' that was responsible for the group's success (*The Economist*, 1996: 72).

education, law, communications, management, social work, criminal justice, and public policy. I think it fairly safe to say that there are now fewer rules for ethnographers to follow but more work to do. This seems to me a far better world for practising or wannabe ethnographers than one with less work and more rules.

There are of course problems. Not the least of which surrounds what kind of studies we are comfortable calling ethnographic and just how we are to assess their quality. Such matters appear to be very much on the minds of the contributors to this volume. All recognize the distance they have traveled from the so-called cultural island perspective and the one-site, one-tribe, one-scribe conceits associated with such work. All recognize that they have cut back on the rather timeless culture concept to fit a restless, shifting, multicultural world populated by natives not so very different from themselves. All recognize the tensions that arise as research sponsors unfamiliar (or perhaps too familiar) with ethnographic research clamor for timely and useful results. And all recognize that the unblinking ethnographic eye celebrated in Latour's (1993) much-mentioned call for 'symmetric anthropology' is not so easy to bring off in those highly segmented worlds of self-interested organizational actors who not only can read the reports of research conducted on their turf but may actually wish to do so (with the exception perhaps of some of the randy managers studied by Alex Ouroussoff in Chapter Two).⁵

Yet grumbling about the difficulty of doing ethnography while doing it is a rather well-established narrative ploy (Van Maanen 1995). We should not make too much of it for it signals a rather high and I think welcome degree of self-awareness on the part of ethnographers who realize at the outset of their work that they have already acquired at least some, if not a good deal, of the culture they are going to the field to study. It also reflects the troubles that come with working in league with sometimes remarkably patient but understandably chary and occasionally resistant informants who know quite well that ethnographers as pesky social researchers are out to interpret what they say and do in ways they might not find agreeable.

How ethnographic work is understood and (sometimes) read by various audiences is a matter on which all authors in this collection have something to say. In Chapter One, for example, Malcolm Chapman, wanting to overcome the 'lingering primitivism' his new colleagues in a management school ascribe to

⁵ A handy little book edited by Caroline Brettell called *When They Read What We Write* (1993) might well be placed in the ethnographer's backpack (or laptop bag). Ethnographic authority of the classical sort thrived in part under a cloak of distance and difference. The natives never knew what was written about them. Today they do, and we write more circumspectly as a result or risk being upbraided and run out of town. To wit, 'natives' have become 'members' or sometimes 'clients'. And 'primitives', 'savages', and 'subjects' are thankfully long gone, as well maybe as 'informants' (an increasingly ugly and troublesome label for those who knowingly aid us in our studies).

anthropology, goes well out of his way to tutor – with apparent success – a skeptical colleague trained in the dismal art of economics. David Mosse in Chapter Eight bluntly suggests that the more independent, critical, and unrestricted the work of the ethnographer in the field, the less legitimacy and practicality the work is likely to have for those who make the study possible.

More generally, it seems that readers outside anthropology, sociology, and the relatively small ethnographic circles that form on the periphery of other social science disciplines and professional pursuits are rather accepting of the so-called ethnographic facts. These same readers are, however, sometimes suspicious of ethnographic interpretations which they read as intentionally subversive, confused by empyrean clouds of theoretical vapor, or, most crushing of all, too obvious for words. For a good number of readers outside the field a trustworthy ethnography is a straightforward realist tale focusing largely on what people say and do as witnessed by the author who displays the expected courtesy of staying out of the way when the ethnographic facts are neatly bundled up in writing. No more, no less. No spin. Thank you very much.

Such response is tied partly to the sacralization of culture that I think goes with the concept's popularization at large as well as what we might call the cult of authenticity associated with heightened cultural sensitivity and awareness. A broad misunderstanding of ethnography results and anything other than the native's point of view as put forward by the native – in Geertz's (1983: 17) memorable phrase, 'an ethnography of witchcraft by a witch' – is read as less than the real thing. To wit, the enormous popular appeal of Studs Terkel's tape-recorded (albeit extensively edited) interviews such as *Working* (1974) and *The Good War* (1984) or Tracy Kidder's hanging-out form of literary journalism in *The Soul of the New Machine* (1981) and *Among Schoolchildren* (1990). Many if not most readers take writings of this sort as indisputably authentic cultural accounts devoid of insidious interpretation. While there are certainly many good reasons to call into question various ethnographic narratives, authenticity is rarely one of them.

Whatever else it may be, ethnography is an interpretation. It is something added to all the wondrous facts that are collected or stumbled on during a period of fieldwork. The purposes of ethnography do not normally spring from the settings in which fieldwork is conducted but come (and leave) with the ethnographer. In Chapter Six, for instance, Simon Pulman-Jones tells us that while he is not the only social theorist in the children's behavioral clinic he studied, he is the only one for whom the theories of others serve as data rather than competition for his own. His work entails the interpretation of interpretation and his aims in the setting sharply differ from those of his temporary colleagues however convenient it may be for all to forget such matters for a time.

It helps to keep in mind that we never observe or study a culture directly since the term 'culture' does not denote any concrete reality. It is an abstraction and, as

commonly used; a vague – but still useful – abstraction.⁶ Culture is to ethnographers as life is to biologists, force is to physicists, or God is to theologians. We learn of the customary talk and behavior that goes on among those we study and then artfully categorize what we've heard, seen, and felt according to our own interests, training, skills, and insights. To be sure, we rely heavily on others (both in and out of the studied settings) but, in the end, it is the ethnographer and not the native who develops and takes responsibility for whatever cultural accounts and representations mark the study. To position one's self at an angle, to provide a 'cross-eyed' vision and independent voice, is of course what we count on and expect ethnographers to provide as an intrinsic component of their work. It is also what makes ethnography a good deal more complicated than simply a method of collecting data.

Let me now turn to classification and briefly delineate some – although surely not all – of the substantive interests followed by those intrepid ethnographers of organizations including most of those represented in this book. What do they study? What problems seem to focus their research? Where does their work fit into what is known (or thought to be known) about organizational life? These are of course big questions I am not about to fully answer. But some inkling where this once and future field has been and is going comes from examining topical domains.

Four areas of inquiry are I think sufficiently well established – indeed prominent – so that a crude picture of field can be drawn despite considerable theoretical and stylistic variation that exists within each domain. I must, however, add a proviso that no one study fits a single category perfectly. Ethnographers are an integrative and ambitious lot and ethnographic holism has not retreated entirely or lost all of its appeal. Nonetheless I do claim that most ethnographic studies of organizations

6. I realize that some influential modern work on culture suggests that we might be best off avoiding the hyper-referential word altogether and write more specifically about knowledge, practice, tradition, technology, discourse, ideology, or habitus. There is much to be said for such a tactic. It may dispel the totalitarian overtones and perverse idealism that sometimes surround the use of the term 'culture'. But substituting alternative concepts merely defers, and does not solve, the analytic difficulties. Kuper (1999) suggests that such difficulties become most acute when 'culture' or any other stand-in term shifts from something to be represented, interpreted, or even explained to a source of stand-alone explanation – beyond established social institutions and political or economic conditions – for why people think and act as they do. As suggested by some of the contributors to this volume (e.g. Bob Simpson in Chapter Five and Simone Abram in Chapter Nine), ethnography sometimes suffers from a myopia that sharply delineates behavior at close range while obscuring the proximate and less visible structures and processes that engender and sustain behavior. A fetish with fieldwork may deny the legitimacy of social observation beyond the tête-à-tête of interpersonal interaction. Other sources of information exist and the best – and most responsible – ethnographies are perhaps always interdisciplinary. This ethnographic broadening is effectively preached and demonstrated by Nash (1993), di Leonardo (1998), and, most recently, Burawoy and Verdery (1999).

are sufficiently focused individually but varied enough collectively that the following taxonomy is neither pointless nor pat.⁷

Studies of Organizational Processes and Informal Relations

Fieldwork of the ethnographic sort is intended to be largely unobtrusive and, at least at the time it is carried out, non-reactive. Ethnographers listen, observe, participate, converse, lurk, collaborate, count, classify, learn, help, read, reflect, and – with luck – appreciate and understand what goes on (and maybe why) in the social worlds they have penetrated. It is an unspoken methodological paradigm that is generally effective in not scaring away the phenomenon of interest and is more or less at odds with the no-nonsense discovery of truth by experimentation, survey, or highly-formatted call and respond interviews, techniques which most fieldworkers believe ignore context and create reactions. Preserving the apparent naturalness and everyday character of what is being studied is the stock and trade of ethnographic work on the ground (and in writing).

Nothing puts these precepts and trade skills to test more than the study of organizational processes and informal relations. Such processes and relations concern the way members of organizations individually and collectively come to terms and cope with, but seldom solve, the recurrent problems and contradictions they face when going about their daily tasks. Research interest might center on how work is patterned, decisions made, control exercised (or thwarted), or autonomy carved out. Organizational processes come in a variety of cultural forms including routines, rituals, dramas, and games.

7. There are of course many other useful ways to categorize organization studies including classification by type of organization (business, science, public service, and development) as put forth in this book. I am drawn to the topical order because I have used it once before to classify a somewhat broader sampling of research in the field (Van Maanen 1999). It also allows me to shake and bake the previous chapters in a different way than editorially arranged. I realize of course that some quite interesting ethnographic work is going on at the edges and between my categories. Worth mentioning in this regard are ethnographic studies examining narrative in organizations (e.g. Czarniawska 1997; Ewick and Silbey 1995), the impact of gendered expectations in the workplace (Garson 1975), the role of emotion work in organizations (e.g. Leidner 1993; Hochschild 1983), and the time and place norms associated with work (and non-work) activity (e.g. Nippert-Eng 1995; Perlow 1997). It is also worth noting that what might be called the ethnography of amusement or pleasure (and of the entertainment, leisure, and tourist organizations that make up this enormous industry) is a growing area of study (e.g. Fjellman 1992; Davis 1997) as is the cultural response to cultural products and the organizations that produce them (Radway 1997; Lutz and Collier 1992). While this is no place for a literature review, my point is simply that ethnography of the worthy sort David Gellner and Eric Hirsch outline in their introductory remarks to this book has become of central feature in organizational studies (a cross-disciplinary area that is itself expanding).

Informal relations refer to the so-called 'underside' of organizational life – those adaptive but sometimes hidden and unofficial arrangements by which things get accomplished (or ignored). Some are tightly scripted, rather predictable, and governed by well-established social rules and cognitive schemes. Some are not. Most probably fall somewhere in the middle, identifiable as human ways of fleshing out the skeletal, formal chains of command and task structures of an organization by distributing various levels of responsibility, trust, fealty, and interpersonal control among members – workers, supervisors, customers, clients, managers, and so forth. Both organizational processes and informal relations are tractable by a logic embedded in what people are trying to do but they are also forever and always subject to considerable situationally specific variation.

