INSIDE ORGANIZATIONS

ANTHROPOLOGISTS AT WORK

EDITED BY
DAVID N. GELLNER
AND ERIC HIRSCH

AFTERWORD BY
JOHN VAN MAANEN

Most of us work in or for one, but there are surprisingly few sustained analyses of the problems and peculiarities of organizations. Anthropologists are increasingly turning their attention to the study of western organizations, and this timely collection addresses the pleasures and pitfalls of ethnographic research undertaken across a range of organizational contexts. From museums to laboratories, health clinics, and multinational businesses, leading anthropologists discuss their fieldwork experiences, the problems they encountered, and the solutions they came up with.

This book highlights the practical, political, and ethical dimensions of research in organizations. Among issues vividly described are the relations between gender and politics in organizational hierarchies. How are sexual politics played out and experienced in health clinics? How does a business manager's personal biography affect relationships within the organization as a whole? How are language and metaphor used to refigure the way people think about and act in organizations? Institutions often have well-defined procedures for bringing in visitors and guests. When is the anthropologist an insider to the organization, and when an outsider? What ethical issues arise when researchers are caught between observing organizations and participating in their work?

In answering these and other questions the authors consider both the current status and future prospects for organizational ethnography. Comprehensive and varied, the book represents an invaluable aid to anyone interested in the politics and complexities of working life.

David N. Gellner and **Eric Hirsch**, both at *Brunel University*

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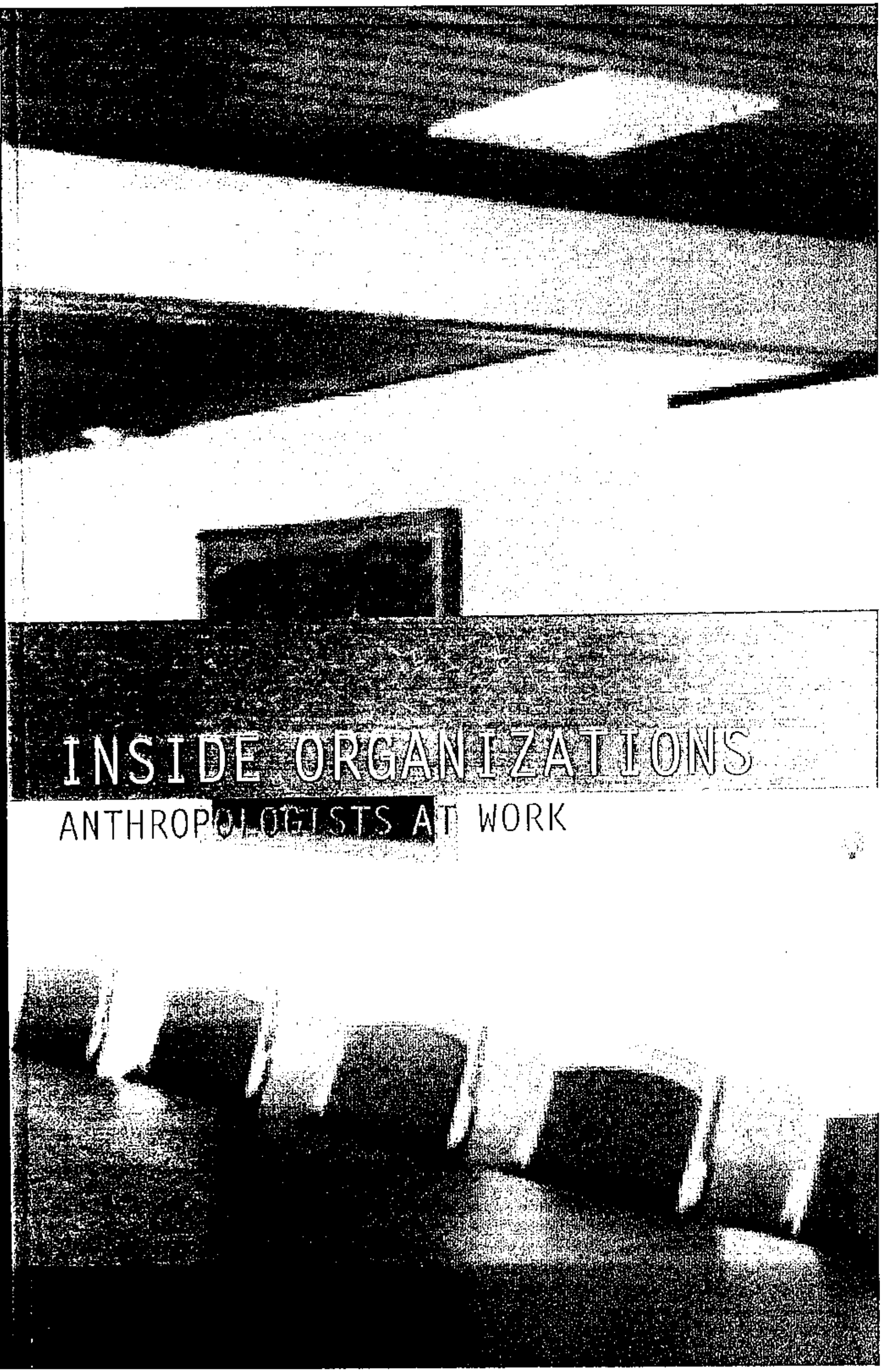

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Bob Simpson is a Senior Lecturer in anthropology at the University of Durham. He has a long-standing research interest in Sri Lanka where he carried out doctoral research into healing rituals and the transmission of ritual knowledge. He subsequently carried out research into many aspects of divorce and separation in the UK and published numerous articles and a book (*Changing Families: An Ethnographic Approach to Divorce and Separation*, Berg, 1998) on contemporary family life. More recently he has undertaken research into kinship and the new reproductive and genetic technologies in the UK and Sri Lanka.

John Van Maanen is the Erwin Schell Professor of Organization Studies in the Sloan School of Management at the Massachusetts Institute of Technology. He has been a visiting professor at Yale University, the University of Surrey, and INSEAD (Fontainebleau). He has published in the general area of occupational and organizational sociology. Cultural descriptions figure prominently in his studies of the work worlds of patrol officers in the United States, police detectives in London, and park operatives in Disneyland. His books include *Tales of the Field* (1988) and, most recently, *Qualitative Studies of Organizations* (1998).

Acknowledgements

This book grew from a project sponsored by the National Network for Teaching and Learning Anthropology, directed by Sue Wright. Project FDTL 124/96, originally conceived by Professor Adam Kuper, rejoiced in the sober title 'Short-term ethnographic research in industrial and organizational settings in connection with undergraduate work placements and Master's dissertations'. (The keen ethnographer of anthropologists' organizations might begin by deconstructing it.) The aim of the project was to improve the teaching of ethnographic methods (a) to Brunel Human Sciences undergraduates, all of whom have to do two 5–6-month work placements as part of their degrees (and many attempt to do some ethnography), and (b) to MSc students in medical anthropology and the anthropology of childhood, all of whom have to write an ethnographically based dissertation. We began by drawing up a wish list of all the anthropologists we knew who had done research in organizations, and then invited them to come and speak to our students during the academic year 1997–8. We were delighted by the number who responded positively and by the quality of the presentations. Although in many cases substantially reworked, this volume contains chapters by nearly all our speakers; Martin O'Neill's ethical case study (Chapter Eleven) and John Van Maanen's Afterword were added subsequently.

We are grateful to Adam Kuper and Sue Wright for their support and guidance, to the National Network for Teaching and Learning Anthropology for funding the project, to the contributors for their cooperation and patience, and to Eric Macfarlane, Mitchell Sedgwick and Lola Martinez for advice. Perhaps our greatest debt is to the generations of Brunel students whose work-placement experiences have fed our interest in the ethnography of organizations, and in particular to those students who participated when the chapters of this book were originally presented.

David N. Gellner and Eric Hirsch
Brunel

Introduction: Ethnography of Organizations and Organizations of Ethnography

Eric Hirsch and David N. Gellner

Ethnography

Social anthropology has some claim to have discovered ethnography as a method (Stocking 1992), and to be the only discipline so far to have put it unequivocally at the centre of its research activity. But can methods devised and refined in the early days of anthropology for the study of non-industrial societies be as successful and revealing in the study of organizations in the industrial world? What are the problems and what are the implications of using ethnography in such settings? And what are some of the solutions? It is with these questions that the present collection is concerned.

Ethnography is now a popular term. There is ethnography of the classroom (Hammersley 1983), ethnography of television audiences (Morley 1988), ethnography of medical students (Sinclair 1997), and ethnography of the police (Young 1991). But what exactly is it? Undeterred by the almost religious mystique with which some anthropologists used to surround the term, we may say that 'ethnography' refers (1) to a set of activities, a way of doing research work 'in the field', and (2) to the *product* of those activities. It is the activity which comes first. Thus the practice of *ethnographic fieldwork* – fieldwork in which the researcher engages with the people being studied, shares their life as far as possible, and converses with them in their own terms (ideally in their own language, i.e. without an interpreter getting in the way) – gives rise to *an ethnography*. This was conventionally a book, sometimes called an *(ethnographic) monograph*, that describes in detail the social life of a particular place or institution. It is our impression that usage number (2) – talk of '*an* ethnography' – is confined to anthropological circles, whereas the other usage – ethnography as participant observation, i.e. as qualitative field-research method – is much more widely known, being current in sociology, psychology, geography, and organizational studies.[1]

1. For an excellent survey of the increasing impact of ethnographic method within organizational studies, see Bate (1997). He distinguishes ethnography (1) as an activity from (2) ethnography as a kind of intellectual effort or paradigm, and (3) ethnography as narrative style; or, more pithily: ethnography as respectively doing, thinking, and writing. We tend to think that there is no one ethnographic style of thinking or writing, but rather different styles of thinking and writing based on ethnography.

The emergence of ethnography as a method in so many different disciplines in recent years can hardly be put down simply to the influence of anthropology. More important is the general intellectual history of the human sciences, which may broadly be characterized as a long march away from positivism: that is, away from attempting to model them upon, and justify them in terms derived from, the natural sciences.[2] Thus, apart from any specific demand for doing ethnography, there has been, quite independently in different disciplines, the discovery of 'the client's point of view', which may be seen as an aspect of growing democratization or critique of established relations of power.[3] This development in the human sciences doubtless reflects a still broader cultural democratization. Bate (1997: 1162) quotes the BBC journalist, Michael Buerk, reflecting on his reports of the first Ethiopian famine fifteen years earlier: today the victims would have to be given the chance to speak for themselves, to put their own point of view.

Yet, despite this evidence that many disciplines are beginning both to be dissatisfied with the jejune results of positivist paradigms and to be aware that careful ethnographic work is likely to bring far greater insight, there are reasons to believe that ethnography will always have an air of subversiveness about it. Alongside the postulated general cultural movement towards democratization, there is a countervailing trend towards control, measurement, and quantification of outputs. Any 'method' that insists it lacks a cut-and-dried technique, any discipline that grants a central position to the voices of the 'client' and refuses to prejudge what they might say, will always be suspect to powerful organizations.

One of the aims of this introduction is to consider the various 'moments' involved in conducting ethnography – the ethnographic process – particularly as these are relevant to the study of organizations. As we intend to show, the practical issues involved in such studies (e.g. access) cannot be divorced from important questions of theory (e.g. how boundaries are constituted).

Organizations

Organizations are many and various, but they all have explicit rules, a division of labour, and aims that involve acting on or changing everyday life. Weber famously posited his ideal type of bureaucracy as the most rational – that is, the most efficient and dispassionate – form of organization, because of its high degree of special-

2. Anthropology, of course, also participated in this long march. Malinowski had justified his ethnography in positivist terms, as did Radcliffe-Brown even more unequivocally. But the practice of ethnography has survived and flourished, long after Malinowski's positivist defence of it has withered away (Stocking 1992: 240–1).

3. See Mayer and Timms (1970) for social work, Chambers (1983) for international development, and Mitchell (1993) for psychotherapy.

ization and its rule-governed behaviour. Sociologists ever since have delighted in showing, with ethnographic attention to the everyday and the small-scale, how participants in bureaucracies act to undermine efficiency, either by sticking blindly to the rules, or by secretly undermining them and the chain of command. Weber's ideal type may well have been flawed when taken as an account of how bureaucracies actually work, and he may well have been mistaken to imply that the more bureaucratic an organization is, the more efficient it is. But Weber's ideal type captured an important insight, namely the organizational aspiration to be separate from the values and norms of non-bureaucratic, everyday life. Organizations are based on rules that are consciously set up and sometimes changed. Unlike other social institutions,

> Even the smallest organizations necessarily involve conscious monitoring and control of the relationship between means and ends on a fairly regular basis. Such monitoring and control implies a conscious disciplinary process whereby organizational leaders adapt internal structures to their perception of external conditions (Morgan 1990: 5).

Whereas organizations are ubiquitous in the modern world, in the societies where anthropologists began to do ethnography there was plenty of social organization but few or no organizations. Indeed British social anthropology came to be identified with the demonstration of how predictable social life could be carried on without the specialized order-enforcement agencies (police, courts) familiar in 'developed' countries. In the 'tribal' world, social organization focused on the household and on kinship. There was little that went beyond it, little that aspired to separate people from the realm of kinship: age sets, unions of shamans, occasional messianic movements perhaps, but not much else; and even these did not usually involve anything akin to a salaried personnel. In the modern world, by contrast, organizations are everywhere, including those three that, even as the early ethnographers worked, were already being introduced into the societies they studied: the church, the school, and the hospital.

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Given that anthropology began as the study of the non-modern in societies without organizations of the sort described, can it contribute anything to their study? In fact social anthropologists in Britain have long carried out, or been connected to, studies of industrial and other organizations, even though these studies have rarely been acknowledged as part of the history of the discipline (Wright 1994). The same has been true on the other side of the Atlantic: American anthropologists were centrally involved, for instance, in the controversial Hawthorne Studies in Chicago during the 1930s (Gillespie 1991). This volume aims to show how anthropologists in Britain today are bringing a distinctive ethnographic expertise to the study of modern organizations (in many cases having first served an apprenticeship doing a classic 'village study').

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As implied above, organizations, of whatever kind, usually have some sort of governing ethos. Recent managerialism has tried to codify this ethos, to impose a 'mission statement' which would encapsulate this ethos and 'martial the troops' by means of a single 'culture'. This crude Durkheimianism has recently been usefully contextualized and critiqued by Wright (1998). In contrast to business managers, anthropologists are inclined to look for evidence of different views and divergent interests, even if these are based on shared assumptions about how things should work out (Edwards 1994). Despite a shared governing ethos, many organizations experience, at the same time, entrenched factional 'warfare' between constituent parts (see Chapters Six, Seven, Ten below). Such factions often come together and act as one when the institution as a whole faces opposition or scrutiny from outside.

Organizations do not exist in a vacuum. They operate in a wider context which both provides them with the aims they pursue and sets limits to the way they may operate. This context may be recurrent government funding shortages in the National Health Service or elsewhere (Chapters Four, Six, Seven), it may be the capitalist market (Chapters One and Two), competition between laboratories and individuals for results (Chapter Three), political competition for resources (Chapter Ten), the need to be able to demonstrate 'doing good' through measurable indices (Chapter Eight), or the spread of managerialism to government-funded agencies. Rather than working inside an organization, the anthropologist sometimes researches at the 'interface' between organizations and 'the people' (Chapters Five, Eight, Nine), a situation for which anthropological skills and awareness can be argued to be particularly suited.

In his afterword John Van Maanen discusses some long-standing and recent lines of inquiry in the ethnography of organizations. He not only suggests a way of thinking about different ways of studying organizations, but also, in reviewing much recent work by anthropologists or social scientists influenced by anthropology, provides a valuable bibliographic survey of the field. Rather than attempt to duplicate what Van Maanen has done so effectively, here we concentrate instead on some of the key issues involved in the ethnographic study of organizations. These practical issues and their theoretical implications have concerned the contributors to this volume, as well as many of the authors discussed by Van Maanen.

Inside or Outside? Issues of Access

Conventional fieldwork in places such as Melanesia, Asia, Africa, or Latin America often involves lengthy negotiations with state and local bureaucracies regarding access to field sites, length of stay, and so on (Clifford 1997: 22–3; Law 1994). These negotiations and the documents and persons involved are usually left out of

ethnographies. But some anthropologists are now beginning to include descriptions of this process of gaining access because it has come to be realized that the behaviour of such organizations provides important insights into the way particular places are locally conceptualized, bounded, and resourced. The organizations studied here have many formal similarities to these 'gatekeeper' organizations in non-Western contexts. And indeed similar issues of access arise: boundaries and identities have to be negotiated, and the duration of stay and focus of research have to be cleared, as several of the following chapters illustrate. For the ethnographer, adaptability to the circumstances is essential, since, as Buchanan *et al.* (1988: 56) remark, 'negotiating access for the purposes of research is a game of chance, not of skill'.

Shared assumptions within organizations often mean that there are precise expectations of the researcher who stays at length within it. Ouroussoff's experience of general indifference to her research providing she fulfilled the duties of her job (Chapter Two) may be untypical. Large organizations often have experience of management consultants. This may mean that the anthropologist is regarded as a potential enemy by staff, who assume that he or she is a management consultant whose report will recommend redundancies (Chapter Four); everyone expects a report with an executive summary listing bullet points for action. That there should be such a report is in some versions of 'applied' anthropology the whole point (Chapters Eight and Ten).

Particular individuals may refuse access; but the planners, whom Abram (Chapter Nine) expected to be hostile, surprised her with their openness. Certain categories of individual, as has often been described in the sociology of marginal groups and as Parker (Chapter Seven) vividly illustrates, simply do not wish to be studied. Some organizations, such as research laboratories (Chapter Three), have an established procedure for integrating interested outsiders, but once the outsider appears to take on 'insider' status, problems of appropriate categorization become apparent. The longer the researcher hangs around, the more he or she participates, the harder it is to assimilate him or her to the conventional model of a researcher armed with clipboard and questionnaire. People who were comfortable at first may start to worry about the researcher as the research goes on.

Access is therefore not something to be negotiated once and then forgotten about (as the apparatus of ethics committees confronting medical researchers might suggest). It is, on the contrary, something that has to be both scrutinized for the way it transforms the research and continuously negotiated throughout the time of fieldwork. It is inconceivable, for example, that in-depth open-ended interviews could be conducted without the researcher explaining what he or she is about and gaining the interviewee's consent and cooperation. It is the impossibility of interviewing without consent which is – quite apart from ethical considerations – the main objection to covert ethnography (Sanjek 1985). It may be, however, that despite the ethnographer's attempts to explain him- or herself, he or she continues to be slotted into the easily understood category of student, a role sufficiently close to the researcher's that it can conveniently be accepted (Chapter Nine); in many cases, of course, researchers actually *are* students.

It is widely agreed that successfully conveying a sense of 'being there' to the reader is a mark of good ethnography.[4] But what does 'being there' mean or entail? In discussion, Malcolm Chapman made the point that no one (no authority) can stop you renting a flat in a French village, whereas there are numerous barriers to gaining access to a business, for example. And yet, the mere fact of residing in the village does not mean that people will let you into their homes or their lives. Particular thresholds or boundaries need to be transcended. What are these? They are certainly spatial (e.g. being 'let in') and temporal (e.g. people allowing one to be a part of their activities). The most significant, perhaps, is that one has gained some insight or understanding into 'the native's point of view', i.e. into how the world looks or is perceived from the perspective of the people themselves. What is this context, what are its boundaries, how are these determined and how are these connected to the local understanding thereby attained? They are certainly spatial and temporal, but this lived experience of space and time is structured as such through particular concepts, ideas, statements, and the way these are realized in particular social relations. These are the connections at the basis of ethnography and upon which ethnographies (whether the brief case studies found here or the expanded examples developed in monographs) are constructed: they 'speak to the truth of how things relate to one another' (Strathern n.d.: 9).

Specifying Ethnography

What are the differences between doing ethnography and doing interviews? Both can result in the creation of 'an ethnography', so students are understandably perplexed and want to know how they can tell that they are really 'doing ethnography'. Is only sustained participant observation in an organization for at least one year necessary before research can be called ethnography? This is a question which goes right to the heart of current debates between anthropology and fields such as cultural studies that also advocate ethnographic research.

Chapters One and Two, both conducted in business organizations, take up different positions on this issue; and the issue recurs elsewhere in the volume. Chapman (Chapter One), while recognizing participant observation as the ideal, believes that repeated interviewing can achieve ethnographic depth. Ouroussoff (Chapter Two) argues that research based on interviews can only result in ethnography if it is unusually sensitive (e.g. Le Witta 1994). Others suggest that

4. Geertz (1988), Miller (1997: 16; cf. Chapter Four below), Bate (1997: 1163).

the relationship between ethnography and interviews is more of a continuum (Chapter Three).

Where anthropologists cooperate with economists to conduct research in manufacturing businesses, even such minimally qualitative procedures as semi-structured interviews are perceived as radical (Chapter One). Is it then a question of what one is trying to achieve in the research which dictates whether 'immersion' or more scheduled encounters are appropriate? Furthermore, should the ethnographer pursue his or her enquiries into what happens 'after hours', outside of work, and how this is connected with life inside the organization? There are certainly advantages to so doing, but only a few of the contributors to this volume were able to achieve this. The ethical questions that arise from doing ethnography are not the main focus of this book, but they cannot be avoided. We include an evocative personal account of practical and ethical problems which the ethnographer trying to combine observation and participation may face – dilemmas that became especially acute in this case because the ethnographer routinely found himself in life and death situations (Chapter Eleven).

Perhaps the key to doing ethnography, whether based on a long-term stay or interviews, is the achievement of an empathetic of understanding similar to the therapeutic situation (cf. Chapter Six). This still leaves unanswered the question of the distinctive nature of an ethnography (cf. Thornton 1988, Strathern 1991). It lies, we argue, in its commitment to *methodological* holism – that is, to accepting that in principle anything in the research context can be relevant and could potentially be taken into account – while simultaneously recognizing that *descriptive* holism – the assumption that all social contexts are tightly interconnected social wholes (cf. Clifford 1988: 104) – is in most cases misleading. Indeed some have argued that descriptive holism is ideologically loaded and especially inappropriate in the light of today's increasingly 'multi-sited' contexts of ethnography (Marcus 1995). The consequence for the researcher is clear: he or she needs to adopt a curious kind of cross-eyed vision, one eye roving ceaselessly around the general context, any part of which may suddenly reveal itself to be relevant, the other eye focusing tightly, even obsessively, on the research topic. This is what Miller (1997: 17) calls the commitment to holistic analysis and Hine (Chapter Three) calls 'learning from looking elsewhere' in order to achieve a 'rich', contextualized understanding of whatever it is that one is supposed to be researching.

Finding a Focus

Ethnographic research conventionally takes shape around an initial plan or proposal of research, sometimes based on preliminary knowledge of the fieldwork site(s). However, there is often a disjunction between such intended research and what happens in practice. Regardless of the extent to which a plan is adhered to, how does one set about establishing an appropriate focus? The potential data are infinite; one cannot befriend, or even interview, everyone (unless the organization studied is extremely small); so where should one direct one's attention? This can be an extreme problem (Chapter Ten) but in most cases the people themselves within the organization – those with whom one is having day-to-day contacts and/or interviewing – will suggest the most appropriate focus. To be in a position to appreciate this, one needs to be open-minded and attentive, prepared to engage with 'native' categories and representations. Each organization will have its own preoccupations whether they be objects and visitors (Chapter Four), managing disturbed children (Chapter Six), or sexuality and suffering (Chapter Seven). This is why the ethnographer has to spend time being open to the concerns of the people studied, to spend time *not* being directive. It often then turns out that a small set of key concepts provide the crucial insights needed to gain an 'ethnographic' understanding of the organization (e.g. Simpson's discussion of the metaphors used by divorcing couples in Chapter Five). Such key concepts appear to 'hold things together' or to define the terrain that is being contested.

Focus is achieved with effort and empathy. Recorded information/data and one's recollected perceptions provide the critical, interactive setting where 'themes' are enabled to emerge out of a particular focus. In short, one is searching for a 'pattern' both in the data/perceptions and by implication in how the people studied present order and systematize their lives. Although ethnographers try to be systematic in some respect or other (surveying all households in a locality, or interviewing all the nurses on a ward, for example), this has less importance than the methodological holism mentioned above. To know every nook and cranny of a place is neither conceivable (even Malinowski was not able to produce a synthetic portrait of the Trobriands) nor, fortunately, necessary. To suggest this is to highlight an important tenet of anthropological description and understanding: that the focus derived from those with whom you have entered into relations is a focus they in turn use to order their lives and relations within (say) the particular organizational context.

It is clear, as many have argued, that a degree of reflexivity is essential for the ethnographer. The researcher has to be clear about his or her objectives and the limitations he or she is working under. Otherwise it will appear, as every practising anthropologist has surely felt, that one has failed to gather 'enough data'; at the same time one is faced by 'too much data' and the problem of how to 'write it up'. These paradoxes and frustrations are doubtless avoided by followers of more formal methodologies.

Doing Ethnography

It is part of the point of ethnography that there will never be, and cannot be, total agreement over what is the best way to go about it. But this does not mean that

nothing can be said. For the sake of argument we can start from the position of Bate (1997) and expand. Bate suggests that good ethnography

1. conveys the sense of 'being there' (discussed above);
2. produces details and conclusions that are unexpected (we would add that this is the payoff for the – to some people – frightening prospect of research with open-ended questions);
3. reflects the polyphony – the multiple voices – of the real world;
4. offers a model or theory: it is not just for entertainment.

διαχωρίζεται ανθρωπολογία δημοσ.

This last feature, or something like it, is necessary to distinguish anthropology from travel writing or journalism. Certainly much travel writing conveys better than many anthropologists do the sense of being there: but whose sense of being there? The very best travel writing – one thinks of V.S. Naipaul's *India: A Million Mutinies Now* – conveys a vivid sense of place and of the very many people whom the author met and spoke to. It might be accepted as a kind of ethnography since it is so evidently (at least overtly) about Indians and not about the author himself. However, it is less than wholly candid about how the material was collected, and it does not really offer a 'model' or theory.

To Bate's list one might add that good ethnography displays at least some of the following virtues. It

5. contextualizes its findings (the methodological holism discussed above);
6. pays attention to questions of power and inequality, by examining the ways in which some participants' voices and models prevail over others' (Chapters Eight and Nine);
7. emphasizes both what people say and what they do, and looks for connections and disconnections between the two (Miller's commitment to treating people as material agents: see Chapter Four);
8. does not restrict itself to 'front-stage performances', but pays equal attention to what people do and say when they are 'off duty' and not being watched (Chapters Eight, Eleven);
9. looks closely at how language is used (see especially Chapters Three, Four, Five, Nine);
10. is reflexively aware of the ethnographer's ambiguous position: empathetically trying to get at the points of view of numerous people – specialist and lay, old and young, male and female, powerful and relatively powerless – and at the same time attempting to put these together into some kind of overall pattern;
11. does not simply seek confirmation of what is already known (i.e. does not selectively pick ethnographic illustrations for a position already worked out), but always presents the material in sufficient richness that it can be worked

over by someone else with different interests and different theoretical dispositions; in other words, the writer attempts to make a contribution to 'the ethnographic record' as 'the central shared heritage of anthropology' (Kuper 1994: 117).

Putting it Down and Writing it Up

There are several problems confronting ethnographer of organizations which the more traditional anthropologist may not have had to face. In the first place, in order to carry out the research he or she may be employed in the organization and therefore have to write reports and carry out tasks like any member of the organization. Mascarenhas-Keyes (Chapter Ten) argues that the researcher should adapt the way in which conclusions are presented to the style and ethos of the organization in question, to ensure that they have maximum chance of being taken up. Mosse, on the other hand (Chapter Eight), suggests that there is an irresolvable tension between studying *inside an organization*, and therefore accepting its aims and agenda, and critically *studying the organization itself.* Anthropologists working for organizations may find that their notes on the organization become a kind of covert research which it is hard to publish. Inevitably, different ways of writing, perhaps even different ways of interpreting material, are appropriate for different audiences: these are likely to include the organization itself and academics, as well as various kinds of more popular readership.

But what should be done when these audiences merge into each other? As Chapman remarked in his oral presentation, Godfrey Lienhardt did not have to worry when writing *Divinity and Experience* that the Dinka themselves would read and assess it (though by the time he died many Dinka had indeed come to Britain and of these many must have read it). The researcher on organizations today knows that any ethnography that is recognizable will immediately be read by the people it is about, and every word must be weighed in consequence. As Macdonald describes (Chapter Four), the kind of publication thought appropriate by those studied may be of a very limited and esoteric kind, so that if the researcher publishes a more accessible description of the field site, it may be met by anger or outrage. These are tangled ethical and pragmatic issues, which have to be faced by the anthropologist studying organizations. Those studying more remote peoples may once have been saved from having to confront them, but this is rarely so today. Even the Ik have begun to protest at the way Colin Turnbull depicted them in *The Mountain People* (Turnbull 1972; Heine 1985).

In this book, the various chapters are 'finished products' and little space is devoted to the question of why the text was written up in one way rather than another (Van Maanen 1988, 1995). They are the outcomes of a series of strategic

decisions about how best to describe and interpret the categories and lived experience of those with whom the authors had the privilege of being engaged, in either short-term or long-term relations. These strategies are in part dictated by the established literature and in part by the particular theme or set of themes found significant for presentation; literary techniques for presenting an interesting narrative and argument cannot, of course, be ignored. Intrinsic to this is the issue of closure: all understandings are partial but once this is recognized it is then appropriate to write as if one has achieved a relatively complete understanding of the themes and materials to hand. These may be superseded by subsequent research, but for the particular moment of description and interpretation, it is useful to imagine one has captured the (partial) truths. In short, although a textual entity has been created, one has, more importantly, produced an approximate representation of the way people present and live their lives.