Perhaps the most well-regarded and well-known study of such matters – in the United States at least – is Melville Dalton's (1959) classic *Men Who Manage*. Seeking 'to get as close as possible to the world of managers and to interpret this world and its problems from the inside' (p. 1), Dalton noted that the gaps between prescribed and observed behavior and the competitive relations among individuals and groups aspiring to power in the organization were not simply fleeting departures from standard operating procedures but patterned, predictable, and potent ways of doing things together which sometimes supported official organizational goals and sometimes did not. His findings, however, could only have derived from a method able to track the subtleties in both meaning and action as they emerged on the ground.

The processes that caught Dalton's attention – decision-making, communication, cooperation (or lack thereof) – and the informal relations that characterized the performance of various managerial tasks are of course quite general. Indeed, his findings and approach have since vividly informed a number of other organizational ethnographies such as Kanter's (1977) study of gender relations in the workplace and Jackall's (1988) justly acclaimed portrait of the relativist bureaucratic ethic that governs managerial choice in large business firms. While studies in this domain are typically focused on contextualized, specific, and most often unarticulated local practices and rationalities as displayed by members of relatively small and demarcated organizational units or groups – of clerks, machine operators, police officers, doctors, and so on (and on) – the results are cast in highly general terms subject only to the potential ethnographic veto arising from a parallel future study.

The field guide for studies of this sort – providing a set of analytic categories and a theoretical rationale – is Erving Goffman's *The Presentation of Self in Everyday Life* (first published in 1956). The empirical center of the book is a partial community ethnography derived from Goffman's dissertation research, supervised by W. Lloyd Warner, in the Shetland Islands. But it is well worth keeping in mind that Goffman also thought of the work as something of a handbook for examining organizational behavior – '(for studying) behavior that goes on within the confines

of a building or plant' (p. xi). The sensitizing notions conveyed in the work – such as front- and back-stage regions, impression management, and teamwork – have led to a lasting image of an organization as built up and sustained by the ritualized performances of organizational members. Goffman went on to do fieldwork in mental hospitals and publish *Asylums* (1961), a monograph that evokes the everyday grit and grind of organization (under)life like no other.

In this volume, the work of Sharon Macdonald (Chapter Four) and Simon Pulman-Jones (Chapter Six) neatly fit my organizational processes and informal relations category. Both studies focus on work groups in a single organization. Both closely examine the on-going negotiation and facework that routinely occur in these settings as tasks are situationally defined and carried out. Macdonald looks to the way new exhibits are developed in a science museum and finds not only a good deal of indeterminacy built into the process – thus effectively blunting some of the critical if not paranoid intellectual discourse that surrounds cultural production and museum work – but also suggests that the hedge on change and creativity in the museum stems from members who understandably fall back on certain long-standing institutional routines and recipes when in a pinch. A kind of organizational regression takes place as staff members charged with coming up with an innovative and client-friendly exhibit lose their enthusiasm for certain ideas and approaches as resource constraints, management oversight, and rather traditional assumptions surrounding the 'accessibility and clarity' of museum displays come into play.

Pulman-Jones also concentrates on routine interaction and teamwork. The interest here is on the labeling, evaluating, explaining, and, when possible, helping of children with chronic behavioral problems. As the sadly inconclusive story of Gemma makes clear, the clinic is a not-so-total institution whose staff struggles to meet a mixed, sometimes contradictory, set of organizational goals. Staff members are segmented however, assigned membership to either the core team or the psychiatric team, and the differences between the two animate the clinic. While Pulman-Jones notes that the two groups are in theory complementary, each doing what the other cannot, in practice the two are regularly at odds and, in the end, the work of the clinic suffers and its place if not survival in the larger social-service world is left in doubt.

Both studies examine first-hand the political and social worlds of quasi-professional work and focus on the gaps between official and unofficial practice as well as on the relations between groups vying for influence within the organization. The activity supported by informal relations does not necessarily resist or oppose what is organizationally prescribed – at times it may even buttress official procedures – but it flows from a local logic of institutionally segmented and stratified groups trying to accomplish what they regard as their real work. Laying out such logic and the constraints that seem to keep it in place is what studies of

organizational processes and informal relations do best. Studies of this sort can easily make a quota of formal generalizing remarks sufficient to satisfy all but the most obsessed of theory fans. But what is perhaps most memorable and valuable is not the analytic work or the bold generalizing but the particularizing as authors report on specific episodes that serve as small epiphanies to readers. The human logic attached to organizational processes is thus made apparent, rather vivid and concrete, allowing a degree of ethnographic charity to be granted to those whose behavior might otherwise appear as odd, alien, lazy, malicious, conformist, mendacious, uninformed, or simply inexplicable.

Studies of Organizational Identity and Change

The roaring success of the American comic strip Dilbert reminds us of the mocking contempt many employees have for their bosses and the popular management theories they import willy-nilly into the workplace. Such theories often come christened with silly acronyms, tarted-up in fancy scientific language, and, so fast do they rise and fall, written in disappearing ink. Much high-flying and mass-marketed management theory is thin gruel indeed. Full of vapid rationalization and superficial fad, more than a few best-selling management books offer little more than an intellectual, if not ideological, justification for the foolhardy, flavor-of-the-month notions top executives rain down on lower- and mid-level employees. Airport bookstores are today stuffed with organizational salvation texts preaching the latest gospel of business process re-engineering, downsizing (or, as some say, rightsizing), knowledge-based organizational design, economic value-added analysis (EVA), and (fading fast) total quality management.

Nowhere is the penchant for fad and folderol more prominent than in the discourse surrounding organizational identity and change. The popularity of advice tracts and change manifestos seems to reflect an acute, widespread, and apparently pressing need among managers for guidance as venerable cultural patterns associated with organizational life unravel with innovations in technology, alterations in governance structures, the decline of bureaucracy as a model of organizational form, and the internationalization of markets and communications (e.g. Friedman 1999; Handy 1996). Stability, it seems, has given way to variability, and it should be no surprise that top executives trapped in unsettled times are on the lookout for ways to swiftly redirect and alter the character of organizations over which they assume responsibility and for which they are increasingly held accountable. Yet where heads of state agencies or chief executive officers (and their consultants) get their Masters-of-the-Universe ideas probably matters less than the consequences of putting such ideas into play. On this matter, a good deal of scholarly ink has been spilled.

Studies of organizational identity and change are often – perhaps most often – ethnographic in character. Because symbolic meaning and unfolding history are critical to any account of collective identity, there is perhaps no other substantive area for which ethnography is more suitable as a method of study. While there are a variety of theories floating about to explain identity and change, few can claim much predictive success and most are largely descriptive, basing their claims on the interpretation of a carefully laid-out case or set of cases. Narrative is the name of the game and students working this domain increasingly draw on methods and styles originating in the humanities and arts as well as anthropology and the social sciences. Over the years, the writing about organizational identity and change seems to have avoided much (but not all) of the mischief engendered by the sort of runaway positivism of variable analysis and hypothesis testing that afflicted organizational studies more broadly. At any rate, when well done, studies of organizational identity and change provide an elegant mix of representation and interpretation as authors balance the interaction of events, groups, ideas, institutions with the intentions and feelings of human actors and do so in a historically sensitive fashion.

Such studies inevitably involve the notion of organization culture(s) and this creates something of a problem for ethnographers. Culture is now, as noted earlier, a rather well-worn accounting device, available as an explanatory mechanism not only to ethnographers but to organizational members (and their many professionally interested observers such as consultants, reporters, stock analysts, social critics, politicians, competitors, prospective employees, and so on). It can be and is used to explain if not justify such matters as sterling or poor organizational performance, the success or failure of mergers and acquisitions, or the presence or absence of harmony among members. In some organizations, upper-level managers studiously work up explicit cultural values that are trickled down to the ordinary folks in the operational trenches who are presumed to then take their social and behavioral cues from their betters.

This consciousness of culture may even be promoted by an officially sanctioned (and budgeted) cultural engineering department charged with making sure the organization is a happy and productive one. Culture can thus be codified, posted to a wall, and cheerfully presented to an ethnographer as a way to ease the burden of organizational study. Home-grown culture claims are usually far more inchoate, contested, and ambiguous than these terse examples suggest, but cultural descriptions – whether appearing on the back of business cards or emanating from boozy lunchtime encounters – are quite common these days. Such raw material serves of course as an ingredient for a cooked ethnography (of the sort represented in this book) and thus represents yet another symbolic domain to be contextualized and interpreted by the ethnographer.

It is worth noting, however, that organizational members who give such an honored place to culture are themselves helping to displace the loony but

surprisingly successful modern idea that organizational life is dominated solely by a secular, rational, matter-of-fact, means-ends logic and is therefore beyond or above culture – an economic notion that if true would allow us to dismiss matters such as custom, myth, ceremony, and ritual as unimportant and insult our boss's most recent convention speech with impunity. While we cannot afford to take member depictions of culture at face value or assume, for instance, that what some organizational members call traditions are in fact traditional, we can certainly count on the presence of organizational participants who are altogether prepared to take culture seriously, having dabbled a bit in some cultural analysis themselves.

All this is to say that there is widespread interest in culture as expressed in organizational settings. Ethnographers have been at the game of cultural representation for a long time and are altogether aware that the conventional talk and behavior from which their own portraits of organizational identity and change are derived serve also as interpretative resources to organizational members.

One of the best works to walk the fine line between ethnographic and member accounts is Gideon Kunda's (1992) study of 'culture control' in a high-tech firm. Examining the daily corridor and office talk, formal meetings and assemblies, offsite gatherings and written ideological materials on view in the firm (e.g. e-mails, posters, employee handbooks, etc.), Kunda argues that Techies (his term for organizational members) are active rather than passive recipients of the organizational culture. Paradoxically, however, the longer Techies remain with the firm and the higher they rise in the hierarchy, the less likely they are to experience and appreciate the culture as their own (although they may still post it on to others). The end result is widespread cynicism such that the very processes put to work creating a strong and desirable work culture undermine its existence.

Several chapters in this volume examine organizational identity and change. Most explicitly, Alex Ouroussoff in Chapter Two sketches out the corporate culture of Bion International (a pseudonym like almost all of the organization names used in this collection) where ideas associated with the capacity to manage and being a man are tightly interwoven and the successful high-flying managers in the firm must display toughness, potency, and an almost insatiable appetite for decisive action. This is compared to the culture of a non-profit firm called C&R where the identity between the capacity to manage and being a man is lacking and the successful manager need not demonstrate toughness, potency, or bold action. The contrasts could not be any more striking and Ouroussoff concludes that the profound differences between the two studied organizations give readers a good indication of the cultural diversity that exists among organizations in Britain. More provocatively, the chapter suggests just how difficult it is to imagine changing and perhaps improving the work situation for both women and men in an organization like Bion where 'libidinal desire and economic performance are unconsciously experienced (by male managers) as integral to one another'. At Bion, gender is social structure.