Conclusion: Issues of Symmetry and Complexity

Latour (1993) advocates a 'symmetrical anthropology': this refers to the idea that all institutions and organizations should be studied in exactly the same way, on the basis of the same assumptions. A high-tech scientific laboratory can and should be studied exactly as is a tiny island community: each, he argues, is a particular configuration or network of actors, where the actors can be humans as much as artefacts, lines on paper, or any other created object. Latour claims that what distinguishes such networks is not their difference in kind (science vs non-science) but their relative size or scale and stability. In short, particular actor-networks have the capacity to achieve greater length and durability, but the difference is merely quantitative. For Latour scientific institutions are qualitatively no different from other institutions.

Latour's argument is both sophisticated and seductive. In many respects its spread among social science disciplines parallels the spread of ethnography as a form of research technique and written text. His book *Laboratory Life*, co-authored with Steve Woolgar, was a key text that marked the beginning of ethnography in the post-Kuhnian revision of the study of science. However, as Strathern (1996) has pointed out in her discussion of Latour and actor-network theory more generally, there are important qualitative differences between networks in the way they are 'cut' or bounded. This, in turn, has to do with the socio-historical emergence and continuity of particular conceptual regimes which are simultaneously regimes for ordering the world. Strathern's particular example is 'ownership' and 'property' and the way it places limits or 'cuts' the networks, for example, in scientific discoveries and their patents. This she compares to forms of 'cutting networks' in Melanesian mortuary rituals which are not predicated on Western ownership/property notions. At one level, these networks may appear to be 'symmetrical': Melanesian 'stopping of flow' and patent claims on discoveries or inventions in the West. But once the details of concepts and action on the ground, so to speak, are brought carefully under scrutiny we see important differences that cannot be glossed over. This is as true for the institutions and organizations themselves as it is for the way they come to be represented in ethnography.

There is more than symmetry at stake here: there is also the fact, attested above, that organizations have a view of themselves, both for internal and for external consumption. This means that organizations are not just the sum of their participants' interactions: organizations acquire a life and a momentum independent of the people who make them up. Much organizational time and effort goes into controlling and disciplining those on the inside. These controls can be both subtle and complex (as Macdonald's Chapter Four illustrates). Whether this complexity should be taken to encompass different and incommensurable aims and attainments, or whether, on the other hand, there should be some uniform method for assessing different organizations, some single way – suitably adjusted for different types of organization – of judging their output and assessing whether they produce 'value for money' is one of the crucial issues in the field (Strathern n.d.).

With the exception of Mosse's Chapter Eight, all the chapters deal with organizations within the Euro-American context (and even the 'aid' organizations are Western-based). In a rough and ready fashion it is possible to separate the various case studies of this book into three broad categories or types: (1) those concerned with 'science', where the aim is to produce replicable results (Chapter Three; cf Rabinow 1996); (2) those concerned with business, which is oriented to the market and making a profit (see Chapters One and Two); (3) those concerned with agencies of the state, including the provision of welfare, where the aim is to embody and fulfil the wider values of the society (most of the other chapters).

However, the moment such a division is made, qualifications have to be noted. In the case of science, the laboratory is driven to produce results, but the funding and/or output is often market-driven and so we immediately see the influences of business/capitalism. In the case of the firm a not dissimilar problem emerges. For businesses, the explicit objective is profit or market penetration, but if the firm rides roughshod over the welfare of its staff and/or its consumers then it may soon see profits fall. Finally, state and welfare organizations are increasingly being judged as much by their market efficiency as by their 'care', and 'care' itself is increasingly measured in terms derived from the market. In short, within one organization we can perceive the contours of other, separate but also partially connected, kinds of organization.

When does the partially connected nature of organizations described here become relevant to the non-Western contexts conventionally studied by anthropologists? Perhaps this has always been the case but the Malinowskian tradition 'blocked out' the organizational features of colonialism and missions.[5] The ethnography of organizations may have been ignored by anthropologists in the past because it appeared to capture only a part of people's lives, instead of the 'full' view supposedly attained in a village setting. Furthermore, studying organizations has always seemed the preserve of other specialists: sociologists, economists, historians. Chapman (Chapter One) records his worry that, when doing fieldwork in Brittany, he should perhaps have been studying the local factory and hospital, rather than the more anthropologically conventional, but locally marginal, topics of ethnic identity and fishing.

It is our belief that the same methods can be used for village-level, organizational, and multi-sited ethnography. The chapters below show in a variety of different contexts how this is done, what specific problems arise, how they are addressed, and what the implications are of particular responses to these problems. Whether *the results* of that ethnography are to be seen as demonstrating similarities between small-scale societies and the modern West, or, by contrast, as showing fundamental differences, is in the end a matter of philosophical standpoint and interpretation, depending also on the level and the framework within which the interpretation takes place. What this collection demonstrates, we hope, is that the anthropologists now at work in organizations have a distinctive and valuable set of perspectives to offer, not just on 'tribes' or villages, but on the modern world of organizations as well.

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5. See Asad (1973) for an early statement along these lines.

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Part I
Business

–1–

Social Anthropology and Business Studies: Some Considerations of Method

Malcolm Chapman

A conference was held at Manchester Metropolitan University in April 1998. It was called 'Managing Global Change'. The conference broadly concerned business and management studies, with particular reference to the economies of East and South-east Asia. The conference was an interesting one, with invited contributions from some well-known figures of local and/or international repute: Patrick Minford (economist; see 1998), Peter Dicken (geographer; see 1998), John Dunning (doyen of scholars of international business), Gerald Kaufman (politician), and Bruno Leblanc. The last of these is a scholar, researcher, consultant, and teacher in the world of management education; he is of long experience, and able to work in English, French, German, and Polish. He gave the final address, in which he looked at issues for the future of management education and research. This involved him in looking at the various disciplines which have contributed to business studies – notably economics and psychology. Here he noted an absence. 'Looking back,' he remarked, 'it amazes me that we have never had anthropologists in our faculties of business and management; we need them and their ideas.'[1]

I was sitting next to Professor Leblanc at the time, having earlier made my contribution to summing up the proceedings. We had not previously conversed during the conference, so I had the heartening experience of being told that I was needed by an entirely disinterested source. Anthropologists will recognize that this is not a particularly common occurrence; not, perhaps, since the days of the Colonial Office have anthropologists had any clear instrumental role in mainstream political and economic activity. Economists, sociologists, and psychologists have, in their different ways, achieved this; anthropology has not. An invitation into the heart of business studies, therefore, where so much busy and expensive pedagogy and research is taking place, is not one to be treated lightly.

1. This is a cheat. I pretend to quote verbatim, but am really reconstructing from memory; the reconstruction is not far off, however, and the meaning is faithfully rendered.

I was and am an anthropologist, Oxford-trained from the early 1970s, and particularly influenced by the work and teaching of Edwin Ardener.[2] I studied ethnicity and the Celtic fringe, and have published variously on the subject (1978, 1982, 1992, 1993). I began a move into business studies in 1989 by reading for an MBA at one of the U.K.'s oldest business schools, Bradford University Management Centre. This was not, to judge from the surprised responses of my colleagues, a self-evident career move, and its motivations bear upon the content of this chapter. In the Celtic fringe, I had studied ethnicity and language, and I had studied a fishing village and fishing as a means to this. These were interesting subjects, but there nevertheless seemed to be a great deal of life that I was under no compulsion to study, or that was only at the edges of my vision – very generally, nearly all the things that went on in offices, factories, bureaucracies. Virtually everybody in Brittany had some sort of investment in being (or not being) Breton, but for most this was not something they thought about or cared about very much. What where they doing the rest of the time? I came to the view that my concentration upon a 'traditional' occupation, and upon ethnicity, had in some important ways impeded a study of still more important things. I believe that this was not merely a personal idiosyncrasy, but was built into the subject of social anthropology at the time. The limitation was difficult or impossible to surmount, although with hindsight it is not obvious why that should have been so.

It was, therefore, with a view to overcoming this lingering primitivism in the subject, that I turned to business studies. Social anthropology has a post-war history of concentrating upon primitive societies, of working through the shift from 'function to meaning', of holistic study, of participant observation, and of retreat from numerical analysis. Business and management studies, by contrast, have been late-born aspirants to the positivist social sciences. They have many precedents, and some of the 'founding fathers' (there are no founding mothers) are from the first half of the twentieth century; in general, however, the great explosion in research and teaching was to be in the second half of the century, beginning in the USA in the 1950s and spreading to Europe in the 1960s and 1970s. Business and management academia have been working through their own (as it were indigenous) positivist agenda. For academic resources, in the post-war period, they have looked to social sciences which also aspired to this model: most particularly, to social psychology and to economics (see Buckley and Chapman 1996; Chapman 1996–97). Social anthropology was not an obvious partner, as business and management studies pursued their positivist agenda.

2. For what it is worth as an anthropological credential, I was, I think, the very last to be obliged to endure (or to achieve) the once-classical sequence of degrees (MA, Diploma, B.Litt., D.Phil.), ruinously expensive in both time and money, which Oxford anthropology used to require. Innocent pride to salvage from the wreckage: Evans-Pritchard was similarly qualified.

Researchers within business and management studies eventually started to notice, however, as anthropologists had noticed several decades before in relation to their own preoccupations, that the results were not forthcoming – that the predictive science was being continually postponed, amid ever-repeated calls for further research funding. Faith in the ultimate appearance of a predictive science is still strong, but doubt is growing nevertheless.

In this situation, many authorities are calling for an integration of other social sciences into the weaponry available to management studies – social anthropology is increasingly cited. This is partly driven by the hope that the specialist knowledge provided by social anthropology will allow the positivist agenda to be furthered, as if the obstacle were merely technical. Within cross-cultural management studies, for example, scholars have commonly tried to tease out the variables which might explain differences from one context to another – variables like law, religion, industry structure, organization structure. There has long been an intuition that something like 'culture' might be important as well, and scholars have turned to social anthropology for a definition of 'culture' (vain hope), so that this too can be measured and fed into the multivariate statistical models – one independent variable among others which will help to explain 'behaviour' (see Sekaran 1983; Negandhi 1983; Roberts and Boyacigiller 1984).

We can perhaps predict that nothing much will come from this, for the potential that social anthropology offers is based upon more fundamental features. Social anthropology has already had the experience of being knocked completely off its poise by anti-positivist and anti-behaviourist arguments; it has already had several decades of experience of finding some sort of (fragile) equilibrium in the aftermath of this; it has also, through its own long debate with itself, gained considerable sophistication in constructing arguments aimed at positivist and behaviourist positions. All these features make it a particularly interesting ally for business studies at the present stage: business studies is still predominantly behaviourist and positivist; it is also still predominantly monocultural (and effectively USA-centred). It is, therefore, vulnerable to, and perhaps capable of enrichment from, social anthropological criticism; business studies is currently undergoing something oddly like 'a shift from function to meaning', and social anthropology can offer its own experience and hindsight. So social anthropology and business studies, by their very differences, offer interesting possibilities for cooperation.

I have experienced the meeting of social anthropology and business studies in many ways and many places, in teaching and research. One particularly fruitful context, however, was provided by a research project, funded by the ESRC under a more general programme called 'Contracts and Competition'. This general programme was inspired by a desire to understand how markets work, particularly in the context of attempts to introduce elements of competition and competitive tendering into the National Health Service. The ESRC wanted to know how markets work, so to speak, so that they could be made to work in the British public sector.

The project, 'The Management of Cooperative Strategies', was conceived by Professor Peter Buckley; his background is in economics, and he is one of the world's leading theorists of the activities of multinational companies. The project was therefore phrased in terms of academic economics, but from the first it was looking explicitly at the frontiers of economics, and at innovative approaches. The aim was to understand how companies organize and understand the range of activities which confront them at their own boundaries. Within text-book economics, the boundary between a company and the outside world is often conceived of as one between 'firm' and 'market'. For the great majority of economic analysis, the firm is a black box, around which inputs and outputs, cost-curves and break-even diagrams, are elaborated. What happens inside a firm to lead to these results is not open to debate within this form of argument. Rationality and competition assume away all problems, all idiosyncrasy, all diversity.

Ronald Coase, in his pioneering attempt to theorize the very existence of the firm (1937), brought the issue of transaction costs into view, opening up as problematic the boundary between the firm and its environment. The firm was still conceived, however, as a unitary entity, unproblematically distinguished from other firms, and unproblematically distinguished from the market. From this viewpoint, there were two kinds of organization – those of market, and those of hierarchy (or organization as commonly understood); within themselves, firms were organized through hierarchy; between firms, market principles ruled. The fluctuating boundaries of firms were to be explained by the varying transaction costs which required, or forbade, recourse to one or another organizing principle. This was a very fruitful approach, and it has been pushed forward in many ways (see Buckley and Casson 1976; Williamson 1975).

The Coasian dichotomy between firm and market, however, is an oversimplification. Many years ago Richardson (1972) argued that the simple dichotomy, firm vs market, actually represented two rather unlikely polar opposites, which were in fact rather rare in reality: the reality, by contrast, occupied the whole range of grey area between these two polar opposites, with things like alliances, friendships, networks, relationships of trust, and so on, actually occupying the real empirical ground. The reality, therefore, could not justifiably be analysed on the basis of a polarization which so signally misrepresented it.

The project 'The Management of Cooperative Strategies' was an attempt to find a method, and a mode of argument, which would allow this problem to be researched. This involved a continuous negotiation of the mutual possibilities and powers of social anthropology and economics. It is possible to live a life as a professional economist, working out complicated models based entirely on assumptions about what the world is like, and without the input of any real factual information at all.

When economists look for empirical information, they have a tendency to analyse this using models which are rigorous at the expense of reality: they operate, that is, with assumptions which allow determinate analysis to be carried out, but which are so unrealistic as to preclude any serious tangling with the complexities of real life.

Social anthropology is a kind of polar opposite of this. Anthropologists try to tackle reality head on; they don't necessarily expect it to make unilinear or determinate sense; if there is a clash between theory and reality, then their sympathies – and their responsibilities – are usually with reality. Although anthropology is (at least in my view) a conceptually very sophisticated subject, there is not much within it that could be regarded as 'theory', in the sense in which positivist social science understands the word.

My colleague Peter Buckley's previous research projects had been carried out using either questionnaires or structured interviews. That is to say, his work has always been empirically rather than theoretically based; but the empirical work has been explicitly designed to answer questions arising from theory: it has been, as the research manuals have it, 'hypothetico-deductive'.

My own previous research projects had been carried out using participant observation fieldwork. They were in part related to problems that I wanted to solve, but they were certainly not containable within the framework of positivist research. My main period of fieldwork was spent in Brittany, where I lived in one village for nearly three years; I was interested in fishing and farming, in ethnicity, in bilingualism, and all sorts of other things. I wrote that work up as a doctoral thesis, a 'monograph', which is typically what social anthropologists do. A single case, years of work. No possibilities of comparison, no sampling, no controls, no statistical analysis: a single case.

The result of this is that our attitudes to what counted as evidence, what counted as research, and so on, were very different. To find some sort of compromise of method between the two styles of research to which we were accustomed, we decided to interview managers using what the textbooks call 'unstructured interviews'. (I think my colleague at the time viewed this as a subset of 'interviews'; I regarded it as a subset of 'talking to people'.) To get some idea of the differences that we brought to the exercise, a few illustrations may serve.