Christine Hine also looks at organizational identity in Chapter Three. In the confessional mode of an ethnographer who had to work long and hard to normalize her outsider presence in the lab, she takes up the work and careers of research scientists with a studied appreciation for how wider narratives – of competition for funds, findings, degrees – were drawn on by organizational members to make sense of the mundane events occurring in the organization. She notes just how the culture of the lab and the scientific results that are produced there are shaped by the notion of being in a race against competing labs. Hines enters the lab knowing something about the computer systems designed to aid the work of lab scientists and uses such knowledge to good effect as she looks closely at how such systems are put into practice in the lab. This dual-sited ethnography allows her to ‘follow a thing’ (information technology) across design and implementation settings discovering in the process just how its meaning shifts.

Another study of organizational identity and change is provided in Chapter Seven by Melissa Parker who takes us into the sexually charged world of a clap clinic in London. Here we learn the status order of diseases and patients as established by an unruly mix of organizational members segmented by rank, training, sexual orientation, and current organizational assignment. Like the two studies above, there is a good deal of culturally shaped work going on in the clinic. AIDs patients, for example, are actively sought out since they bring added resources to the clinic in both money and prestige. Common clap patients do not and are hence treated with dispatch and (relatively) poorly. The organizational culture itself is an unstable one as funding falls in some areas and jobs are lost, as the emotional detachment associated with the work occasionally gives way or as new members come into the organization more or less equipped with new ways of doing old things. Still, sexuality remains an attraction for most employees and serve as an important integrating bond across hierarchies in the clinic.

The three studies summarized here cover a good deal of ground. All are interested in how members of an organization produce, reproduce, and, to a degree, alter the symbolic meanings associated with what they do. Organizational identity is in the foreground of each chapter with change in the background but the two are inherently linked. Moreover, each study suggests that incremental shifts and repositionings of the organizational order are the rule and not the exception. Individuals and groups constantly adapt in response to new problems and this in turn pushes adaptations elsewhere in the organization.

Not represented in this collection are studies of organization change that go beyond modest shifts in the work order and reach deep into organizational structures and cultures. Some fine work in this regard exists. Consider, for example, Barley’s (1986) ethnographic portrait of organizational change in two quite similar radiology laboratories where the same new technology was adopted (computerized scanners) but quite different and largely unintended organizational structures emerged as a

result. Consider too Julian Orr’s (1996) superb look at Xerox repairmen and how the workplace culture shapes their response to both organizational and technological change. Ethnographic work on organizational identity and change cannot but grow in the coming years.

Studies of Organizational Environments

Interest in how the world outside influences the world inside organizations (and vice versa) has occupied organizational theorists for a good deal of time. Much of this work is devoted to defining, characterizing, and categorizing certain alleged features of the social and institutional domains within which organizations, as human creations, operate. This taxonomic focus is hardly surprising. Organizational environments are conceptual fictions that cannot be put under bell jars to have their physical features observed, probed, tested, and measured. Their being is not obvious, their features not given but assigned. Still, there is no shortage of students willing and eager to take up the naming and framing tasks necessary to illustrate, conceptualize, and textualize this domain.

Ethnographic studies of organizational environments are plentiful. Some focus on how ritual practices spread from organization to organization, how broad cultural understandings of proper form and action influence the structural arrangements and socio-economic fortunes in and among organizations, and how new technologies are understood, utilized and in various organizations (often reshaping both the technology and so-called adopting organizations). All suggest that open borders are now far more salient than closed communities.⁸ In addition to fieldwork, studies of this sort rely on a variety of credible secondary data sources including economic indicators, government reports, official statistics, and organizational records. Quasi-historical narratives – in pinched or expansive form – are written to provide meaning and texture to broadly descriptive but not so easily defined environmental constructs such as an institution, regulatory sphere, network, societal norm, industry, field,

8. Broad ethnographic treatments of the environment(s) in which organizations operate are often pitched as occupational, sometimes institutional, studies. See, for example, Rubinstein (1973), Latour and Woolgar (1986 [1979]), Gamst (1980), Applebaum (1981), Estroff (1981), Becker (1982), Kolb (1983), Dubinskas (ed.) (1988), Biggart (1989), Smith (1990), and T. Watson (1994). Some of the most interesting recent studies focus on upheavals in the international order and the resulting impact on organizations and their members. A wonderful display of such work is Glaeser’s (1999) multi-sited ethnography on the merging of the east and west Berlin police forces that took place on the fall of the wall. See, too, Gusterson’s (1996) splendid work on weapon designers in the post-cold war era at the Lawrence Livermore National Laboratory in California, and Milkman’s (1997) careful study of workplace shrinkage in the American auto industry.

governing order and so on. These are all, like organization and culture, conceptual entities but, as well done studies demonstrate, they are as real in their consequence as air quality on health. How such entities form and assert force in the world is the kind of study in which determinative objectivity is a chimera and absolute relativity is self-defeating. Good ethnographic studies do not choose between these poles but walk the tightrope between them.

An example in this regard is Gary Alan Fine's (1996) work in the restaurant industry showing how life in restaurant kitchens emerges from choices made outside those settings. Working in three quite different organizations, Fine demonstrates that what is served (and how it is served) is shaped by the customer niche of the restaurant and the labor and food costs as filtered through the decisions of managers responding to what they regard as market pressures. Another quite different example is Malcolm Young's (1991) close look at police practices as they occur on the ground in urban areas. A career officer who read anthropology rather than law while seconded at mid-career to the university, Young provides a stunning portrait of British policing that cuts across organizations and organizational segments and leaves the reader with a sharp if disturbing symbolic analysis of a most insulated and conservative institution.

Walter Powell's (1985) comparative ethnography of publishing organizations provides a final illustration of work focused on organizations and their environments. Powell places publishing houses in the center of a network of ties that link authors, libraries, booksellers, editors, readers, scholars, and members of parent corporations together. Social ties shape publishing decisions as editors use their network positions to advance their careers, meet organizational goals, and achieve aesthetic aims of publishing high-quality yet marketable books.

In this volume, several chapters provide studies of organizational environments. Most apparent perhaps is Chapter Five where Bob Simpson discusses his multi-sited ethnographic work on divorce and separation in Britain. His study takes place in what he calls a 'kaleidoscope of organizational cultures' involving judges, social workers, barristers, welfare and benefit officers, mediators, doctors, counselors, and many more, including, most critically, members of the families that are splitting up. Sponsored in the research by those wanting to take into account the ever-so-difficult-to-locate 'client's point of view', Simpson notes – with some longing for a fixed research site wherein he could be anthropologically at home – that there is no community of the divorced. His work turns on the metaphors of conflict and separation used by various interested parties in divorce proceedings as a way of ordering and analysing the disorder in a fluid and continually changing organizational field.

In like fashion, David Mosse in Chapter Eight finds himself swimming in multi-organizational waters as he considers the role ethnography plays and might play

in rural development projects. The problem for the would-be ethnographer of development institutions lies in balancing the demands for engagement in an agency with the critical analysis of the purposes and practices of such an agency. To take part in a development project is to put forward an agenda that presumably one believes in. Simultaneously to analyse that agenda as, say, an instance of how thought and practice are structured by development work is, then, a bit like attempting to push (or-stop) the bus that one is currently riding on. Mosse takes us through a bewildering array of participative approaches that have currency in the field and concludes that none of them effectively solves the ethnographic dilemma of how to reflect critically on a project while trying to achieve its goals. In the end, the ethnographer as a member of a development team is probably best served by thinking of social research as a means of testing, refining, and building consensus across groups and organizations party to a development project rather than as a way of analysing and explaining institutional performances and relationships.

Such a position is shared by Simone Abram who, in Chapter Nine, tells a terrific tale of planning and its discontents in a southeast English village. Acting as an ethnographer of policy and policy implementation, Abram works with planners, politicians, local residents, landowners, and developers, all of whom have a stake in the outcome of a housing estate planning process. Like Mosse, Abram is quite aware of the trade-offs involved in practising advocacy ethnography but, in this case, opts not to work with any one group or organization and thus to suspend loyalty while trying to achieve something of a neutral position among those she studies. And, as we might expect, some are accepting of her inquiries (planners) and some are not (politicians). The ethnography that results is not apolitical, of course, but Abram notes it is intendedly disinterested and designed (as the work is being done) to follow through the process by which one view becomes dominant over others.

These three studies of quite complex organizational environments are all rather loose methodologically. This is probably to be expected in multi-sited work for which no apparent format yet exists. The authors tell us about their sources but little about how they sorted out and drew on these sources. As with all ethnographic writings, a good deal of silent selectivity stands behind a writer's decision about what plot to follow, what details to include in a story, what organizing concepts to use, what events to highlight, and so forth. And, as we all know only too well, the facts, even ethnographic ones, do not speak for themselves but must be given significance and weight by interpretive nerve and sharp argument. The craft of organizational ethnography, no less than ethnography in any other domain, is a writer's craft which is learned but not taught. While there may be rules for writing evocative, powerful, persuasive ethnography, rest assured: no one knows what they are.

Studies of Organizational Morality and Conflict⁹

What people (in groups) consider right and worth fighting over cannot be separated from the organizational (or societal) contexts in which conflict appears. In some situations, individuals may act as self-interested, more or less atomized rational actors who seek to maximize their gains or cut their losses. In other situations, people may fight over communal values and beliefs or seek social justice with enormous determination. Those who differ may prefer to negotiate or bargain, turn to a mediator, attack one another, or simply lump their differences and carry on. Ethnographic studies of organizational morality and conflict examine real and imagined breaches in the normative order associated with organizational life. The focus is on disputes – of a mundane or spectacular sort – and the playing out of ruling relations in organizational life.

These can be cut into two types of study. The first focuses on the grievances, conflicts, disputes, and illicit practices that arise in 'legitimate' organizations such as business firms, government agencies, charitable societies, and service groups. The second type examines organizations considered illegal, countercultural, or otherwise beyond the pale given conventional societal standards, such as urban gangs, drug-dealing cabals, groups of flying saucer fanatics, family crime organizations, and religious cults. The line between the two is, however, anything but hard and fast, for ethnographers (and others) continually discover that the boundaries between the legitimate and illegitimate, appropriate and inappropriate, the harmonious and conflictual, the good and bad are everywhere habitually blurred, thus undermining any absolutist or rationalist understanding of normative behavior.