When we went to do our first interview, my colleague said that going in without a questionnaire he 'felt naked'. At the time, a questionnaire would have felt to me to be an unwieldy and intrusive burden. In retrospect, and now that I have been for several more years in the field of business research, I can better appreciate his feelings, while still not necessarily sharing them. From his research background, going in without a questionnaire raised unwelcome questions such as 'how are we going to find anything out?' and 'what will we talk about?'; for me, going in without a questionnaire promised this: that we would be able to talk about what was important to the manager whom we were interviewing, in terms of understandings that were his, not ours. Thus, we revealed an aspect of the theoretical fissure which runs right through the social sciences, generating many oppositions: quantitative/ qualitative, positivist/ interpretive, and so on (see Buckley and Chapman 1997c).

Our first interview lasted about four hours, including talk, lunch, and a walk around the factory. After this, my colleague remarked that this was a particularly long period of time to spend in a company; administering a questionnaire usually only took about half an hour, and you were in any case reluctant to trespass further on management time. (In this case, as in all others, we left to the manager in question the decision as to how long the interview should go on.) I was genuinely surprised by this perception, since by my own standards we had done nothing more than poke a head round the door; the 'four hours' contrasted, for me, not with the half-hour that it would have taken to administer a questionnaire, but with the year-long total immersion fieldwork in the company which would have satisfied purist anthropological criteria.

In an early interview, a manager (who was in production) began to talk of the difficulties of his personal relationship with the new people in the marketing department. My ears pricked up. My colleague's attention wandered. In another interview, we were exploring the relationship of a company to its competitors and suppliers. The conversation wandered to the relationship of the company to the regulators, and to the downstream customers (wholesalers, retailers). Again, I continued scribbling, steeped in holism, and not able to suppose that there was such a thing as 'irrelevant information'. Again, my colleague's attention wandered.

These trivial illustrations have behind them some rather important points of difference. For a social anthropologist, there is no end to relevance. Anthropologists try to be 'holistic' in inquiry. They assume, without even necessarily thinking very hard about it on most occasions, that everything is related to everything else: that somebody's personal relationship with another will affect their professional relationship; that friendship will affect arm's-length market dealing; that politics will affect economics, religion will affect morality, language will affect politics; that the life of somebody within a company cannot be divorced from his or her life outside the company; and so on. All of the major thrusts in social anthropological theory in this century – functionalism, structuralism – have confirmed this bent of thought. Social anthropologists also assume, as a matter of course, that truth is socially created, socially relative, and multi-faceted: that what a manager says to you on Tuesday about his relationship with Mr X from finance might be rather different from what he says on Wednesday, and that since his relationship with Company Y is mediated through Mr X, this will have an effect not just upon the reporting of events, but upon the realization of the events themselves.

The decision to conduct unstructured interviews, and to allow those being interviewed to have a strong hand in conducting the research agenda, proved very

fruitful. We asked managers being interviewed to talk about problems that they faced across the boundary of their company, at its interface with others; at its simplest, this was asking the classic make or buy decision: if the company needs something, does it make it or buy it, and why, and how is this decision made? This proved to be a potent question, which in virtually all cases provoked a long series of reflections and considerations; we usually needed to do little more than nudge the ball along from time to time. Perhaps I am overstating a case here: we did carry a theory with us into the interviews (a theory about the importance of the definition and realization of the boundaries of the firm, as outlined above); we did hold the view that research into managerial perceptions of how the company boundary was created and crossed was theoretically important. After that, however, we found it very useful to allow managers to formulate their own problems, and their own discussion of these. And they were very willing to do so. My colleague was surprised not only by the quality and depth of the knowledge that we gathered during the interviews, but also by the willingness of those interviewed to carry on talking once the interviews were under way.

There is growing resistance, within business, to continued cooperation with the research procedures of traditional business academia – in particular, there is growing resistance to filling in research questionnaires; return rates are low, and the quality of response must often be very doubtful, even when return is made. Some managers that I have talked to now make it policy to put all questionnaires immediately in the bin. This is partly driven by pressure of work and shortage of time. It is partly in consequence of the constant increase of questionnaires arriving in the mail, itself a consequence of the enormous increase in academic activity in this area (undergraduate dissertations, MBA dissertations, doctoral work, post-doctoral work, consultancies, and so on). It is also, however, to judge from our own work, partly driven by an impatience with questionnaires as a research tool. This impatience is provoked by a sense that the questionnaires are simply asking wrong questions, irrelevant questions, and (perhaps most importantly) questions which (to remember Hocart) 'unite what others divide, and divide what others unite'.

Our own approach, by contrast, allowed managers to formulate and pursue problems in their own terms. From a social anthropological point of view, this is routine. Within business and management studies, it was rather a radical departure (see Buckley and Chapman 1997c). It had the virtue that managers were not disaffected by the research procedures, and were not anxious to end the interviews as soon as possible; on the contrary, most interviews went on longer than originally scheduled, and some only ended because of mutual exhaustion in telling and listening. Managers, like everybody else, are interested in themselves and in what they do. A long unstructured interview allows them to talk about this, in all its complexity, with somebody who is also genuinely interested; we conjectured that perhaps this did not happen very often (who else would listen? Spouse? Fellow manager?).

Even managers who felt that it would be hard for them to find the time, have allowed the interviews to run on for several hours, and have given every appearance of enjoying them. No one has yet said 'don't come back', and in some cases we have been back five or six times. In one interesting case, the manager had stressed that he was very busy at the time of arranging the interview, and had restricted it to half an hour in his diary. At the start of the interview, he repeated this constraint. He was one of a very few who answered our probing questions tersely, and was difficult to launch into a stream of reflection and narrative. We eventually ran out of different ways of attempting to effect this, and conceded defeat; that, we said, was all we had to ask. The clock had gone for about twenty minutes. He looked mildly disconcerted, and rallied by telling us all the interesting things that we should have asked about. The interview eventually went on for over two hours, revealing some highly interesting stories of intercorporate and interpersonal trust.

So, by conducting long, unstructured, and multiply repeated interviews, we have not, in anthropological terms, precisely carried out fieldwork. With such a method, some of the anthropological interest in context, in intimacy, in creative remembering and forgetting, can be satisfied. By interviewing different people within the same company, we get multiple accounts, multiple perspectives. We have presented our method, to the managers we have interviewed, as an attempted alternative to the orthodoxy of structured questionnaires, administered at a single point in time. It is interesting that our approach has been almost unanimously approved by the managers that we have been studying.

Did we replicate some of the sense of continuity that fieldwork produces? Well, yes, to some extent, simply by going back again and again to the same people. We already had, of course, a great deal of contextual knowledge of much of their activity, so we did not have to spend several apprentice years learning about all of their social lives; we knew what they were likely to eat, wear, say, and so on. So repeated unstructured interviews, of an open-ended kind, are perhaps not fully satisfactory, but they can certainly be defended. They can certainly be expected to generate friendship, or at least companionability; they can generate shared experience, which grows, and can then be used as the basis for further reflections. It is much easier to talk to somebody about important things, if you have shared experience to provide a context for expression: 'You remember what I was saying about old so-and-so? Well, only last week . . .', and the like. Our own increasing contextual knowledge, not only of individual companies but of the industry as a whole, was important in this.

Open-ended and unstructured interviews also allowed the expression of all apparently relevant dimensions of thought and action. Social anthropology cannot avoid taking a holistic view, and this is a view that seems to appeal to managers. Companies can only thrive in a holistic sense (it is no good making things if you can't sell them . . .), and management is so protean an activity that it is almost

impossible to put boundaries round it; managers recognize this, and appreciate it when academics recognize it as well. The traditional approach of business studies, particularistic and specialist, does not serve this requirement. Anthropologists are interested in social context, without any predetermined limitations, and again this strikes a chord with many managers; they know, from practice and intuition, that the boundaries of relevance are never clear-cut. In long and repeated interviews, they are allowed to tell their story.

What of the results? My colleague's first perception remained valid: our results were rich and detailed. There remained, and remains, the problem of how to write these up, and how to relate the data to the economic theories from which the project originally derived. Here we are still at work. The characteristic approach within business and management studies is to generate numerical results, which can then be subjected to statistical analysis (typically through SPSS). Many conceptual issues which would have troubled a social anthropologist at the start of the research are, by the time the results of statistical analysis emerge, very distant memories, rolled flat and forgotten. It is striking, indeed, that when the results of such research procedures are discussed, attention commonly focuses upon the rectitude of the statistical procedures, rather than upon the often very problematic conceptual issues surrounding the original hypotheses. We are spared these problems, only to be faced with others. Our data is not amenable to statistical analysis, so that route is closed. But the economic theories to whose refinement we hope to contribute have typically accepted only quantitative data as legitimate and valid. One way in which we have tried to get round this is to introduce the idea that managerial *perception* of quantity, and relative quantity, as verbally expressed, is socially real (Buckley and Chapman 1997a).

We have noted that previous studies in this area have typically involved questionnaire responses and published data. The most intimate forms of enquiry have proceeded through one-off in-depth interviews. Such one-off interviews are satisfactory for examining states, moments in time, but not for examining processes, movement through time. Of course, any interview contains an implicit time dimension, in that the interviewee uses past material to make sense of the present, and to predict future events. It is typically present understanding, however, which dictates the structure of recollection, and this present understanding changes over time. The past is not necessarily objectively remembered. This is by now an anthropological commonplace (Tonkin *et al.* 1989), and one which, on the basis of our project, we have explored in the corporate domain (Buckley and Chapman 1997b). By using repeated interviews of the same companies and people, we were able to focus attention not only on successive states, but also on successive recollections of the past. Events that, in prospect, looked catastrophic, can be recollected with tranquillity, as having led to success; events that, in prospect, were scarcely considered significant, can be seen, in retrospect, to have had dire consequences. This is a very fruitful field of inquiry in business studies, and also potentially revolutionary. Business strategists, investors, managers, researchers, are all profoundly interested in being able to predict the future: the potential rewards are colossal, like knowing the result of the next Grand National. In fact, most models that claim to be successfully predictive of the future are little more than slightly disguised models by which the present is successfully predicted by the past. If, in that past and present, managers were continually recreating their past decisions and their expected futures, then the status of models of future prediction looks precarious indeed.

There is not space here to give any full analysis of the materials gathered through our research project. Many different kinds of analysis are in any case possible. In order to give some idea of the advantages that our approach has over questionnaire-based, single-point-of-time-studies, one or two examples may help.

The virtue of continued and ever-deepening acquaintance with our interviewees was manifest in many ways. We had of course promised confidentiality and anonymity, with a guarantee not to disclose corporate or personal names in any of our analyses. Nevertheless, interviewees were sometimes a little guarded at our first meeting, as one might expect. This caution often wore off even during the first interview, so that after an hour or so it was thrown to the winds. Certainly, by the second or third interviews, it was always much reduced. One clear marker of this was the telling phrase, 'I shouldn't be telling you this, but . . .' It sometimes helped, at this critical moment, if the interviewer stopped scribbling notes; the interviewee, helped by the momentary sense that this was *not* going down on paper (though eventually it did), continued with the illicit tale. The point of this is not specifically that we *were* interested in confidential material for its own sake. It is of course true that any full account of corporate life, satisfactory in an anthropological sense, would have to include a great deal of material that was confidential and sensitive, either at the individual, office, or corporate frontier, and we were after as much information as we could get. The significance of the 'I shouldn't be telling you this . . .' moment, was, in part, that it presaged the disclosure of deeper and juicier information, and we were grateful for this. There is no doubt that in most cases, had we been trying to elicit information through the use of a tick-box questionnaire, we would not have been able to access information at this level of sensitivity. One interviewee (already alluded to above) gave us rich information about practices to which he had been a witness that bordered on the corrupt. (The nature of the border is itself an interesting one, since the border is often a grey twilight, rather than a black-and-white line.) Had we asked, from a bald questionnaire, 'have you ever been bribed? If yes, go to section ci, how often?, section cii, how much?, section ciii, who by?', then it is reasonable to suppose that we would have been fobbed off with a negative. (We have a colleague who carried out just such a questionnaire interview among multinational corporations active in sub-Saharan Africa, and found that none of them ever paid or received bribes.)

A more important point about the 'I shouldn't be telling you this . . .' moment, was that it was clear evidence that the narrative necessity gripping the interviewee was overcoming certain kinds of caution and suspicion. The stories that we were told, usually hours long, discursive, multiply recursive through past events and future interpretations, were *whole* stories in an important sense. The people telling them were often as involved in them as it is possible to be. The confidentiality issue – what could and could not be told according to certain conventions – was one that cut across narrative and holistic necessities, and it was generally these necessities that prevailed. If the story needed confidential elements, then the story got them; the narrative drive and integrity, as driven and experienced by the interviewees themselves, demanded this. They wanted to tell their story, and they wanted it to be understood. We were, from many experiences of this kind, confident that our information was indeed richer than anything a questionnaire could have produced. We are not suggesting, of course, that there were no areas of secrecy left undisclosed; only that we got further than most alternative methods (short of full participant observation).

A further example relates to the issue of prediction of possible futures, and the interpretation of realized events. As noted above, the business world is passionately interested in the future. The academic and consultancy discourses surrounding corporate strategy are in many respects exercises in futurology. Every interview we conducted held elements of speculation about forthcoming events, with current action designed to accommodate expected or possible futures. By conducting repeated interviews, we were able to track events in something rather like real time. There is a multi-layered controversy within the economics and strategy literatures about whether corporate strategy is top-down designed, and competent to predict and control the future, or whether strategy is chaotic (perhaps even in a technical sense) and (to use a term made useful by Austrian economics) 'emergent' (that is, you know what you are doing when you find yourself doing it).

We have been able to contribute in this area as well, since we had numerous examples where the futures expected and planned for in interview 1 were confounded and overturned by interview 2 (and so on). One company had an elaborate plan for expanding its sales force, at considerable expense and with some degree of risk. All managers in the company had contributed to the thinking which led to this conclusion, and it was generally accepted, on the basis of available evidence, that this was the right thing to do. Steps were taken towards this. As this was happening, a consultancy was called in to do some in-house management training. As a result of the contacts established through this activity, the same consultancy was asked to look briefly at the corporate decision to engage its own enlarged workforce. The consultancy rapidly came to the conclusion that the right way to move was towards outsourcing of virtually all sales activity, with compelling arguments employed. All this took place over a period during which we conducted eight interviews with the company. Any single interview, however deep, would have failed to track these rapid changes in the wind, with their profound effects on corporate strategy, and on accepted managerial thinking. The implications of such events for the more confident and predictive of strategy models are profound, and we will be exploring these in future publications.

In another company that we interviewed repeatedly, we found ourselves meeting one manager, at the same desk, who was working for a different company, with a different name, and a different strategic rationale, every time we met him. He carried on doing more or less the same thing, with more or less the same people around him, in the same place, while a whirl of corporate acquisition and divestment went on around him. Because his own activity had to make sense within the larger corporate environment within which he worked, and because this larger environment changed its name, structure, and strategic rationale several times during the course of our research, he too had to adopt subtly varying accounts of what he was doing, and why. Again, single-point-of-time studies and questionnaire studies could not have captured the detail and entertainment of this.

There are other problems involved in attempted studies of modern companies, in the social anthropological style, problems that were not fully anticipated when the anthropologist was working on a coral island. We can discuss these under three headings: disclosure, access, and other opportunities.

Disclosure

Anthropologists traditionally came back from exotic fieldwork with a total conviction that the world of their research (coral island, tropical jungle, mountain village) was totally separate from the world of their academic publication. The classic anthropological monograph was about illiterate people speaking another language on the other side of the world. An anthropologist could say what he or she pleased, without any thoughts of confidentiality, libel, ethics, law suits, and the like. An anthropologist could publish photographs of the naked savages, without any fear that the savages, or their children or grandchildren, would turn up demanding reparation. We could take the work of the Oxford Africanists, led by Evans-Pritchard, as an example here.

The security of separation between anthropologist and people is best exemplified, in my own mind at least, by the work of Lawrence Wylie. Wylie was an American anthropologist, who carried out research in the Vaucluse in the south of France, in the late 1950s. His theoretical background was psychosexual, and he devoted considerable attention to how children were reared, the effect of this upon their sexuality and social activity, and so on. He returned to the United States, and wrote what he regarded as a scholarly monograph, directed at serious students of social organization. The work, *Village in the Vaucluse*, is highly respected. However, in the 1960s, it was getting easier and easier to send information back and forth across

continents. One of the people from the village got hold of a copy of the book, and it found a home in the village bar, sitting on the bar-top. The book was in English, and most of the people from the village in the Vaucluse were unable to read it with any fluency. They could look up their own and other people's names, however, and they could get some idea that the book seemed to be about sex, and the curious habits and practices of various individuals – filtered through the half-understood foreign language of English. The result was a predictable uproar, a widespread village sense of having been belittled and betrayed.

Wylie was very surprised by this. The conventions of anthropology to that date had required no sensitivity to this kind of problem. Ever since, the problem has grown more intense. An anthropologist must expect that the objects of study will become readers of the monograph. Wylie's response was to translate the book into French, so that at least the villagers would have direct access to what he said, rather than having bawdy and malicious half-translations thrown at them.

Anthropologists today are almost morbidly sensitive about this kind of problem. It is interesting to note, however, that even as late as the early 1960s, no problem was perceived.

A company, unlike a 'primitive society', is potentially a highly sensitive and litigious organization. The researcher can have no illusions that the readership of the published findings will be confined to a small scholarly audience. The information, moreover, is not just morally sensitive, but potentially commercially sensitive as well. This is not an issue with which social anthropologists have commonly had to deal. One can give disguised names to the companies under discussion, but this is not always an effective solution, particularly where the companies are large ones, and their largeness matters. (Consider: 'this data comes from a large UK manufacturer of aero-engines'.) One of the most celebrated single-company studies in business literature was provided by Geert Hofstede (1980); the company was originally disguised as 'Hermes', but it was not long before everybody knew that the company in question was IBM.

Access

In many societies, there are no formal barriers to an anthropologist. Most anthropologists have not needed 'permission' to go and do their study. They have simply moved in with their tent, or rented their flat, and begun making local contacts. Company research is not like that. We might expect that under most circumstances, if an anthropologist wandered vaguely into the Managing Director's office, sat down, and announced that he was going to do three years' fieldwork there, the security men would move briskly in.

Access is always a problem in business research. We have already noted above that many managers and companies feel 'over-researched' already (although this varies from country to country, and from sector to sector). Many business researchers rely upon the exploitation of previous contacts, friendships, and the like, for first access to a company (and it is first access that is most problematic; we always ended interviews by asking if we could come again, and, as noted above, nobody has yet said 'no'). The problem of access is compounded by the disaffection of managers from the practices of business research. That is a major reason why we congratulated ourselves upon having a research method which managers, once into their stride, could almost be said to have enjoyed.

One solution to the problem of access is to *work* for the company under study: true participation, with or without telling the company of one's true intentions. If with, then the problems referred to below (under 'other opportunities') will raise themselves. If without, then there are clearly ethical issues arising. Activity in these areas is not sufficiently deep or widespread for protocols of any strength to arise. People make their own bargains with their conscience.

Other Opportunities

Social anthropologists face a problem that there is nothing for them to do to earn a living but teach other people to be social anthropologists. This is not healthy. This problem seems to have arisen in large part because of the enduring primitivism of the subject. The smaller, the more exotic, and the more materially impoverished the people of your study, the more prestigious was your work in academic circles; that was your reward. The punishment was that nobody outside academia cared: you had no knowledge to sell for which there was large consumer demand. There are many degrees and examples of partial exception to this generalization, but it can be defended from observed social realities.

In business and management studies, this is not necessarily the case. Many people in business and management academia and research have potential alternative careers in business and management. Many successful researchers become successful consultants, and the rewards are very much higher than those in academia (at least as measured in the wallet). If an anthropologist succeeded in doing a long term of participant observation in a single company, he or she would probably, as a result, be in a position to start operating as a consultant in various domains. Such people would stop being anthropologists, and become something else, too busy to go to conferences or to write learned articles, because the opportunity costs would be so high.

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–2–

What is an Ethnographic Study?

Alexandra Ouroussoff

> The reasonableness of institutions, and above all their utility, is the principled [sic] way we explain ourselves to ourselves. Rationality is our rationalization. *Marshall Sahlins (1976: 72)*

When asked to contribute to a volume on ethnographic method with special reference to organizations, I could not help wondering why Western organizations deserved special treatment. There are no books giving similar preferential treatment to, for example, matrilineal societies or the islands of New Guinea. This is not an omission on the part of anthropologists but follows from (a) the success of ethnographic research over the last 100 years and (b) the principle that one does not modify one's methodology without good reason. Moving away from established principles of research in the field of organization studies would suggest that ethnographers had encountered methodological problems specific to this field.

The anthropology of organizations is, however, a new field of theoretical enquiry. I believe this to be true despite the periodic interest within British social anthropology in studying them. With few recent exceptions, ethnographies of organizations have given low priority to interpreting and describing the culture through which organizational reality is constituted; that is, the symbols, metaphors, and emotions through which each organization coheres as a distinct cultural entity.[1,2]

1. The best known are the Manchester shop-floor studies: Cunnison (1966), Lupton (1963). In these studies the issue of *cultural* coherence is not a subject for ethnographic investigation but answered *a priori* in terms of the economic function of the organization and/or the political function the organization fulfils in the wider society. They are grounded, in other words, in a functionalist interpretation of liberal or Marxist theories of political economy. There are also many examples of policy-driven ethnographies of organizations. For an excellent discussion of the contradictions between policy-defined paradigms and anthropological aims, see Okely (1987).

2. I take coherence to be a condition of human society. Every ethnography is implicitly an attempt to give an answer to the question: What accounts for social coherence? or: Why is everything not undifferentiated chaos? Indeed, one way of thinking about anthropological theory is as a history of the answers anthropologists have given to this question.

The historic lack of interest in developing a cultural perspective of organizations is a consequence of an assumption implicitly held by anthropologists and others, that in this context the question of cultural cohesion has already been solved. This follows from the culturally given yet false assumption that rationality is the primary agent of organizational coherence. Yet if one not only listens carefully to what the natives say but, in an attempt to get beyond the rhetoric, closely observes what they do, one is drawn to a very different conclusion.

This deeper-level analysis reveals a complex, often contradictory relation between managers' explicit formulations and the ideas which underpin their day-to-day practice. And it is in the midst of this complexity that one realizes that the notion of rationality as a unifying factor has to be abandoned. It simply cannot explain organizations and their culture.

This throws into relief two questions: How do the people who constitute the organization conceive of rationality? And what, in fact, accounts for the organization's coherence? These questions differ fundamentally from those generated by positing rationality as prior to enquiry, and if pursued across a whole range of organizations they would reveal a rich and hitherto unfamiliar picture of contemporary capitalism.

An anthropological approach would, for example, raise fundamental questions about the nature and extent of our dependence on organizations and the implications of this dependence for the way we perceive social life. In contrast with societies traditionally studied by anthropologists, access to the natural resources on which we depend for our livelihood is mediated by the relatively concentrated network of extractive industries, manufacturing corporations, and financial institutions that constitute political economy. As individuals, we do not have direct access to natural resources or the skills to transform them. And whether through our wages, social security, or a return on our investment, we depend on political economy for our survival.[3]

Since Malinowski there has been a high degree of awareness within anthropology of the limitations of the rationalist approach to political economy, in particular its failure to include the question of the cultural significance of rationalist explanations. But these ideas are debated by anthropologists in the abstract, leaving rationalist theoreticians to define its mode of operation. And the question of how real men and women in actual situations constitute the human relations through which the processes of political economy must necessarily proceed remains hidden by their categories. An anthropological approach may well reveal that these categories have as much to do with rationalizing the loss of control that lies at the root of our dependence on political economy as with a genuine attempt to

3. A theory of political economy will be found to be implicit in any description of organizational life. I take political economy to be constitutive of hierarchical relations within and between organizations the most significant of which are orientated towards capital expansion and accumulation.

comprehend the nature of its institutions. In any case, the potential of this approach is, as yet, untapped.[4]

Recent changes in government policies in higher education are, sadly, reinforcing the historical trend.[5] In the field of organizational study, anthropology is competing for funding with well-established rationalist frameworks, familiar in social science methodology. These include, for example, positivist sociology, theories of rational choice, and behaviourist psychology (which underpins much of organization theory). These frameworks partly derive their power and prestige from the belief that they have produced powerful results relevant to people who manage the economy, whether civil servants or chief executives. They also have the advantage that they reaffirm commonly held assumptions about the nature of our society. Most importantly, they accept as self-evident that organizations are consciously realized; that is, they assume that people know what their motives are and that these motives are recognizable by certain kinds of social science methods: the extended interview, for example. By definition, rationalist frameworks lack the conceptual tools for identifying the profound discrepancies that can exist between the way people conceive of the organization and their actual practice. Nor are they designed to handle the subtle, imaginative, and symbolic ways people have found to deal with the inconsistencies and contradictions that arise from such discrepancies.

It is against this background that one needs to look carefully at some of the more recent 'ethnographies' of organizations. The tendency is to abandon anthropological conceptions of culture and society in favour of rationalist conceptions associated with more prestigious methodologies.[6]

The question this raises is whether rationalist 'ethnographies' of organizations can contribute to the wider intellectual aims of anthropology. My own view is that this is unlikely. What ultimately distinguishes an ethnographic study of an

4. This dependence may well be the source of our affective experience of the nature/society dichotomy. If this is so, then the prevalence of the dichotomy (Descola 1996) might be better understood through a critique of it as ideology; the way it obliges us to conceal the concrete human relations through which our society creates subsistence wealth and the process of alienation found within these relations. Efforts to dislocate the dichotomy by detailed studies of scientific activity (Latour 1987; Woolgar 1993) implicitly overvalue scientific thinking by positing it as a defining instance of Western thought. Although it is true that scientific thinking has been central to the technological transformation of our society over the past few hundred years, it is easy to exaggerate the pervasiveness of this mode of thinking because of its power and prestige. But it is not the dominant mode, even if it is the most important specialist mode.

5. For a discussion of these changes see Willmott (1995) and Ryan (1998).

6. For an example of a study carried out in this vein, see Harper (1998) and also Chapman (Chapter One of this volume). For an example of one of the few recent ethnographic studies of an organization emerging from the theoretical tradition of anthropology, see Janelli and Yim (1993).

organization from other kinds of studies is its goal: to extend our perception of cultural difference. From a cross-cultural perspective, the rationalist framework constitutes local native theory and forms a part of the *object* of study. As Sahlins says, rationality is how we explain ourselves to ourselves: it is our rationalization. Developing a cultural perspective on political economy involves understanding the relation between this rationalization (our theory of ourselves) and our practice. Failure to explore this relation, far from extending our perception of cultural difference, will only succeed in further reifying an ethnocentric *idée fixe.*

The aim of this chapter is to examine the question of organizational rationality through the prism of anthropological theory. It contrasts the relations between sexual liaisons and corporate ethos in two organizations. While in one of them *symbolic* sexuality is unconsciously tied by managers to the driving logic of corporate success, managers in the other make no such link. I deliberately chose this comparison to give ethnographic prominence to the profound cultural differences that can be found to exist between organizations within British political economy.

The ethnography focuses on the organizations' Head Offices, both located in the southeast of England within 200 miles of one another. Bion International is a highly successful multinational manufacturing corporation, itself forming part of a much larger corporate enterprise, BDC, which holds 55 per cent of its shares. C&R is a non-profit-making service organization. Although managers in both come from all over the British Isles, they share a similar social background in that they are the first generation in their family to have been educated to 'A'-level and/or university and consider their entrance into the managerial class as a significant achievement.

Bion International[7]

During my initial fieldwork in Bion I attributed no great significance to sexual liaisons between managers and their secretaries, regarding their affairs as external to corporate concerns. It was only later, while carrying out fieldwork in C&R where a radically different attitude towards sexual liaisons prevailed, that I began to re-evaluate their significance in Bion. In this chapter I have decided to follow the logic of this re-evaluation to emphasize the remoteness of the link between libidinal desire and economic performance from formal articulations of corporate exigencies and to bring attention to the role of comparison in forcing the appearance of this link.

Bion International, a manufacturing corporation, produces a product with a high international public profile. Unlike a society, a corporation has an explicit

7. All names, both of companies and of individuals, are pseudonyms.

goal. The accumulation of profit is a legal obligation and directors are bound to act in accordance with it (Companies Act 1948). Incorporated in 1949, in terms of both profit and market share, Bion has grown from strength to strength. Managers and directors are proud to be a part of what is regarded as a highly dynamic and successful enterprise. In 1979–80, when I carried out the original fieldwork, the Company employed 7,000 people in the UK.[8]

An atmosphere of urgency pervades Bion's Head Office where eight Directors and 320 managers are responsible for co-ordinating production and distribution between five factories. The managers' orientation is towards the future of the company, a future which depends on their capacity to overcome the problems with which they are confronted daily. On the first of the two occasions on which the Financial Director agreed to let me accompany him through his working day I arrived, as he had requested, at 7 a.m. to find him already at work. As early as 6 a.m. the Chief Executive and other senior managers can be found in their offices preparing for the day ahead.

Even at this time in the morning the Financial Director was under considerable strain. As the day progressed it became apparent that the pressing issue was whether a new overseas advertising campaign would bring a return on capital recently invested. A significant cash investment had been made and he had arrived at work already aware that there was some doubt as to whether initial expectations would be met. By the time his secretary came into his office at 9 a.m., he had a list of tasks for her to carry out. With a constrained 'Good morning', he handed it to her and brusquely (though not rudely) asked her for a cup of coffee.