A good deal of work of the first type tracks back and through the anthropology of law. The 'trouble case' method is a familiar strategy for examining conflicts of all sorts and there exists a rich ethnographic literature concerned with how members

9. This category is lifted from a useful literature review of ethnographic contributions to organizational sociology by Morrill and Fine (1997). An enthusiastic and historically informed push for the ethnographic study of conflict in organizations is provided by Barley (1991). A wonderful example of work in this category is Morrill (1995). Disputes and conflicts in organizations are not of course ignored in other research domains but what sets studies of this category apart from others is the ethnographer's focused and intense interest in the moral order of the setting or settings under examination. Conflict, as treated in, for example, ethnographies of informal relations in organizations is interesting insofar as it animates group dynamics or marks and solidifies social boundaries in the organization but it is not the centerpiece of the work. In organizational identity studies, conflict among self-interested groups is interesting insofar as what is at stake is the character of the overall organization, but how conflicts routinely arise and how they are settled among organizational members are of secondary interest. Conflict is considered common, but the moral basis on which it turns is not of central concern.

of various societies and cultural groups handle their disputes (e.g. Nader and Todd 1978). A recent organizational ethnography making good use of such an approach is James Tucker's *The Therapeutic Organization* (1999). The setting for this study is a self-labeled 'post-bureaucratic' business firm that has tried with some success to decentralize and flatten hierarchical lines of authority, reduce specialization, and promote strong personal relationships and goodwill among employees (who are a relatively homogeneous lot in terms of class and race). Tucker's interests lie in how employees of a firm emphasizing teamwork and cooperation in a work world marked by considerable (and intentional) ambiguity define and respond to deviance in their midst. He finds they do so rather sympathetically, turning interpersonal and intergroup troubles into intrapersonal afflictions of the sort that self-help, counseling, and friendly third-party coaching can typically resolve. Conflict does not of course vanish but is shaped, managed, and constrained by subtle, informal means initiated by fellow employees diagnosing and treating one another (and themselves) for personal lapses and inadequacies. Tucker concludes with the entirely plausible thesis that therapeutic means of dispute resolution – and a morality system based on self-help, personal character, and individual responsibility – thrive under conditions of equality and intimacy.

Of particular interest to ethnographers in this subdomain is the social organization of what might be thought of as systemic deviance. A number of striking ethnographic studies explore persistent negligence, incompetence, mismanagement, rule-bending, and rule-breaking in organizations as diverse as ministries, hospitals, sports teams, universities, and business enterprises. Sometimes it is a single case that organizes a work as in Diane Vaughan's (1996) ethnographic look at the working norms and practices of various operational and engineering groups in NASA leading to the Challenger disaster. Other times, corporate misconduct (or dirty business) is simply business as usual and ethnographically portrayed in precisely such a way (Punch 1996; Mars 1982). Perhaps nowhere is the friction between the espoused aims of a collectivity and the evasive social reality of daily interactional life revealed as more problematic than in ethnographic studies of organizations created to uphold moral systems such as the police and the courts (e.g. Manning 1981; Conley and O'Barr 1990).

Just as ethnographers of supposedly legitimate organizations illustrate their unseemly or deviant sides, ethnographers of deviant organizations illustrate their seemly or normal sides. This anthropologizing of unconventional and alien occurs in organizational as well as societal contexts. One such study is Jankowski's (1992) ethnographic treatment of urban gangs as quasi-rational business organizations in which Weber's venerated entrepreneurial spirit, not risk, turf, or violence, is the driving force in the organization. Another example of the contextualization of the seemingly exotic is Anne Allison's (1994) linking of male dominance and organizational culture to playing out (and with) gender identities in the hostess clubs

associated with corporate nightlife in Japan. Hostess clubs of both the respectable and not-so-respectable sort are in Allison's view not peripheral play palaces for upper-level Japanese *sararimen* out for a good time but rather central establishments for the creation of bonds among white-collar men which help to forge a masculine identity that suits the needs of the prestigious organizations that foot the bill.

In this volume, commentary on organizational morality and conflict is part of the background of most chapters (most apparent perhaps in those by Ouroussoff, Pulman-Jones, and Parker) but comes to the fore only in two. And, in both, the deviance and conflict (largely submerged) to be accounted for is our own. In Chapter Ten, Stella Mascarenhas-Keyes looks at some of the difficulties ethnographers run into when working with and for non-academic institutions. The call for short-cut or time-efficient research methods is familiar to ethnographers who have written needs assessments, market appraisals, or set forth intervention strategies and prescriptions for change at the bequest of others. But is speedy ethnography — what Rist (1980) calls, 'Blitzkreig ethnography' — possible? Can it answer the kinds of questions the movers and shakers of policy worlds have in mind? Mascarenhas-Keyes is not altogether certain but documents a fair number of mistakes she made in the field as a way to gently remind readers that research purpose(s) and quality are quite flexible, contestable matters, irrevocably linked to political and moral concerns.

Martin O'Neill, in Chapter Eleven, worries about the proper role of an ethnographer when studying one's own kind and having therefore the skills and interests that may help others in times of need. In theory, O'Neill notes, the ethnographer is the 'detached observer' attending first and foremost to research. In reality, as O'Neill discovered, such a stance is not only impractical and likely to cause resentment, but immoral if suffering results. The brief case study O'Neill puts forward recounts an episode in which he more or less forgot his research role and joined with those he was studying in the handling of a professional (and urgent) task. It is a case that reminds us that the role of an ethnographer always competes with other roles we play — as citizens, as friends, as parents, or, in O'Neill's case, as a former ambulance worker quite able to offer aid and assistance if needed on the scene of an emergency. And, as O'Neill comes to realize, to embrace fully a research role would be to reduce ethnography to a soulless, empty, technocratic pursuit, devoid of recognizably human obligations and attachments.¹⁰

10. It seems the moral dilemmas of fieldwork become increasingly of interest to ethnographers as they move closer to home. With such a move has come the somewhat belated recognition that participant-observation is yet another brilliant addition to our repertoire of ways to make ourselves (and others) uncomfortable when in close contact. That there are no special moral codes available to fieldworkers (or special dispensation from them) is a point well and movingly made by Behar (1996), Wolfe (1992), and Rosaldo (1989).

Both O'Neill and Mascarenhas-Keyes remind us of a few of the ethical and moral dilemmas that seem to be particularly linked with organizational ethnography. Along with other contributors to this volume, they also remind us that there may be some larger-scale changes at work that bode ill for ethnographic scholarship. As we try, for example, to find ways to speed up our performance, accept the dictates of research review boards without principled resistance, reduce our aims to the representation of mentalities or points of view not contextualized by class, race, gender, politics, or economics, or adopt without question the aims of research sponsors as a kind of tax burden necessary to carry on our work, ethnography become less distinct, adventurous, and valuable as a research endeavor. Ethnographic work will certainly continue in the foreseeable future, but of what kind?

Some Final Words . . .

The four areas of organizational study highlighted above serve mainly as templates to organize research materials. But they can also be thought of as thematic tensions (or temptations) built into organizational ethnography. Each offers a seductive way to frame ethnographic materials. Lengthy periods of fieldwork allow for considerable topical selectivity and range. As a relatively artistic, improvised, and situated model of social research, a good number of authorial choices are required when writing up (or down) the results of a study. A single-sited study could result in a research report focused on any one or several of the following: organization culture and sub-culture, power and authority, managerial decision-making, feuding occupational enclaves, environmental pressures and influence, change (and resistance to change), and so on. Thematic choices among many possibilities are required and any given study could go in a number of substantive directions. Moreover, since ethnographies are typically designed while they are being accomplished, what begins as, say, a focused probe of informal relations on the shopfloor might well end up as a study of new recruits to the organization or the response of the workforce to a new production technology or form of management.

There is, in sum, a rather intractable unpredictability associated with ethnographic studies. Templates can be bent, stretched, and combined in a variety of ways. Where the research will lead is often up in the air throughout a fieldwork stay and beyond. Sponsors of ethnographic research may regard this as perverse but few ethnographers will argue that it could or should be otherwise. Trying to chart a direction in a scholarly field that emphasizes particularism over universalism and regards the playing out of chance and indeterminacy more highly than causality and correlation is therefore a risky business. But, like the economist who predicted nine of the last two recessions, I will bring this meandering to a close by considering (quickly) a few areas where I think ethnographic work might in the near future concentrate and prove particularly helpful and timely to both organizational researchers and

practitioners alike. These are limited to broad alterations now taking place in the workplace (and in the nature of work itself) and address the rather general 'flies in amber' critique leveled occasionally at ethnography for too often presenting a rather static view of social life and soft-pedaling flux, change, and historical agency (e.g. Marcus 1998; Clifford 1988)

Most pertinent perhaps is the blurring of practical and conceptual boundaries surrounding the workplace as work careers, jobs, firms, hierarchy, and the division of labor change in character and meaning.¹¹ Sharpness and clarity give way to fog and uncertainty. Where exactly is the workplace? What is a firm? Who is a manager? Societal-level changes are afoot here as well. Take for example the many public agencies that are now expected to operate according to market principles. How are they doing? Or, what is happening to those public services once thought to be the sacred preserve of the state but which have been turned over to profit-making firms? What is a public good? How is it dispensed? Who wins and who loses?

A good deal of conceptual mischief is also occurring on both sides of the conventional boundary separating home and work. Policies, practices, and opportunities continually arise to integrate or collapse these worlds. Increasing amounts of paid labor are now forms of 'remote work', performed at home, in the car, at a client's office, in hotel suites, in conference centers, or, more generally, out of the traditional plant or office. And home moves toward work as well with fashionable 'quality-of-life programs' of on-site day care, elder care, satellite schools, and flex-time programs appearing in the workplace. A result is that some of our most tenaciously held assumptions about the proper organization of work and the proper work of organizations are being questioned.