When she returned, he told her, this time with less courtesy, to set up an urgent meeting with the Overseas Sales Director. She seemed to take his manner entirely in her stride. Speaking on the telephone with the Sales Director, his tone was abrasive as he listed the figures he needed for his meeting with the Chief Executive in two hours' time. His day continued at the same pace and with the same level of underlying impatience. He left his office at 7 p.m., but whether he was able to leave his work is another matter.

Although every day is not equally arduous, the ability to keep pace and accomplish tasks under pressure is an important theme in the working life of managers. Production must keep up with expanding demand, and market share sustained or improved. The ability to move through the day with speed and deliberation, maintaining the offensive, being seen to be in control of events, are all aspects of a temperament considered to be essential to the advancement of Bion's interests.

8. I was a participant observer between November 1979 and January 1980 (returning in 1981 as an outside researcher). I made return visits to Head Office in 1988 and 1994 and to the principal manufacturing location in 1999.

From the standpoint of Bion's Head Office, the key to corporate viability lies in selecting managers whose character traits ensure that skills and experience will be properly harnessed to corporate objectives. The competent Bion manager is described as independent and tough-minded, a man of character, who is not afraid of taking tough action.

These attributes shape and guide a manager's general demeanour, making him easy to recognize. Selecting the right kind of manager for Bion is considered to be a fairly straightforward process. Managers scorn the use of personality tests and other 'scientific' procedures designed to improve the quality of new recruits. Their confidence is well placed: they are very successful in selecting men who share their orientation to reality.

An important feature of this orientation is the idea that only men can be responsible for corporate success or failure. The capacity to manage is seen as an exclusively male attribute, though not all men possess it. The ninety-four women who work in Head Office as secretaries occupy the more static administrative sphere. Both managers and secretaries regard secretarial work as peripheral to the central purpose of the company.[9]

By contrast, the most dynamic and challenging work takes place in the manufacturing division, which also carries the highest prestige. Managers and Directors in Bion's eight divisions agree that without the product, there is nothing to sell and therefore no profit to be made. In terms of routine pressures the biggest threat to profit is a decline in productivity. Although far removed from the manufacturing locations, Head Office buzzes with news of events affecting production. Managers are well aware of the personalities in charge of each of the five factory locations, and a great deal of energy is spent on comparing their distinctive approaches to 'controlling production' (their term).

The capacity to control is, in the context of day-to-day management, the most important trait of the tough manager. A critical issue facing the Company at the time of study was the need to cut unit costs in its largest manufacturing location. The factory manager's brief was to improve capital equipment and to maintain costs (including wages) while not lowering output. Success would depend on the 1,360 shop-floor workers accepting the new techniques. Since the company's inception, Head Office and factory managers have held the conviction that male workers in the manufacturing department (as contrasted with men in processing or women in packing) overvalue their contribution to the production process and are prone to lowering output when working conditions do not suit them. From the

9. This less essential role is also reflected in the fact that their skills are more easily replaceable and less expensive to purchase. (Literally unseen by managers are 104 Head Office clerical workers. There is, unfortunately, no space here to describe the considerable implications for gender and class relations.)

managers' standpoint, the male workers in the five manufacturing departments have proved the greatest threat to productivity.[10]

Reflecting the perceived volatility of production, the Director of manufacture is informed on a weekly basis of factories' productivity levels and divisional executives are quick to visit a factory manager if productivity falls below target. During declines in productivity tensions can run very high (tough managers can be *temporarily* defeated) but these are nevertheless routine problems, and precisely the reason why tough managers are necessary. Despite the high tension managers also know that the overall pattern of action tends towards the creation of profit.

Although the association between managers' positive masculine traits and the capacity to meet corporate aims is not formally acknowledged as such, it is indirectly expressed in spontaneous discourse. Weak managers are ridiculed for their failure to act, their failure of nerve, and their generally pathetic attempts at control. Regardless of the nature of his experience or the quality of his skills, a weak manager constitutes an economic deficit. His dithering and indecisiveness will mean, for example, that the energies of his subordinates will not be properly directed. Weakness denotes dependency, a susceptibility to being manipulated (particularly by subordinates), and the need to refer to colleagues or superiors for advice. Thus, weak managers involuntarily undermine corporate interests by deflecting substantial amounts of energy from the corporate goal.

The contrasting image of the weak manager highlights the most significant positive masculine trait through which corporate interests are served: the capacity to behave independently. This capacity is seen as intrinsic to the person, summed up in the Director of Sales' quip, 'You either have it or you don't.' There are no training courses designed to instil independence in the weak manager. Needless to say, weak managers are not contenders in the hierarchical bids for promotion.[11]

10. The scope these workers have for lowering output is considerable. The process of production is continuous: an interruption caused by, for example, a momentary lack of vigilance by a single worker can, in an instant, lead to thousands of pounds in lost revenue. The line between intentional and unintentional lapses in vigilance is difficult to draw and senior managers – though not always line managers – are predisposed to assume intention. There is unfortunately no room here to explore the basis of this conviction. However, changes in legislation have not affected the fundamental dynamics on the shop floor or managers' perception of the machine operators. Changes in labour law and the reduction of workers' rights (Employment Acts, 1980, 1982, 1988, 1990, and 1993; see Hendy 1993), while increasing managers' control over production, have simultaneously led to an increase in underlying tension between managers and workers in Bion and the issue of control is as pressing as before. The difference is that now workers are less able either to voice their disagreements or act on them collectively .

11. The few managers deemed to be incompetent are explained in terms of external pressures such as constraints in the labour market. A position may need to be filled for which there may only be candidates without relevant skills and experience. A weak manager may then be brought in as a temporary substitute.

Although not all managers aim for the top job, promotion to the Board of Bion is considered the pinnacle of a manager's career, the point at which his abilities become acknowledged within Bion and the industry as a whole. As a manager is promoted his reputation for autonomy is consolidated. Expensive cars, larger offices, and bigger salaries are emblematic of his increased value to the company. Unlike some other companies, particularly in the financial sector, where moving on to another company can be seen as an achievement, in Bion leaving the company, even for a higher salary, is regarded as an admission of defeat.

The board of the holding company lies outside managers' aspirations. BDC is composed of men who inhabit a social sphere far removed from those who direct Bion. The chairman of Bion, John Holmes, is the only chairman of a subsidiary to sit on the board of BDC. This is a consequence of Bion's position in the group as a whole and does not reflect his social standing.[12] Although only one of forty-five businesses in the BDC portfolio and not the largest or the most profitable, Bion is the most important manufacturing business in that, unlike BDC's other acquisitions, it is the only company to manufacture a product with a high public profile. A positive public image is considered by BDC to be vital to the stability of shareholder value.[13]

The relatively closed, comprehensively male domain of Bion is supported and complemented by the presence of women. Relative to the contribution men make in meeting the demands of production and distribution, women have less dynamic significance; but their dependence on men is, nevertheless, deeply significant insofar as it naturalizes male autonomy. Both officially and in practice, secretaries depend for their status on their association with their boss's autonomy. By promoting her boss's interests and carrying out her tasks effectively, she adds to her manager's and therefore to corporate strength.[14] The dependence of women complements the positive predisposition of men to behave autonomously. Despite

12. BDC's twelve directors include a number of titled persons who, with the exception of John Holmes, also sit on the board of at least seven other companies (one is on the board of thirty-five companies). Holmes holds no other directorships, nor does he have an office in BDC's London Head Office. In terms of social background he stands well apart, not having attended public school, nor belonging to a gentleman's club; nor is he listed in either *Who's Who* or *International Who's Who*, as the majority of BDC directors are.

13. Reflecting recent structural changes in the global economy, in the mid-1990s BDC was bought by an American mega-national corporation. (For an analysis of these changes see Luttwak, 1999.) The internal structure of BDC, however, remains unchanged and Lord H**** still presides as chairman.

14. This is not an expression of passive accommodation. Secretaries actively identify with their bosses' power and autonomy, competing with each other to promote their own bosses' interests. Somewhere in *The Second Sex* Simone de Beauvoir makes the elementary but sometimes forgotten point that if women did not internalize male values there would be no need for a feminist movement. Unfortunately there is no room here to include ethnographic material showing how the dominant male perspective is expressed through the secretaries' competitive behaviour.

their peripheral status and the manner in which managers characteristically relate to them, 'secretary' is a positive term.

The positive evaluation of women's dependence extends outward towards wives and their children. There is an unwritten rule in Bion that managers should be married by age 30, and the majority are. There is a close consensus that traditional marriages are to be preferred, in that managers earn enough so that their wives do not have to work and should therefore be able to enjoy what is regarded as a far less arduous life of domestic responsibility.

Gender provides a separation between positive and negative expressions of dependency. The deeply pejorative 'weak' cannot apply to women, for whom dependence is appropriate. The positively valued dependent woman symbolically highlights the defective nature of the dependent man. In a man, dependence denotes emasculation, powerlessness, an inherent inability to live up to the male ideal.

Changes in legislation (e.g. the Sex Discrimination Act, 1975) and the appointment of female managers have had little effect on gender formulations. Because women are predisposed to dependence, any initiatives taken by a female 'manager' are assumed to stem from the manager to whom she reports. The four women appointed into management grades so as to avoid public scrutiny and possible litigation are managers in name only. In practice, female managers are thought of as secretaries.[15]

What is striking about these shared conceptions of proper manhood and womanhood is the way they serve to rationalize both the sexual division of labour and the pursuit of corporate aims. In realizing corporate objectives, managers and secretaries simultaneously realize what they take to be their own natural potential.

This being the case, one might imagine that once a tough manager is recruited his competence would be taken for granted, opening the way for closer assessment of each manager's relative contribution to corporate success. The event described below illustrates, however, that far from being taken for granted, the ability to *demonstrate* toughness is essential to a manager's credibility. Following a slowdown in production in one of the factories in Northern Ireland, Martin, a head-office executive in the Manufacturing division who had been with the company for fifteen years, flew to Belfast to inform the workers that failure to reach production targets could lead to the factory's closure. Arriving unannounced, he went straight onto the shop floor and, according to his own account, finding the workers he 'knew' to be responsible for the slowdown, told them that should they continue to hold up production they would be responsible for the loss of over a thousand jobs. His reasoning, presumably, was that his authority as a head-office executive and the fact that he was one of the men who would be involved in the decision to close a

15. It has proved difficult to find statistics showing the ratio of female/male managers in manufacturing; I suspect they have remained relatively static compared with those for other sectors of the economy.

factory would lend weight to his threat. Having said his piece, he went to see the factory manager. His intrusion into factory affairs provoked a row. The factory manager was furious that Martin had undermined his authority and that of his thirty-six factory managers.

Head-Office colleagues spoke about this event with evident admiration for Martin. The story was widely commented upon and elaborated, and the factory manager became the butt of a number of jokes. However, the factory manager's loss of esteem was temporary. He was known to be a tough manager in charge of a very difficult situation, which for the most part colleagues felt he handled well. The row was seen principally as being an altercation between two men of the same calibre. The next round could well go to the factory manager.

From the perspective of head-office managers, the significant details of Martin's visit were that he took the decision to go to Northern Ireland on the spur of the moment without consulting his superiors, that he walked onto the shop floor unannounced, and, finally, that he gave the workers a piece of his mind. They did not discuss the consequences of his actions with regard to production levels or workers' relations with management. A month later I began fieldwork in this factory and had the opportunity to talk to shop-floor workers about Martin's visit. The incident did not appear to have had much impact. No one believed that management would shut down the factory and his attitude, which they saw as offensive, was seen as typical of the way many – but not all – managers in this company behaved.

In the weeks that followed Martin's visit, production levels did improve. However, because so many factors influence production, it would be impossible to assess what sort of effect Martin's action had. But from the point of view of understanding what head-office managers consider to be the significant attributes of a successful Bion manager, the important point is that *they* were not engaged in the question of *effect*. There was no spontaneous debate, no weighing up the possible outcomes of his action. What mattered was the nature of the aim – a threat – and the manner in which it was carried out. Martin's actions clearly demonstrated his autonomy and his determination. He had taken a bold initiative and had not hesitated to see it through. It was taken for granted that the effect was in alignment with the company's economic interests.

Faith in a tie between autonomous behaviour and effective outcome is misplaced. Whether in Head Office or in the factories one can observe on a daily basis how giving primary value to visibility of autonomy over actual effect results in rational, quasi-rational, and irrational outcomes.[16] A calculation of the economic cost of

16. Bion managers skilfully employ the idea of luck to mask discrepancies between the ideal and the reality. I have described elsewhere the striking example of how managers used 'luck' to rationalize the dismissal of a weak factory manager who had produced the best productivity figures in the history of the industry (Ouroussoff 1993).

this prevailing hierarchy of values would have to take into account the amount of energy and time they could have otherwise spent on rational calculation, the cost of failure to assess whether actions have the desired outcome, and the cost of not modifying actions in light of this experience. Calculating managers' failure to realize profit relative to investment in capital equipment and human resources would merit another paper of its own. Here, however, I am concerned with the more fundamental question of the underlying pressures on managers to place visibility of autonomy over and above close examination of outcome. The most significant of these pressures came to light during an episode involving the dismissal of a Director.

During the course of my fieldwork, the Director of Manufacture, Mr Sands, a veteran of twenty-five years, was dismissed by the Chief Executive, a move backed by the board of the holding company. The director, now 59, had worked his way up the Company, having been brought into its oldest factory as a middle manager. This factory was suffering from what the majority of directors considered to be endemic labour-relations problems. The board was in favour of closing the factory down.

Sands decided not to back the closure, at least at this stage. He argued that other possible solutions had not yet been fully explored. The disagreement revolved around the loss of a highly experienced workforce whose value he felt the board was underestimating. Almost 2,000 workers, many of whom had twenty or more years' experience in the industry, would be lost. The time and cost of training new workers had not, according to him, been properly calculated. The Chief Executive took the view that Sands' arguments were based on old loyalties and nostalgia and were indicative of a weakness of character. With the backing of the BDC Board, he was given a fortnight's notice.

Sands had been dismissed because nostalgia had weakened his capacity to make tough decisions. From this it followed that his arguments lacked merit.

For the first week of that fortnight Sands did not appear at Head Office. By the second week rumours began to circulate that he had spent several nights locked in his office refusing sandwiches offered him by his secretary. (Director's offices have showers and WCs.) Whether or not this was true, during the second week I saw him walking down a corridor looking dishevelled and unshaven.

Over the days that followed managers expressed concern for his welfare and suggestions that his evident depression might lead to suicide were taken seriously, though no one ventured to offer him any actual support. Despite their concern, the consensus was that he had brought it on himself. He had flinched from making the tough decision and had paid the price. The obvious contradiction between the inherent nature of toughness and his sudden change in temperament which managers implicitly attributed to an act of will, was ignored. During the months that followed managers deliberated over what he had achieved throughout his career. As the weeks went by these achievements began to look less and less impressive. The final conclusion was that, when seen in the cold light of day, his achievements had been, after all, insubstantial. The slow reinterpretation of old events resulted, in effect, in the rewriting of Sands' history.