Organizational ethnographers are well equipped to examine local and broad shifts taking place in what it means to be a 'good' organizational citizen, to put forth a 'fair' day's work, or observe the 'right' of privacy (e.g. 'Have you had your urine test today?'). What does a supervisor do in a 'post-bureaucratic' world? What of those protean yet ever-more-elusive middle managers in organizations? How are managers and non-managers distinguished when 'self-directed' work arrangements push supervisory responsibility to front-line employees? Is the difference merely a parking space, a style of dress, the size of a pay check, access to certain

11. It is here perhaps where postmodernism plays an obvious and useful analytic role. As Marcus (1998) suggests, postmodernism finds its subject matter and audience where meanings are fluid, where stable identities are difficult to find, where the boundaries that structure identity such as work, family, home, and state are apparently collapsing, and where the experience of liminality is seemingly widespread. Such conditions give postmodernism its exaggerated, hyperreal character as well as its apocalyptic flair. With a few exceptions, ethnographers have been wary of postmodernism, approaching it not so much as a poisonous or dastardly narcotic but as a temptation still somewhat out of reach. This cautious stance of ethnographers toward postmodern social theory is taken up by Graham Watson (1987) in his wonderfully titled article, 'Make me reflexive - but not yet'. See, also, the essays collected in Brown (1995).

scarce resources or, as Nippert-Eng (1995) argues, the right to blur boundaries that others cannot? Does it mean that the manager becomes simply a financial overseer (untrained in the specifics of the work or organization)?

12. Systems of meaning are the bread and butter of ethnographic research and they are anything but stable. With structural and technological change, work arrangements and the interaction patterns that support them shift as individuals and groups face new problems that both reflect and promote broader cultural changes that in turn influence organizations in new ways. Telecommunication technologies for example alter interaction patterns in and between organizations but we know little of such matters. Ten years ago few of us had even heard of, much less used, the internet, voice mail, cell phones, e-mail, video conferencing, and the like. Changes are no doubt both subtle and dramatic but they are seemingly taking place on a grand scale. What might an ethnography of an organization look like if the work studied involved computer technicians writing programs in the morning in Beijing and sending their work at the end of the day to a similarly trained group in Seattle who pass it on at the end of their day to another group in Bangalore who, after putting in their efforts, then send it back to Beijing? Messy ethnography indeed.

Consider, too, the use of teams in organizations. Teamwork is something of a current buzzword in a number of organizations large and small. Work teams typically cut across occupational divisions of labor, operational units, and, sometimes, hierarchical ranks. Occasionally government regulators, consultants, suppliers, and customers or clients to the organization are added to the mix, vastly complicating the work lives of employees. A person's job becomes embedded within a greater variety of others' work such that just who is responsible for what becomes difficult to determine. Moreover, the same individual may serve on several teams simultaneously. Teamwork of this sort may demand new skills and create new group alignments and loyalties that slide and collide with one another at various times in various places both inside and outside the organization. What this might mean in terms of one's identity (or lack thereof) as an organizational member, a worker, a manager is clearly an open question.

13. Relatedly, shrinkage of the workforce in organization is also of much current interest. What are we to make of across-the-board reductions in force? With fewer resources to support their work, the presumed lucky ones left seated after a sharp cut may well be asked to stand and do a good deal more than they have previously been accustomed to do - to fix computers, move furniture, monitor the phones, polish the floors, run copy machines, host visitors, create their own desk-top-publishable reports, and turn off the lights on the way home. Fewer permanent employees may well do the same amount of work required in the organization but the way they think about, interact, and carry on with their duties will surely change. And what of the downsized? Where do they go? What do they do? How does a laid-off working mother of three feed her family?

Figuring out where the organization begins and ends and just who belongs to it is yet another area that is likely to be increasingly problematic in the future. The growing use of part-time workers and consultants, the 'out-sourcing' of services, the tightening of connections between clients and providers or vendors and buyers all lead to ever more slippery and amorphous organizational borders. If workers are 'leased' between NatWest in the UK and Bank One in the US for months, even years, at a time, for whom do they work? Does it matter? What are we to make of occupational gypsies or transients – whether machinists, fishermen, software engineers, or university professors? Are organizational structures and cultures becoming increasingly homogenized or isomorphic as permeability advances or are they increasingly diverse? How are power centers shifting?

By raising these questions, my purpose is simply to underline the point that the study of contemporary organizations is and will remain a vibrant area. It is a (somewhat) delimited domain of social life richly and, I think, especially fit for ethnographic representation. The work presented in this volume is testimony to the ethnographer's pledge to take us elsewhere and thus helps us learn what certain groups of people who are more or less stuck in organizational conditions they did not entirely choose are doing and why. This is 'getting out the news' – telling what lab scientists do, how some managers get ahead, what development planners are up to, how certain clinics are organized, and so on. It is storytelling to be sure, but remains firmly in a scholarly, non-fiction division and when done well carries a good deal of persuasive clout and may well expand our sense of human possibilities through the study of how people do things together.

While there are pressures on ethnographers to produce detachable conclusions and weave ever more intricate analytic webs, these are stymied by the storytelling or narrative basis of the trade and resisted with style. Narrative is not an ornamental or decorative feature designed to make ethnography more palatable or audience-friendly, but a cognitive instrument in its own right. Realism, as a representational, tale-telling practice, plus a belief system grounded on a witnessing ideal and tied to a methodology that remains improvisational and loose, still marks the trade. This is not to be dismissed. It may well be what ethnographers do best. There is much to be done.

References

- Albolafia, M. (1998), *Making Markets*, Cambridge, MA: Harvard University Press.
 Allison, A. (1994), *Nightwork: Sexuality, Pleasure, and Corporate Masculinity in a Tokyo Hostess Club*, Chicago: University of Chicago Press.
 Appadurai, A. (1997), 'Fieldwork in the Era of Globalization', *Anthropology and Humanism*, 22: 115–18.

- Applebaum, H. (1981), *Royal Blue: The Culture of Construction Workers*, New York: Holt, Rinehart, and Winston.
 Barley, S.R. (1986), 'Technology as an Occasion for Structuring: Evidence of CT Scanners and the Social Order of Radiology Departments', *Administrative Science Quarterly*, 31: 78–108.
 — (1991), 'Contextualizing Conflict: Notes on the Anthropology of Disputes and Negotiations', in M.H. Bzerman, R.J. Lewicki, and B.H. Sheppard (eds), *Research on Negotiation in Organizations*, 3: 165–203.
 Becker, H.S. (1982), *Art Worlds*, Berkeley: University of California Press.
 Behar, R. (1996), *The Vulnerable Observer: Anthropology that Breaks Your Heart*, Boston: Beacon Press.
 Biggart, N.W. (1989), *Charismatic Capitalism: Direct Selling Organizations in America*, Chicago: University of Chicago Press.
 Brettell, C.B. (ed.) (1993), *When They Read What We Write*, Westport, CT: Bergin and Garvey.
 Brown, R.H. (ed.) (1995), *Postmodern Representations*, Urbana: University of Illinois Press.
 Burawoy, M. and Verdery, N. (1999), *Uncertain Transition: Ethnography of Change in the Postsocialist World*, NY: Rowman and Littlefield.
 Chanlet, J.-F. (1994), 'Francophone Organizational Analysis (1950–1990)', *Organization Studies*, 15: 47–79.
 Child, J. (1995), 'Guest Editorial' (Special Issue: The European Perspective on Organization Theory), *Organization Science*, 6: 117–18.
 Clifford, J. (1988), *The Predicament of Culture: Twentieth-Century Ethnography, Literature, and Art*, Cambridge, Mass.: Harvard University Press.
 — (1997), *Routes: Travel and Translation in the Late Twentieth Century*, Cambridge, Mass.: Harvard University Press.
 Conley, J.M. and O'Barr, W.M. (1990), *Rules versus Relationships: The Ethnography of Legal Discourse*, Chicago: University of Chicago Press.
 Czarniawska, B. (1997), *Narrating the Organization: Drama of Institutional Identity*, Chicago: University of Chicago Press.
 Dalton, M. (1959), *Men Who Manage: Fusions of Feeling and Theory in Administration*, New York: Wiley.
 Davis, S.G. (1997), *Spectacular Nature: Corporate Culture and the Sea World Experience*. Berkeley: University of California Press.
 di Leonardo, M. (1998), *Exotics at Home: Anthropologies, Others and American Modernity*, Chicago: University of Chicago Press.
 Dubinkas, F. (ed.) (1988), *Making Time: Ethnographies of High Technology Organizations*, Philadelphia: Temple University Press.
 Economist (1996), 'Nobel Savages', 30 March, 72.

- Estroff, S.E. (1981), *Making It Crazy: An Ethnography of Psychiatric Clients in an American Community*, Berkeley: University of California Press.
- Ewick, P. and Silbey, S. (1995), 'Subversive Stories and Hegemonic Tales: Toward a Sociology of Narrative', *Law and Society Review*, 29: 197–226.
- Featherstone, M. (ed.) (1990), *Global Culture*, London: Sage.
- Fine, G.A. (1996), *Kitchens: The Culture of Restaurant Work*, Chicago: University of Chicago Press.
- Fjellman, S.M. (1992), *Vinyl Leaves: Walt Disney World and America*, Boulder, CO: Westview.
- Fox, R.G. (ed.) (1991), *Recapturing America: Working in the Present*, Santa Fe, NM: School of American Research Press.
- Friedman, T.L. (1999), *The Lexus and the Olive Tree: Understanding Globalization*, New York: Farrar, Strauss, and Giroux.
- Gamst, F.C. (1980), *The Hogshead: An Industrial Ethnology of the Locomotive Engineer*, New York: Holt, Rinehart, and Winston.
- Garson, B. (1975), *All the Livelong Day: The Meaning and Demeaning of Routine Work*, Garden City, NJ: Doubleday.
- Geertz, C. (1983), *Local Knowledge: Further Essays in Interpretive Anthropology*, New York: Basic.
- (1988), *Works and Lives: The Anthropologist as Author*, Stanford, CA: Stanford University Press.
- (1995), *After the Fact: Two Countries, Four Decades, One Anthropologist*, Cambridge, Mass.: Harvard University Press.
- Glaeser, A. (1999), *Divided in Unity: Identity, Germany and the Berlin Police*, Chicago: University of Chicago Press.
- Goffman, E. (1959 [1956]), *The Presentation of Self in Everyday Life*, New York: Anchor.
- (1961), *Asylums: Essays on the Social Situation of Mental Patients and Other Inmates*, New York: Anchor.
- Gupta, A. and Ferguson, J. (eds) (1997), *Anthropological Locations: Boundaries and Grounds of a Field Science*, Berkeley: University of California Press.
- Gusterson, H. (1996), *Nuclear Rites: A Weapons Laboratory at the End of the Cold War*, Berkeley: University of California Press.
- Handy, C. (1996), *Beyond Certainty*, Boston: Harvard Business School Press.
- Hannerz, U. (1992), *Cultural Complexity: Studies in the Social Organization of Meaning*, New York: Columbia University Press.
- Hinnings, B. (1988), 'Defending Organization Theory: A British View from North America', *Organization Studies*, 9: 2–7.
- Hochschild, A.R. (1983), *The Managed Heart: Commercialization of Human Feeling*, Berkeley: University of California Press.

- Jackall, R. (1988), *Moral Mazes: The World of Corporate Managers*, New York: Oxford University Press.
- Jankowski, M.S. (1992), *Islands in the Street: Gangs and American Urban Society*, Berkeley: University of California Press.
- Kahn, J. (1995), *Culture, Multiculture, Postculture*, London: Sage.
- Kanter, R.M. (1977), *Men and Women of the Corporation*, New York: Basic.
- Kidder, T. (1981), *The Soul of the New Machine*, Boston: Little, Brown.
- (1990), *Among Schoolchildren*, New York: Avon.
- Kolb, D.M. (1983), *The Mediators*, Cambridge, Mass.: MIT Press.
- Koza, M.P. and Thoenig, J.-C. (1995), 'Organization Theory at the Crossroads: Some Reflections on European and United States Approaches to Organizational Research', *Organization Science*, 6: 1–8.
- Kunda, G. (1992), *Engineering Culture: Control and Commitment in a High-Tech Corporation*, Philadelphia: Temple University Press.
- Kuper, A. (1999), *Culture: The Anthropologists' Account*, Cambridge, Mass.: Harvard University Press.
- Lamphere, L. (ed.) (1992), *Structuring Diversity: Ethnographic Perspectives on the New Immigration*, Chicago: University of Chicago Press.
- Latour, B. (1993), *We Have Never Been Modern*, tr. C. Porter, Cambridge, Mass.: Harvard University Press.
- and Woolgar, S. (1986 [1979]), *Laboratory Life: The Construction of Scientific Facts*, Princeton: Princeton University Press.
- Leidner, R. (1993), *Fast Food, Fast Talk: Service Work and the Routinization of Everyday Life*, Berkeley: University of California Press.
- Lewis, M. (1990), *Liar's Poker: Rising through the Wreckage on Wall Street*, New York: Penguin.
- Lutz, C. and Collier, J. (1992), *Reading National Geographic*, Chicago: University of Chicago Press.
- Manning, P. (1981), *The Narc's Game*, Cambridge: MIT Press.
- Marcus, G.E. (1998), *Ethnography through Thick and Thin*, Princeton: Princeton University Press.
- Mars, G. (1982), *Cheats at Work: An Anthropology of the Workplace*, London: Unwin.
- Milkman, R. (1997), *Farewell to the Factory: Auto Workers in the Late Twentieth Century*, Berkeley: University of California Press.
- Morrill, C. (1995), *The Executive Way: Conflict Management in Corporations*, Chicago: University of Chicago Press.
- and Fine, G.A. (1997), 'Ethnographic Contributions to Organizational Sociology', *Sociological Methods and Research*, 25: 424–51.
- Nader, L. and Todd, H.F. (eds) (1978), *The Disputing Process: Law in Ten Societies*, New York: Columbia University Press.

- Nash, J. (1993), *Crafts in the World Market*, Albany: State University of New York Press.
- Nippert-Eng, C.E. (1995), *Home and Work: Negotiating Boundaries through Everyday Life*, Chicago: University of Chicago Press.
- Orr, J.E. (1996), *Talking about Machines: An Ethnography of a Modern Job*, Ithaca: Cornell University Press.
- Passaro, J. (1997), 'You can't take the Subway to the Field', in A. Gupta and J. Ferguson (eds), *Anthropological Locations*, Berkeley: University of California Press.
- Perlow, L.A. (1997), *Finding Time: How Corporations, Individuals and Families Can Benefit from New Work Practices*, Ithaca: Cornell University Press.
- Powell, W.W. (1985), *Getting into Print: The Decision-making Process in Scholarly Publishing*, Chicago: University of Chicago Press.
- Punch, M. (1996), *Dirty Business: Exploring Corporate Misconduct*, London: Sage.
- Radway, J.A. (1997), *A Feeling for Books: The Book-of-the-Month Club, Literary Taste, and Middle-Class Leisure*, Chapel Hill, NC: University of North Carolina Press.
- Rist, R.C. (1982), 'Blitzkrieg Ethnography: On the Transformation of a Method into a Method', *Educational Researcher*, 9: 8–10.
- Rosaldo, R. (1989), *Culture and Truth: The Remaking of Social Analysis*, Boston: Beacon Press.
- Rubinstein, J. (1973), *City Police*, New York: Farrar, Strauss, Giroux.
- Smith, V. (1990), *Managing in the Corporate Interest: Control and Resistance in An American Bank*, Berkeley: University of California Press.
- Terkel, S. (1974), *Working: People Talk About What They Do All Day and How They Feel About What They Do*, New York: Random House.
- (1984), *The Good War: An Oral History of World War II*, New York: Random House.
- Tucker, J.C. (1999), *The Therapeutic Organization*, New York: Oxford University Press.
- Van Maanen, J. (1995), 'An End to Innocence: The Ethnography of Ethnography', in J. Van Maanen (ed.), *Representation in Ethnography*, London: Sage.
- (ed.) (1999), *Qualitative Studies of Organizations*, London: Sage.
- Vaughan, D. (1996), *The Challenger Launch Decision: Risky Technology, Culture, and Deviance at NASA*, Chicago: University of Chicago Press.
- Watson, G. (1987), 'Make Me Reflexive – But not Yet: Strategies for Managing Essential Reflexivity in Ethnographic Discourse', *Journal of Anthropological Research*, 43: 29–41.
- Watson, J. (ed.) (1997), *Golden Arches East: McDonald's in East Asia*, Stanford, CA: Stanford University Press.

- Watson, T.J. (1994), *In Search of Management: Culture, Chaos and Control in Managerial Work*, London: Routledge.
- Wolfe, M. (1992), *A Thrice Told Tale: Feminism, Postmodernism and Ethnographic Responsibility*, Stanford, CA: Stanford University Press.
- Young, M. (1991), *An Inside Job: Policing and Police Culture in Britain*, Oxford: Clarendon.
- Zabusky, S.E. (1995), *Launching Europe: An Ethnography of European Cooperation in Space Science*, Princeton, NJ: Princeton University Press.

Name Index

- Abram, S. 4, 5, 9, 239, 249
Akeroyd, A.V. 229, 230
Álbolafia, M. 236, 256
Allen, I. 151-2, 155
Allen, J. 187, 202
Allison, A. 251-2, 256
Álvesson, M. 87, 95, 179
Appadurai, A. 172, 179, 234, 256
Applebaum, H. 247, 257
Ardener, E. 20, 32
Asad, T. 13
Atkinson, P. 89, 95, 229, 230
- Bagadion, B.U. 173, 179
Baland, J.M. 177, 179
Barley, S.R. 246, 250, 257
Bate, S.P. 1-2, 6, 9, 13
Baumann, P. 177, 179
Beales, A. 223, 230
Beck, U. 103, 104, 114
Becker, H.S. 247, 257
Beck-Gernsheim, E. 103, 114
Behar, R. 252, 257
Berreman, G. 89, 92, 95
Bicker, A. 183, 203
Biggart, N.W. 247, 257
Boddy, D. 64, 74
Bourdieu, P. 223
Boyacigiller, N. 21, 33
Brettell, C. 237, 257
Brown, R.H. 254, 257
Buchanan, D. 5, 13, 64, 74
Buckley, P. 20, 22-3, 25, 27, 32-3
Buerk, M. 2
Burawoy, M. 239, 257
Burke, K. 56, 57
Burton, L.B. 103, 116
- Cahoon, L. 177, 179
Casson, M. 22, 32
- Chambers, R. 2, 13, 165-7, 179
Chanlet, J.-F. 233, 257
Chapman, M. 4, 6, 7, 10, 13, 37, 92, 94, 236, 237
Charlesworth, M. 72, 74
Child, J. 233, 257
Clifford, J. 4, 7, 13, 93, 95, 234, 254, 257
Coase, R. 22, 32
Cochrane, A. 187, 202
Cohen, A.P. 90, 95
Collier, J. 240, 259
Collins, H.M. 72, 74
Collins, J. 107, 114
Conley, J.M. 251, 257
Cortyon, J. 101, 114, 115
Cullen, S. 199, 202
Cunnison, S. 35, 57
Czamiawksa, B. 240, 257
- Dalton, M. 241, 257
Davidson, A.I. 200, 202
Davis, G. 109, 114
Davis, S.G. 240, 257
de Beauvoir, S. 42, 57
Denning, P.J. 62, 74
Descola, P. 37, 58
di Leonardo, M. 239, 257
Dickens, P. 19, 32
Donegan, E. van 137, 155
Donnan, H. 113, 114
Douglas, D.J. 224, 230
Douglas, M. 134, 135
Dubinskas, F. 247, 257
Du Gay, P. 82, 95
Dunning, J. 19
Durkheim, E. 4
- Edwards, J. 4, 14, 199, 202
Edwards, M. 177, 179
Errington, F. 82, 95

Name Index

Estroff, S.E. 247, 258
 Evans-Pritchard, E.E. 20, 30
 Ewick, P. 240, 258

Farrington, J. 178, 179, 180
 Featherstone, M. 234, 258
 Ferguson, J. 234, 258
 Fine, G.A. 248, 250, 258, 259
 Finer, A. 109, 113, 114
 Fjellman, S.M. 240, 258
 Flyvbjerg, B. 185, 202
 Forester, J. 192, 202
 Foucault, M. 185, 200
 Fox, R.G. 234, 258
 Freire, P. 167
 Friedman, M. 236
 Friedman, T.L. 243, 258
 Fujimura, J. 72, 75

Gamst, F.C. 247, 258
 Gardner, K. 183, 202
 Garson, B. 240, 258
 Geertz, C. 6, 14, 80, 103, 106, 114, 175, 179, 234, 238, 258
 Gewertz, D. 82, 95
 Giddens, A. 113, 114
 Gilbert, E. 164
 Gilbert, G.N. 72, 75
 Gillespie, R. 3, 14
 Ginsburg, F. 104, 114
 Glaeser, A. 247, 258
 Goffman, E. 86, 89, 95, 215, 220, 224, 230, 241-2, 258
 Gooday, G. 83, 95
 Gupta, A. 234, 258
 Gusterson, H. 247, 258

Hall, S. 82, 95
 Hammersley, M. 1, 14, 89, 95, 229, 230
 Handy, C. 243, 258
 Hannerz, U. 234, 258
 Haraway, D. 104, 114
 Harper, R. 37, 58
 Hastrup, K. 87, 89, 93, 95, 104, 114
 Haynes, J. 110, 114
 Heath, D. 72, 75
 Heine, B. 10, 14
 Henty, J. 41, 58