For twenty-five years Sands was regarded as a tough manager. For ten of those years, from the standpoint of his colleagues, he had successfully run the largest and most profitable factory, did battle with the unions, and survived at least two Chief Executives. He was then promoted to Head Office and eventually to the Board. He was considered, if not the most, at least one of the most successful managers in the Company. Before his dismissal, managers described him as arrogant and difficult to deal with. Although irritating to some, these characteristics were also evidence of his toughness. Following his dismissal, however, the 'inherent' traits that had been a necessary condition of his success suddenly ceased to exist and twenty-five years' worth of evidence amounted to nothing.

Sands was not the only one to have his history reinvented in this way. During the course of my fieldwork, the same happened with regard to two other managers. One was also dismissed and the other, having been denied a long-sought-after promotion, left of his own accord.

Managers did not reflect on, discuss, or elaborate the reasons for their gross collective misjudgement. From their standpoint, they were correct in their assessment of Sands' 'inherent' qualities then and they are correct now. The contradiction implied by their sudden revision of his 'inherent' traits does not form part of their conscious frame of reference. The fact that this revision invalidates the principles which they claim to be the source of the Company's power and success goes unrecognized. Nevertheless, despite their conscious claims, the process of revising Sands' history not only invalidates his past successes but simultaneously invalidates the principle that success within the company is contingent on a manager's inherent masculine traits.

So, although at a conscious level there is absolutely no doubt that success is contingent on toughness, at an unconscious level managers also know that as circumstances change these 'inherent' qualities can be overturned. Since managers collectively engage in the revision of others, each manager must also unconsciously know that it could happen to him: that is, that the nature of his masculinity may, after all, be open to question, and that the one thing that is supposed to guarantee success – constant demonstration of autonomy – in fact guarantees nothing. Hence it is the threat of his masculine qualities being defective that unconsciously drives each manager continually to re-assert his masculine traits. Contrary to their conscious perceptions, managers are in fact under continual pressure – what must seem like an eternal pressure – to demonstrate to their colleagues and their superiors that they are real men and therefore worthy of the designation 'tough', the designation they falsely and unconsciously assume guarantees their usefulness to the Company.

The need to sustain the illusion that usefulness to the corporation is contingent on inherent masculine traits masks a harsher reality: their economic dependency on an entity with power to dismiss them arbitrarily. Managers and directors live on their wages and have limited personal capital. Even in times of low unemployment, many managers have no guarantee that their skills will transfer. Because managers do not avail themselves of the collective support of a trade union which would offer both financial and legal support in the event of a disagreement with the Company, each manager is dependent on his own material resources.[17]

The powerful emotional investment in the idea of their own autonomy protects them from the reality that however hard a manager works, however successful a manager deems himself to be, the corporation has the power to dismiss him. To avoid legal proceedings, managers who are dismissed are given generous pay-offs but, given the powerful investment in the association between the capacity to meet corporate aims and their self-definition as male, this cannot compensate them for their sense of defeat, humiliation, and failure.[18]

From a conscious perspective, their masculinity is not in need of confirmation, but is seen as a fixed quality from which their ability, their security, and indeed their 'autonomy' ultimately stems. From an unconscious perspective, autonomy is an illusion through which managers come to feel they can control the uncontrollable.

The conscious yet false premise that masculinity is not open to question performs a powerful organizational function. In protecting managers from becoming aware of their own vulnerability, managers are also 'protected' from experiencing the discontinuity between their own personal interests and the interests of the corporation. It is not simply that managers experience their own interests as aligned with the interests of the corporation, an assumption which contains within it the idea of two sets of interests, but that managers experience personal and corporate interests as one and the same, thus increasing the motivational force of their commitment.

Against this backdrop it now seems unsurprising that female sexuality would play an important role in sustaining the illusion of autonomy. But at the time of

17. The consensus in Head Office is that to make a case of unfair dismissal gives a manager the reputation of a trouble-maker and lowers his chances of finding work elsewhere.

18. It is only within the limited context of this study that managers' psychological predisposition towards dependency is given priority over material dependence on the corporation. Such priority would fall away were the context to be widened to include the manager's upbringing and the relation of his family to the means of production. This issue of context gives some indication of the potential theoretical complexities involved in comparing an ideology of autonomy held by people wholly dependent on political economy with an ideology of autonomy held by people who have both direct access to natural resources and the skills to transform them (the Hageners of Papua New Guinea, to take one example (Strathern 1981)).

the study, I was as blinded as the managers by our shared assumption that the sphere in which managers exercise their responsibilities for the company is separate from the sphere of personal relations, the one area of social activity seemingly undetermined by corporate aims.

The separation between public and private concerns did also generate a great deal of empirical evidence which further stood in the way of my seeing the underlying logic. Managers, for example, do not talk about their wives or children, or their life outside the Company. To raise such issues in Company time is considered unprofessional. Outside official Company time domestic issues are regarded as tedious subjects for conversation. And unlike in many other companies, wives do not attend pensioners' parties or Christmas dinners, and throughout the course of my fieldwork I had no occasion to meet a manager's wife or engage in discussion about their domestic life.

Similarly, talk of managers' sexual affairs fell into the category of the personal. Despite the professional way managers relate to secretaries, their own as well as those of their colleagues, it had of course occurred to me that some managers might be having extra-marital affairs.[19] It was, however, only as I became better integrated into the managers' world, and found myself in circumstances where managers felt relatively relaxed and not obliged to be seen orienting all of their energies towards corporate goals, that the subject would come up. It was, for example, between 7 and 9 in the morning when managers often visit one another's offices and discuss matters unrelated to work, or on the long train journeys between factories that I began to hear of their affairs.

These revelations did not take the form of malicious gossip but tended to be more in the vein of discussing a bit of Company news. As the months went by it became apparent that, despite the discreet way in which affairs were discussed, public display was very much the point. Who was sleeping with whom was common knowledge. Although managers never spoke about their own affairs, they discussed the affairs of close colleagues when they were not present. Their chat usually revolved around the physical qualities of the women or mistresses (their term).

At the managerial level, having a mistress serves to enhance a manager's image. Managers with mistresses, especially those with particularly attractive mistresses, are spoken of with admiration and there is a vicarious identification with managers who succeed in finding a mistress. Managers also boast about one another's successes; the effect seemed to me to be very much like one's home team winning at football: good for public morale. But what seemed to me to be the more important point was that having a mistress was not an indication of a manager's capacity to manage. A tough manager will continue to be a tough manager even if he has no

19. With few exceptions, and in contrast to managers, secretaries are single.

mistress. Conversely, a weak manager with a mistress, no matter how attractive, is still a weak manager. By the same token, being a mistress does not increase or decrease a secretary's prospects for promotion. For both managers and secretaries affairs are regarded as a separate, private matter.[20]

Directors also have mistresses and I knew relatively early on, four or five months into the fieldwork, that six of the eight directors were having affairs with their own or another director's secretary. An exception was one director who was having an affair with a female administrator in the company's largest factory. What took quite a bit longer to grasp was that whereas for managers having a mistress falls into the realm of enhancement, at board level having a mistress is an imperative.

The two board members not having affairs were the Chief Executive and the Director of Manufacture. Holmes had recently married his secretary following his wife's death: a gap of three months did not raise any eyebrows. Since his new wife no longer worked for the Company it was widely speculated that he would soon be looking for a replacement. This speculation was not based on the assumption that Holmes was a womanizer. He had no such reputation. It followed rather from what was taken to be a truism, that Bion Directors have mistresses.

The second Director was the newly promoted Director of Manufacture, the replacement for the unfortunate Mr Sands. About three weeks after his promotion I was in the Company bar after work talking to a colleague of the new director. He mentioned to me that Kevin, the director, was looking for a suitable mistress. I asked him what he meant by 'suitable' and he said, 'Well, you know, not someone from the telephone exchange.' He went on to say that the right sort of girls were few and far between. I asked him to keep me posted. A month later I was travelling by car to one of the factories in the North with this same manager and I asked him how Kevin was getting on. He told me that he hadn't managed to find anyone and was in fact very distressed about it. A director, he said, should not have to find himself in such a position. In other words, it was his status as a Director that was at issue. The colleague then asked if I knew anyone who would be 'willing to help him out of this extremely difficult situation'. The problem was that there were no 'girls' of the appropriate status available.

It was following this conversation that I began to notice that directors' secretaries were better educated,[21] better looking, and dressed in a sexually more provocative manner than managers' secretaries. They were also of a more uniform age (20–26).

20. There are, of course, a range of reasons why secretaries should wish to have affairs with managers. Describing these affairs from their perspective would merit a separate discussion and would not affect the relevant point here.

21. Directors' secretaries have 'A'-levels plus secretarial college. Three Directors' secretaries were bilingual (i.e. overqualified for the job). Requirements at the lower levels are GCSE English plus secretarial college and some previous experience.

There was a very particular 'look' to secretaries on the fifth floor to which the other secretaries in the building did not conform.

At the time of the original study the emphasis on having a 'posh' mistress at the level of director seemed to me simply to demonstrate a higher level of aspiration which Directors, with more resources at their disposal, could indulge in. This aspiration was a privilege of status that, like company cars and executive offices, widened the division between Directors and their senior managers, providing them with yet another opportunity to make the distinction manifest. Although I found the whole issue of 'posh' secretaries and mistresses intriguing, relative to the managers' intense preoccupation with production and distribution and the amount of energy they pour into profit accumulation, the issue of mistresses did initially appear to be peripheral to their main concerns.

It took the experience of fieldwork in another organization, one that placed the public/private divide along a very different axis, to break the categorical given profit/not sex. Managers and secretaries in this second organization see a clear link between the private world of sexual affairs and the public life of the organization. This led me to realize that I had too readily accepted certain indigenous categories held by Bion managers, namely, the conceptual separation between female sexuality and corporate success.

C&R

What follows is drawn from an ethnographic study of an organization that occupies a very different sector of the economy.[22] It is non-profit-making and ultimately depends on donations rather than investors for its existence. The organization directly employs 10,000 people, a thousand of them in their London Head Office. It is here referred to as C&R.

I spent the first day of what was to be an eighteen-month study, between 1992 and 1994, sitting in on a number of meetings in the Chief Executive's office. The following morning I wrote up my field notes and at 2 p.m. went along to meet the Chief Executive's number three. I was just getting to the purpose of my visit, which was to ask if I could attend a highly sensitive meeting later that afternoon, when he cut me short and said, 'Are you doing anything right now?' 'No.' (Odd question.) He stood up, picked up his jacket, and walked out of his office. I followed him downstairs, across the road, and into the pub. He ordered a couple of drinks and we sat down. He had brought with him pen and paper and without saying anything began to draw a diagram. It took him several minutes. When he finished it he looked up and said, 'You will never understand the politics of this organization

22. No attempt is being made here to draw a correlation between the symbolic function of sex in a particular organization and organizational aims.

if you don't know who is sleeping with whom.' He then talked me through the diagram. The first thing that struck me was that the Bion prescription – wives on the outside, mistresses on the inside – did not apply here. Both wives and mistresses were welcome to work for the organization. He explained that some of the long-standing affairs had produced offspring who could be found playing along side their legitimate brothers and sisters in the company crèche. A year's fieldwork proved his diagram to be faultless. This manager's candour also turned out to reflect an intense and explicit – certainly compared with the circumstances at Bion – preoccupation with colleagues' sexual affairs. It is considered perfectly acceptable to discuss one's own affairs, the affairs of others as well as all the complications that follow from having to work in the same organization as one's wife and mistress, or, significantly, lover and husband. In C&R organizational politics are rarely discussed without reference to sexual politics. In contrast to Bion, the two domains are not regarded as separate.[23]

During the second week of fieldwork in C&R I went to see a secretary in one of the planning departments to discuss the forthcoming restructuring of secretarial work. This was the first time we had met. She opened the discussion by saying, 'You may have already heard that I am having a relationship with C&R's most senior Executive . . . well, it's true.' She went on to say they had being seeing each other for seven years. I was surprised at the open and matter-of-fact way she told me this. Guessing what type of response she was expecting, I asked her whether her relationship with the executive had created difficulties with her colleagues. She said that it had. It made people both envious and anxious because they knew she had the ear of the Director and was in a position to tell him anything she might see or overhear. She was very keen to get across to me that she did not in fact do so.

In C&R, affairs were openly recognized as morally problematic. Not only the implications for work relations but also the emotional effects on wives and husbands are openly discussed. In one department, for example, a number of people were concerned about a forthcoming promotion that would place a woman who was having an affair with a married manager next to his wife. Given the size of Head Office, this kind of coincidence happened infrequently. But what was significant was the response the coincidence generated among the men and women who were 'in the know'. They were concerned to find a way of protecting the feelings and reputations of all three. In C&R to say an issue was not public means that it would not be raised in a departmental meeting but would have to be resolved by other, non-institutional means. In other words, a private matter could also be recognized as a collective concern.

23. In C&R profound, unconscious contradictions revolve around the relation between managers and those on whom they rely for contributions.

This followed from the assumption that the quality of working relations underwrites a manager's capacity to carry out his or her tasks effectively. Whether working relations within a department are conducive to eliciting the best from people will to some extent depend on the relations between individual personalities as well as on their personal circumstances, which are seen as subject to change. High tension between two managers within the same department, for example, was initially put down to a disagreement over strategy. Several days later, when the disagreement was still unresolved, a wider range of explanations came into play. The personalities of the two managers were discussed as well as the personal circumstances of one of the managers, which it was felt were hampering his ability to respond sensibly to a difficult situation.

This is not to imply that some people are neither critical of decisions nor judgemental when it comes to the way others may be managing their personal lives. The point is rather that the range of possible causes of tension brought into the discussion by members of the department assume emotionally constituted persons, each with his or her own distinct history. People are deemed to have complex lives that involve spouses and children as well as colleagues. Work and home lives are seen as to some extent interdependent. Referring to domestic problems in work time, and even occasionally allowing domestic needs to take priority, are just part of life. It is, for example, perfectly acceptable for a manager to say he has to leave a meeting early in order to collect his child from school; it would be assumed that he had done his best to make alternative arrangements. When scheduling meetings, it was not uncommon for managers to give domestic reasons for preferring one time to another: attending a child's school play, for example. When I had to miss an important group of meetings because my own son was ill, the reaction was sympathetic. And when I returned I was surprised to find myself in more than one discussion with male managers, comparing the particular strains of flu our respective children had suffered from that winter. Showing responsibility towards one's own family enhances a manager's reputation. My own relations with managers improved once they learned that I also had to negotiate complicated domestic/work arrangements. Because people are expected to be open about domestic matters, a great deal of information about the personal lives of colleagues accumulates over the years. A manager's success, or lack of it, in his job is seen in the light of the person's particular history, that is, as relating to a given set of conditions which differ from manager to manager.

Institutional arrangements for selection and promotion reflected this conception of success. Committees composed of departmental members from each rank – that is, colleagues with whom one has worked, often for many years – follow procedures well known not just to the members of the committee, but to everyone in the organization. There are varying ways in which a manager might be assessed, taking into account his or her skills in relation to the needs of a given department,

his or her relations with colleagues, and his or her personal circumstances. The priority given to any of these would depend on the particular department and the constitution of its committee. Although formal procedures are only one aspect of a far more complex reality which can include intense, and sometimes vicious, behind-the-scenes politicking, nevertheless it is the committee as a body that recruits, promotes, demotes, and very infrequently dismisses a manager.