Hine, C. 4, 5, 7, 9, 91, 246
 Hinnings, B. 233, 258
 Hocart, A.M. 25
 Hochschild, A.R. 240, 258
 Hofstede, G. 31, 33
 Hogg, D. 151-2, 155
 Howe, L. 191, 202
 Huizer, G. 199, 202
 Humphreys, L. 223, 230
 Hutchinson, S.A. 223, 224, 228, 230

Iyengar, S. 173, 180

Jackall, R. 241, 259
 Janelli, R. 37, 58
 Jankowski, M.S. 251, 259
 Johnson, M. 107, 115
 Jones, S. 160, 180

Kahn, J. 234, 259
 Kanter, R.M. 241, 259
 Kaufman, G. 19
 Kayser-Jones, J.S. 137, 155
 Kidder, T. 238, 259
 King, M. 109, 114
 Knorr-Cetina, K. 72, 75
 Kolb, D.M. 247, 259
 Kopytoff, I. 82, 95
 Kornhauser, L. 109, 115
 Korten, F.F. 173, 175, 179, 180
 Koza, M.P. 233, 259
 Krause, I.-B. 137, 155, 219, 220
 Kunda, G. 245, 259
 Kuper, A. 10, 14, 239, 259

Lakoff, G. 107, 115
 Laing, R.D. 53, 58
 Lamphere, L. 234, 259
 Lasch, C. 103, 115
 Latour, B. 11, 14, 37, 58, 70, 72, 75, 80, 83, 87, 95, 200, 202, 237, 247, 259
 Law, J. 4, 14, 89, 95
 Layne, L.L. 72, 75
 Leach, E. 103, 115
 Leblanc, B. 19
 Leidner, R. 240, 259
 Lévi-Strauss, C. 106, 115
 Lewis, D. 175-6, 180, 183, 202

Name Index

Lewis, M. 236, 259
 Le Witt, B. 6, 14
 Lienhardt, G. 10, 14
 Lipsky, M. 199, 202
 Lloyd Warner, W. 236, 241
 Loder, C. 205, 220
 Lofland, J. 229, 230
 Long, A. 174, 180
 Long, N. 174, 180
 Lupton, T. 35, 58
 Luttwak, E. 42, 58
 Lutz, C. 240, 259
 Lynch, M. 72, 75

McCalman, J. 64, 74
 McCarthy, P. 99, 101, 114, 115, 116
 Macdonald, S. 4, 5, 8-10, 12, 242
 MacFarlane, G. 113, 114
 Malinowski, B. 2, 8, 12, 36, 223, 236
 Manning, P. 251, 259
 Mannon, J.M. 224, 226, 228, 230
 Marcus, G.E. 7, 14, 63, 72, 75, 104, 115, 254, 259
 Mars, G. 251, 259
 Marsh, D. 199, 202
 Martin, E. 91, 96
 Mascarenhas-Keyes, S. 5, 8, 10, 252-3
 Massey, D. 187, 202
 Maxwell, R. 62, 75
 Mayer, J.E. 2, 14
 Mehta, M. 171, 180
 Metz, D.L. 224, 228, 230
 Milkman, R. 247, 259
 Miller, D. 6, 7, 9, 14, 78-9, 86, 88, 90, 95
 Mills, C. Wright 94, 96
 Minford, P. 19, 33
 Mitchell, S.A. 2, 14
 Mnookin, R. 109, 115
 Modell, J. 137, 155
 Morgan, G. 3, 14
 Morley, D. 1, 14
 Morrill, C. 250, 259
 Mosse, D. 4, 5, 9, 10, 12, 183, 238, 248
 Mulkay, M. 72, 75
 Murdoch, J. 185, 200, 202

Nader, L. 251, 259
 Naipaul, V.S. 9, 14

Nash, J. 239, 260
 Negandhi, A. 21, 33
 Nippert-Eng, C.E. 240, 254, 260

O'Barr, W.M. 251, 257
 Ogus, A. 101, 115
 Okely, J. 35, 58, 89, 96, 201, 203
 Olwig, K.F. 104, 114
 O'Neill, M. 7, 9, 252-3
 Orone, P. 169, 181
 Orr, J.E. 247, 260
 Ouroussoff, A. 4-6, 237, 245, 252

Parthasarathy, R. 173, 180
 Parker, M. 4, 5, 8, 246, 252
 Parkinson, L. 109, 115
 Passaro, J. 234, 260
 Perlow, L.A. 240, 260
 Pfaffenberger, B. 62, 75
 Piper, C. 109, 114
 Platteau, J.-P. 177, 179
 Pottier, J. 167, 169, 181
 Powell, W.W. 248, 260
 Pulman-Jones, S. 4, 7-8, 137, 238, 242, 252
 Punch, M. 251, 260

Rabinow, P. 12, 14
 Rachel, J. 64, 76
 Radcliffe-Brown, A.R. 2
 Radway, J.A. 240, 260
 Rapp, R. 104, 114, 137, 156
 Reinharz, S. 228, 230
 Rew, A. 178, 180
 Rhodes, R.A.W. 199, 202
 Richardson, G.B. 22, 33
 Ricoeur, P. 106
 Rist, R.C. 252, 260
 Roberts, K. 21, 33
 Robertson, A.F. 104, 115, 183, 203
 Rosaldo, R. 252, 260
 Rose, N. 113, 115
 Roy, D. 223, 230
 Rubinstein, J. 247, 260
 Ryan, D. 37, 58

Sahlins, M. 35, 38, 58
 Salmen, L. 174, 181
 Sanjek, R. 5, 14

Name Index

- Schneider, D.M. 105, 115
 Schön, D. 107, 115
 Sekaran, U. 21, 33
 Serres, M. 200
 Silbey, S. 240, 258
 Sillitoe, P. 183, 203
 Silverstone, R. 79
 Simpson, B. 4, 8, 9, 239, 248
 Sinclair, S. 1, 14, 137, 156
 Smith, V. 247, 260
 Stack, C. 103, 116
 Stocking, G. I. 2, 14
 Strathern, M. 6, 7, 11, 12, 14, 47, 58, 94, 96, 103, 105, 116
- Terkel, S. 238, 260
 Thoenig, J.-C. 233, 259
 Thornton, R. 7, 15
 Timms, N. 2, 14
 Todd, H.F. 251, 259
 Tonkin, E. 27, 33
 Traweek, S. 72, 76
 Tucker, J.C. 251, 260
 Turnbull, C. 10, 15
- Uphoff, N. 164, 181
- van Gennep, A. 134, 135
 Van Maanen, J. 4, 10, 15, 260
- van Velsen, J. 171, 181
 Vaughan, D. 251, 260
 Veneración, C. 173, 181
 Verdery, N. 239, 257
 Vike, H. 185, 199, 03
 Volken, H. 164, 181
- Walker, J. 101, 109, 114, 115, 116
 Watson, G. 254, 260
 Watson, J. 234, 260
 Watson, T.J. 247, 261
 Weber, M. 2-3, 251
 Weiss, C. 102, 116
 Werbner, P. 82, 96
 Williamson, O. 22, 33
 Wilmott, H. 37, 58
 Wolfe, M. 252, 260
 Woolgar, S. 11, 37, 58, 64, 70, 72, 76, 247
 Wright, S. 3-4, 15, 175, 181, 183, 186, 203, 220
 Wylie, L. 30-1, 33
- Yim, D. 37, 58
 Yiftachel, O. 185, 203
 Young, M. I. 15, 248, 260
- Zabusky, S.E. 234, 261
 Zonabend, F. 180, 203

Subject Index

- access 4-6, 177, 214, 235
 refusal to grant 139-40, 186, 199
 returns for 32, 64, 92-3, 199, 229
 to a laboratory 63-4
 to ambulance service 224-5
 to businesses 5, 31-2
 to home lives 7, 74, 91
 to rural women 168
- advocacy 160, 178, 199, 208, 249
- AIDS, *see* HIV
- alienation 55
- anthropology, social 1-3, 6, 11, 19-24, 32, 35, 234, 236, 238
- applied anthropology 5, 32, 103, 205, 219, 252; *see also* policy
- autonomy 40, 42, 44, 46-7, 53, 205, 218, 240
- back stage, *see* front stage
- Bangladesh 175-7
- behaviourism 127-8
- Brittany 13, 20, 23
- bureaucracy 2-4, 20, 100, 241, 243, 251, 254
- business studies 1, 19-21, 25, 29, 32, 87, 243, 247
- capitalism 36, 57, 78, 183
- careers 32, 40-2, 49, 52-3, 62, 66, 254
- case studies
 of ambulance service 224-8, 252
 of businesses 38-56, 237-8, 245
 of care for disturbed children 118-34, 242
 of clinic 142-55, 238, 246
 of court hearing 105
 of divorce 111-12
 of failed access 139-40
 of international development 167-8, 170-2, 174, 249
 of laboratory 63-71
 of local planning 188-99, 249
 of museum 79-85, 242
 of urban community development 206-19
- case study method 104, 167, 223, 244, 250
- children 8, 51-2, 100, 102, 105, 108-9, 112, 118-34, 172, 242
- classification 5, 8, 69, 117-18, 119-26, 242, 247; *see also* metaphor
- 'client perspective' 2, 85, 90, 101, 113, 242; *see also* multiple voices, symmetry
- community 103, 106, 163, 169, 174, 177, 196, 208, 248
- comparison 11-13, 38, 54, 247-8
- complexity 11-12, 208, 255-6
- defies questionnaires 29
- of changing development discourse 165
- of dependence on organizations 36
- of ethnographic findings 2, 92-4, 160, 164, 175, 177, 184, 199, 239
- of welfare agencies 99-100, 120, 128
- computers, *see* IT
- confidentiality 28-31, 92, 118, 147-9
- conflict, *see* factions, multiple voices, politics
- consultants/consultancy 5, 19, 29, 32, 85, 88
- ethnographers contrast with 93-4
- medical 138, 146
- 'covering' 224
- cultural studies 6, 82-3
- culture
 anthropologists as experts in 21, 37, 78, 113, 234, 236-8
 definitions of 238-9
 essentialized 4, 21, 234, 238
 managerialist view of 4, 244
 of different organizations 35, 56-7, 62, 100, 113, 141, 224, 244-7
- democracy 183, 185-6
- democratization 2, 103, 113
- development
 agencies 160, 162, 174, 176, 249
 case studies of 167-8, 170-2, 174, 249
 international 2, 159-63, 176-8, 183, 199, 208, 248-9, 256

- in UK 186
- diachronic method 24, 26–30, 86, 88, 164, 173–5; *see also* ethnography (as a process)
- disputes, *see* factions
- divorce 99–114, 248
- economics/lists 7, 13, 19–20, 22–4, 27, 29, 36, 238
- education 37, 118, 131–2, 162, 169
- emergency workers 224–6, 252
- emotions
- constitute organizations 35
 - distancing of 144–5, 194–5, 246, cf. 226–7
 - fathers and 105
 - of fieldworker 146, 226–7, 229
 - townplanning and 189, 191, 194–5
- ESRC 21, 79
- ethics 5, 7, 10, 28, 30–2, 54, 223, 227–9, 250–3
- ethics committees 5, 228
- ethnicity 13, 20, 23, 215, 219
- ethnography
- as a process 2, 11, 26–30, 80, 82–3, 87, 103–5, 119–20, 164, 175, 186
 - as interpretation 9–10, 90, 104, 238–9
 - constraints on 35–8, 253
 - defined 1, 9–10, 78
 - interdisciplinary 239
 - multi-sited 7, 13, 72–3, 104, 107, 113, 199–201, 247, 248–9, 255, cf. 253
 - nature of 6–11, 78–82, 86–90, 104, 240, 248, 252, 266
 - of organizations 5, 10, 35–6, 61–2, 73–4, 117–18, 233–5, 253–4
 - partial nature of 8, 11–12, 74, 91, 104, 162–3 *see also* complexity
 - realism and 238, 256
 - spread beyond anthropology 1–2, 234–5
 - writing up 10–11, 93–4, 217–18, 249, 252–3
- exclusion 83, 126, 169–71, 174, 177–8, 191, 224
- experts
- fieldworker as 66, 73, 201
 - in development 160
 - in divorce 100–1, 107, 111, 113, 248
 - planners as 192
 - to be treated symmetrically 9, 200
 - see also* consultants, planners, psychiatry, psychology, psychotherapy
- factions 4, 128–34, 137–8, 144–5, 174, 242, 250–1; *see also* politics
- fieldwork, *see* ethnography, participant observation
- fieldworker 5–7, 32, 186
- as apprentice/student 6, 65, 74, 200–1
 - as participant 39, 139, 145–6, 160–1, 205–6, 211, 224–5, 227–8
 - in a laboratory 64–7, 73–4
 - in Process Research 173–5
 - in research team 101–2, 139, 173
- finances 4, 12, 40–1, 79, 82, 102, 131, 149–51, 212–13, 246, cf. 36–7; *see also* market
- Finer Report 109, 113
- France 30–1, 233
- front stage/back stage
- ambulance crews and 224–5
 - confidentiality and 92
 - defined 86, 215–16, 241–2
 - ethnography must study both 9, 86, 88, 106, 170–1, 216
 - in IT 63
 - in Science Museum 89–90, 92
 - see also* public/private
- functionalism 24, 35; *see also* holism
- funding, *see* finances
- gatekeepers 5, 214; *see also* access
- gender 9 160, 215, 219
- in business 40, 42–3, 54, 241, 245, 251–2
 - in Science Museum 82, 84, 89
- metaphor and 108
- PRA and 170
- see also* masculinity
- genetics 62–5
- geography/ers 1, 234
- globalization 234, 243
- government
- agencies of 4, 12, 100, 105–6, 162
 - business and 255
 - costs of 4, 102
 - HIV and 151
 - local 120, 183–4, 186–7, 190
 - reports 247
 - sanctifies marriage 103
 - use of research 102
 - see also* ESRC, NHS

- Hawthorne studies 3, 236
- health 162, 168
- hierarchy
- defines organizations/firms 22, 36, 164, 169, 177–8, 245
 - in clinic 128, 138, 143, 246
 - in laboratory 62
 - in museum 79, 91
 - in villages 172
 - varies between organizations 213, 251, 254–5
 - see also* careers, power
- HIV 137–8, 144, 149–55
- holism
- attacked 7, 234
 - characteristic of anthropology 9, 20, 24, 26, 78, 90, 103, 239
 - conflicts with confidentiality 29
 - methodological vs. descriptive 7–9
 - presupposes a 'community' 103
- hospitals 3, 63, 137–55, 226–7, 242, 246
- impression management, *see* front stage
- India 9, 160–1, 163, 167, 171, 173–4
- interviews 5–6, 8, 37, 63, 106, 172
- semi-structured 7, 80, 166, 176, 223
 - unstructured 9, 23–9, 32, 104, 139, 223
- irrigation 161, 167, 174
- IT 62–4, 66–70, 73–4, 205–7, 211–12, 246, 255; *see also* technology
- kinship 101, 103, 105–6, 172
- laboratories 4, 5, 11–12, 61–74, 246
- language 9, 20, 23, 105, 113; *see also* metaphor
- Latin America 167, 174
- lay understandings 9, 82, 84, 224; *see also* experts
- legislation 41, 43, 99, 113, 184, 193; cf. 109
- management studies, *see* business studies
- managerialism
- brings market to welfare 12, 149–50, 254, cf. 37, 243
 - in Science Museum 87–8, 90–1, 93
 - in sponsoring research 102–3
 - on culture 4, 244–5
 - see also* business studies, consultants, planners
- managers 150
- control exhibitions 85, 93–4
 - control research 145–6, 176–7, 199
 - participant observation with 38–57, 199, 241
 - resistance to questionnaires 25, 212
 - sex and 38, 47–52, 55–6, 143, 145, 245
 - styles of 53–4, 212–13, 245, 254–5
 - unstructured interviews with 23–30, 51
 - see also* hierarchy, power
- Manchester shopfloor studies 35
- maps 69, 71, 166, 168, 199
- market
- businesses oriented to 4, 12, 39–41
 - firm vs. 22
 - globalization of 234, 243
 - influence on family 104
 - in NHS 131, 149–50, cf. 254
 - see also* managerialism
- Marxism 35
- masculinity 40–1, 43–4, 46–7, 54–7, 252; *see also* gender
- Melanesia 11–12, 35, 47
- metaphor 8, 35, 99–100, 105–12, 201, 235, 248
- migration 104, 162, 168, 234
- mission statements 4, 88
- models 9, 21, 23, 27–8, 91, 94, 166, 168, 225
- Monks Report 152
- morality, *see* ethics
- multinationals 22, 28, 38
- multiple voices/viewpoints
- anthropologists should explore 9, 94, 184, 199–201, 219, 234, 244
 - in development 159, 162–3, 184, 191, 208
 - on exhibitions 83
 - on IT 73
 - on planning 184, 186
 - within clinic 127
 - within firm 26
 - see also* symmetry
- narrative(s)
- compulsions of 26, 28–9
 - construction of 71, 110, 121, 125, 127, 246
 - ethnography as 1
 - learning specialist 65–7
 - outcome of ethnography 11, 235, 238, 244, 247, 256
 - study of 106, 240
 - see also* rewriting

Subject Index

- National Health Service 4, 21, 131, 144-5, 149, 224
- networks
 anthropologist as part of 102, 229
 of experts 100, 102, 165, 185, 215
 social reality to be studied 11, 22, 114, 125, 185, 199-200, 208, 247-8
- NGOs 151, 160-2, 174-6, 205-14
 types of 160, 208
- note-taking 28, 80, 92, 104, 175
- objects, material 85, 87
- offices 20, 39, 45, 66, 69, 80, 254
- oral transmission 68, 175, 213-14
- organizational analysis 206, 208-18
- organizational studies. *see* business studies
- organizations 2-5, 10-12, 35-8, 61-2
 information in 176-8
 purposes of 3-4, 117-18, 120
 'regression' of 86, 242
 types of 12, 240
 types of organizational study 37, 239-40, 243-4, 247-8, 250-1, 253-6
 visitor role in 5
see also ethnography, factions, hierarchy, managers, NGOs, politics
- participant observation
 difficulties of 7-8, 32, 73-4, 139-40, 225-9, 252
 gains of 73, 86, 88, 171, 173-4, 201, 225
 hallmark of anthropology 20, 223
 hallmark of ethnography 1, 6, 23, 81, 86, 88
 in development agencies 161, 171, 173, 174-5, 177
 interviews as close to 6-7, 29
 presupposes a 'community' 103
see also ethnography, fieldworker
- participatory research 159, 163-73, 249
- planners 186-7, 191-6, 249
- policy
 divorce and 101-2, 113
 ethnography of 159-60, 175-6, 199-200, 249
 on housing 183-4, 186-7
see also applied anthropology, government
- politics
 local 188, 196-8
 organizational 85, 89-90, 131, 159-61, 177, 208, 215-19
see also factions
- positivism 2, 20-1, 23-4, 37, 165, 177, 244
- postmodernism 235, 254
- power 9, 170-1, 176-8, 214-15; *see also*
 hierarchy, politics
- pressure groups 184, 190-1, 194-5
- process research, 163-4, 171-6; *see also*
 ethnography (as a process)
- professionals, *see* experts
- promotion, *see* careers
- psychiatry/ists 119, 125-7, 132, 145, 154, 228
- psychology/ists 1, 19-20, 37, 109, 118-20, 132, 233
- psychotherapy/ists 2, 109, 118-19, 122, 125-32, 154; *cf.* 251
- public, the 79, 85, 197;
see also lay understandings
- public interest, the 192-3, 196, 254
- public/private dichotomy 48-52, 54, 56-7, 100, 103
- questionnaires 5, 23-6, 28-30, 212, 240
- rationality
 economic 22-3, 36-8, 57, 189, 211, 236, 245, 250, 251
 lack of 45, 67-8
 technical 40, 164, 191-2, 195-7
- reciprocity, *see* access (return for)
- reflexivity 8, 89, 94, 161, 163, 165, 227, 229, 237
- report-writing 5, 92-3, 175, 217-18, 252
- rewriting history 27-9, 46, 89
- ritual 11, 67-8, 103, 240, 242, 245, 247
- science, natural 2, 11-12, 61-2, 65-6, 77, 82, 84, 86, 90-1
 ethnography of 71-4, 200
see also laboratories, positivism
- Science Museum 77-93, 242
- secretaries 40, 42-3, 48
- sexuality 8, 38, 47-52, 55-6, 141-4, 147-8, 219, 246
- sociology/ists 1, 3, 13, 19, 37, 233, 234, 236, 238
- state, the, *see* government
- statistics 21, 23, 27, 39, 62, 247
- stories, *see* narrative
- superstitions 84-6
- symbols 35, 38, 55-6, 201, 244, 246, 248
- symmetry 11-12, 200, 237, 249;
see also multiple voices

Subject Index

- teamwork 255
- technology 165, 176, 246-7, 255; *see also* IT, science
- therapy, *see* psychotherapy
- time, attitudes to 26-8, 70, 130, 154, 217, 237
- trade unions 47, 77, 162
- triangulation 166
- USA 21, 30, 226, 228, 233, 241
- welfare 12, 99-100, 120, 128
- workers 40-1, 45, 199, 213