The ubiquitous threat to livelihood that in Bion is connected to the criteria for success in terms of a single rigid ideal finds no parallel in C&R. If, for example, a manager is not considered to be working effectively, a series of procedures are in place to help him identify and overcome his failing. He is given ample opportunity for reform and dismissals are therefore extremely rare. In the previous ten years, only one person had been dismissed: a manager quite low in the hierarchy who had literally been 'caught with his fingers in the till'. It is openly acknowledged that in C&R there are quite a high percentage of incompetent managers incapable of reform. This is a cause of some frustration for those having to work with them: 'having to pull another manager's weight' was a complaint I often heard. In C&R, tolerance of dependence is an institutional imperative. In contrast to Bion, where the managers' assumption of autonomy and the will to be tough causes them to overlook their dependency on the company, at C&R hierarchy is seen as constitutive of relations between unique persons. The social dynamic is experienced as inhering in relations of interdependence rather than driven by the need to demonstrate self-sufficiency.

It is worth stressing that the distinct ways of constituting relations are not simply the effects of the organizations in question. When recruiting, managers explicitly state that they look for not only a candidate with the right skills and qualifications, but for the kind of person who will 'fit in' to the organization. The capacity to fit in, however, refers both to the characteristics managers consciously seek and to a more complex emotional schema to which they do not necessarily have conscious access. In Bion, for example, managers are concerned to discover whether their unconscious assumptions about masculinity – assumptions that play such a critical role in determining how they perceive and experience the organization – are shared by the potential recruit.

As long as a degree of choice exists, on the part of both the applicant and the Corporation, there is likely to be a deep homology between the character of the person and the culture of the organization. It is just as much a case of managers seeking candidates whose internal reality matches the needs of the organization as of candidates seeking external 'objective' confirmation of their own internalized reality.[24] In creating a synthesis an important function of the process of selection

24. This deep-rooted internalized reality is a product of the experiences of childhood. For a discussion of the process of internalization, see Laing (1967).

is fulfilled which is to provide the shared subjective experience through which the organization is able to cohere as an entity.

Although there is obviously enough cultural overlap between some organizations to allow managers to function effectively in more than one, as it happens the cultural differences between Bion and C&R are so profound as to make it extremely difficult, if not impossible, to imagine a Bion manager working effectively in C&R. This point is perhaps most clearly illustrated by the differences in gender formulations.

In C&R gender identity is not rigidly tied to the capacity to meet organizational aims. There are two female heads of department and two female deputy heads. In contrast to Bion, all four are accepted as full social persons and expected to take full responsibility for their decisions. This is not to say that promotion into the senior grades has been plain sailing. Male C&R managers openly discussed their reluctance to promote women, arguing that it is disruptive to what to them is a cosy, club-like atmosphere. They are, they say, used to working in a male environment and it is unsettling and therefore inconvenient to have to learn how to work effectively with women at this stage in their careers. Managers who had not yet experienced working closely with women in senior positions thought it would take time to become accustomed to it. Those who were already working with them confirmed such concern. But senior managers also said, with no prompting from me, that this reluctance, which they think perfectly understandable, is ultimately indefensible both on ethical grounds and in terms of the needs of the organization. The capacity of women to manage is not at issue.

For their part, female managers in C&R found the going very hard. Being accepted as full social persons does not mean they are treated as equals. They feel the standards set for them are higher than for their male counterparts, which they resent. Whether or not this is the case is extremely difficult to assess, but all four of the senior women did feel they were succeeding in meeting these standards.

Unsurprisingly, there is no pressure in C&R for male managers to have either wives or mistresses. And, although some senior managers have both wives and mistresses, possessing a mistress is not, in itself, given a positive value: it does not form part of a collective ideal towards which managers at any level of the organization strive. The lack of identification between having the capacity to manage and being a man means that female managers (and for that matter, mistresses) are considered full social persons, wholly implicated in the world of power. In contrast to what happens at Bion, the 'private' sphere of personal relations is not identified with the domain of women and thus split off from the central needs of the organization.

In C&R, then, a manager's manhood is not contingent on his achievements for the organization. It derives from a much wider moral universe that includes more of his human relations to the world. His qualities as a father, as well as the way he

orders relations with his wife and mistress, form a conscious part of the frame of reference through which his manhood is constituted. In C&R it is possible for a man to be a moral person – a full social person – and an indifferent manager. His manhood, his very being, is not at the service of the organization and as a consequence he is not driven by the need to act for something outside himself in order to reconfirm his own sense of self.

It was the experience of doing fieldwork in an organization where female sexuality is neither central to, nor excluded from, the organization's main concerns that shot into relief the symbolic centrality of female sexuality in Bion: more specifically, the way in which female sexuality is covertly brought in to serve Bion's corporate aims. The deep hierarchical motive underpinning the requirement to have a mistress had been obscured by the absolute nature of the conceptual separation between the dynamic universe of the male corporation and the peripheral domain of women.

Bion managers are driven by the desire to demonstrate toughness. Each 'bold action' proves (once again) that they qualify as tough men. Achievements are explicitly addressed to one's peers and subordinates, and they emphasize a manager's ability to determine, and therefore control, the world around him. But displays of toughness are at the same time, though less consciously, addressed to the audience above, to the men who hold their future in their hands.

Here we begin to see the real extent and nature of their alienation. Although not all managers are equally ambitious, there is a powerful consensus that ultimate success of a manager's career is the appointment to the Board of Directors. And for many, over a number of years, the continuing display of their worth and significance has been orientated towards this goal, a goal which holds out the promise of control and security. It is difficult to exaggerate the significance of actually reaching the Board of Directors. Not only is this the top of the British division, it is considered the most prestigious of all the divisions worldwide. Although managers can, and sometimes do, take transfers abroad this is seen as a form of defeat. The real prize is to be appointed to the board of Bion. And managers who have achieved this goal have years of confirmation that their criterion of success works.

But the reality at the end turns out to be very different from the ideal. A manager finally reaches the top, but the top of what? The cars, chauffeurs, and offices that symbolize the final attainment of absolute control are no protection against the arbitrary power of those who actually control the Company. The concrete end that held reality at bay vanishes. They are now directly beholden to BDC, men whom they do not know and have barely met. The nature of their relation to their superiors has changed. They are beholden to men who will not bear witness to their 'tough acts', who are interested in results per se, not in the manner in which they are achieved. Their strategy for survival is redundant yet the responsibilities and the pressures to achieve are greater than ever before. The reality they cannot confront is that the source of their power (proof of their masculinity) is illusory; that, after all, they are mere proletarians: they are, and have always been, expendable commodities.

The problem for Bion Directors is how to get the threat under control. The solution emerges from the internal cultural logic. They must confront reality by the roundabout route of known symbols, in this case by means of performance and the explicit production of achievements. Proof of real manhood fends off threats. The problem is to find a way of presenting the evidence to their invisible audience.

At the managerial level there is no institutional imperative to take a mistress in order to succeed. Having a mistress at this level of the hierarchy is merely one of a number of possible enhancements, like wearing expensive suits. It does not in itself make a manager tough. Nevertheless, a mistress is an admirable possession and many highly esteemed managers have them. From the standpoint of the manager, display is very much the point. But what is being displayed if not the soundness of his performance? The collective ideal contains an implicit symbolic association between achievement and potency. But while, for many managers, achievements alone are enough to indicate potency, others (arguably those with the deepest concern with their own insignificance) turn the symbolic representation into reality. The important point, however, is that the symbolic association exists prior to managers being appointed to the board. It forms part of a more complex range of unconscious motivations that can be drawn upon as external conditions create new demands on unconscious life.[25]

Proof of potency takes on added urgency when the vital belief in the efficacy of toughness finds no other means of being sustained. 'Posh' women, that is, women the directors imagine could occupy the social domain of their superiors, represent a symbolic communication with the men above. And through what is in effect an unconsciously held mystical association, directors attempt to display their potency, their managerial competence, to those who have ultimate power over their labour.

In contrast with the way I had originally understood sexual liaisons, in terms of the indigenous distinction between public and private, I now began to see a complex symbolic organization of shared experience whereby libidinal desire and economic performance are unconsciously experienced as integral to one another. Profit as a symbol of potency, a notion that until this fieldwork had, for me, been no more than a cliché, turned out to be a concretely held creative illusion through which 'privately experienced' sexual desire is fused with the structure and function of the enterprise.

25. For a discussion of how sexuality operates as symbol in social hierarchy, see Burke (1969).

Conclusion

The powerful pressures on Bion managers and Directors to accord a higher value to demonstrating toughness than to assessing the consequences of their tough actions are deeply rooted in unconscious self-definitions of masculinity. They do not result either from inadequate information or from a straightforward misunderstanding of the facts. The temptation of rationalist theoreticians is to assume that because conscious thought determines action, a change in the hierarchy of values and a subsequent increase in the rate of profit could be achieved by 'rational' means.[26] But to think in these terms is to entirely miss the point; namely, that such an increase in the rate of profit will depend on the outcomes the *culture* of each specific corporation can yield. In the case of Bion, these outcomes are determined by managers interpreting and defining corporate aims through conceptions of masculinity that limit their capacity to act on the basis of purely economic calculation. Such deep-seated conceptions, whether in Bion or C&R, are not themselves created by pressures imposed on managers by the organization. They have developed historically within each individual manager and are a product of the society into which he was born.

A more fundamental understanding of the underlying cultural process through which these managers' orientation to reality emerges would entail overcoming the very considerable practical difficulties created by the profound separation between domestic and productive life characteristic of capitalist society. To develop an anthropological approach to political economy we would need also to observe these men as husbands, sons, fathers, and grandsons; that is, in the context of family relations as well as the relation of the family to the means of production. Taken together we would then have an ethnographic account of the relations which comprise their universe.

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26. This is already to grant them too much. Rationalist paradigms do not have the tools to identify the deep-seated, shared assumptions that underpin organizational life.

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Part II
Science

–3–

Ethnography in the Laboratory

Christine Hine

Introduction

This chapter aims to illustrate the possibilities and problems of conducting an ethnographic study within a laboratory. The first section introduces the grounds for thinking of the laboratory as an organization open to ethnographic study. The rest of the chapter then focuses on the study which I conducted within a mouse-genetics laboratory, first introducing the project of which the study formed a part, then describing the role which I adopted, before introducing a brief summary of the methods which I used and the results which I gained. The final section of the chapter maps the work in sociology of scientific knowledge which forms the basis for the approach taken in this study and draws together some issues arising from the study which might be relevant in other studies of organizations where knowledge and expertise are highly specialized.

Laboratories and Organizations

This collection of articles is about ethnography in organizations. At first glance, a laboratory might seem quite an esoteric field site for an organizational ethnography. It therefore seems appropriate to consider first what is special about the particular type of organization discussed in this chapter, the laboratory. That laboratories are organizations might not be apparent at first sight, looking from within a culture which gives a very special status to science. The work of scientists is often treated with a deference that suggests it is far too complex for outsiders to understand, and ethnographers might well be deterred from trying to enter a laboratory on the grounds that they would not understand what was going on. It might also seem that the work of scientists offered little for an ethnographer to study. We tend not to think about the work of scientists and the facts that they produce as being cultural artefacts. We generally think of science when it is done properly as being objective, by which we mean outside culture. At the end of the chapter I return to the background which supports a view of science as a thoroughly social practice. For now, it is probably sufficient to say that there is much to be gained by suspending a culturally specific awe about science. Thinking of the laboratory as a kind of organization is one way of demystifying it.

In what ways, then, is a laboratory an organization? As we might expect of an organization, it is a very orderly place. It is a task-oriented setting in which people are employed to do a job. It has, like many organizations, a bounded location. Its membership is quite stable, and there is little difficulty in distinguishing insiders from outsiders. There are definite procedures by which new members are trained, and a status hierarchy with distinct ranks through which members may rise. There are no obvious clients, customers, or users of the organization, but there is a distinct peer community (other laboratories) to which members orient their work. The products of the organization are made available to this peer community by publishing papers and giving talks at conferences. The organization relies heavily on routinized working practices and on documentation. Records of what is done are kept with an almost bureaucratic obsessiveness. The orderliness and purposiveness of the laboratory, as with any organization, are created and sustained through explicit rules, but also through the working practices and embodied culture of the members. On these grounds there is much to occupy the ethnographer.

There is, moreover, the intriguing possibility that the kind of science which the laboratory produces might be shaped by the ways in which it is ordered and by the ways of seeing the world which members share. The laboratory then becomes a vital institution to study if one wants to know how science is shaped. The laboratory is reliant on orderly working practices. It also relies to a great extent on recording and measuring devices which make scientific phenomena visible. Some devices, such as microscopes, literally make things visible (although it takes effort to learn to see them). Other laboratory technologies make things visible in less literal but nonetheless important ways, by producing measurements, traces, and statistical analyses. These devices too, and the working practices which surround them, can be seen as constitutive of scientific knowledge rather than as neutral tools which scientists simply use. Ethnographers in technological settings need to pay close attention to the ways in which the technology, its effects, and its success or failure are interpreted (Pfaffenberger 1988). The rest of this chapter attempts to provide some ideas about how this can be done.

The Study: Information Technology in Science

The study I will be describing was part of a project that set out to examine the use of Information Technology (IT) in human genetics research. The original plan was to provide a counter to some of the hype that surrounded the potential of IT to transform science generally (Denning 1991, Maxwell 1990) and specifically to examine the way in which developments in IT had formed part of the drive for major funding of an international initiative to map and sequence the human genome

(Hine 1993). Pre-sensitized as I was by work in the sociology of scientific knowledge and the sociology of technology, I was aware that the choices made in the production and use of laboratory instruments might play a role in shaping the scientific knowledge which resulted from their use. Also, it seemed possible that there might be a gap between public stories about the benefits of IT and the more private accounts of IT in use. Ethnography provided a means of studying the use of IT in context without prejudging any ways of using it as right or wrong.

The project involved a series of interviews with developers and users of IT in genetics research around the UK. These interviews were semi-structured and aimed to map the range of approaches to the use of IT, capturing the words and meanings of participants in relation to their use of technology and its role in their work. These interviews provided a background to the primary part of the project which comprised two periods of ethnography. The first of these involved a group of computer developers producing IT systems for the UK genetics community. The second involved a mouse-genetics laboratory where the systems were in use. For now, I will focus on the ways in which the first ethnography prefigured and shaped the second.

As a member of the computer developers' tight-knit community, I became well-versed in both the capabilities of the system and their aspirations for it. My role within the team was to rewrite the user manual for the system, bringing it up to date and incorporating changes intended to make it more 'user-friendly'. The work of writing documentation is often a low-status and unpopular task: an ideal role, therefore, for an ethnographer! In addition, it provided a perfect opportunity to interrogate ideas about who the users were who were to benefit from the system and become the readers of the manual. The computer-systems developers spent much time discussing what it was that users wanted from the programs they were developing (and also talking about the ways in which users should be using what they had been given). However, many of the working practices which I observed acted to exclude users from involvement in the development process. Through my role as manual writer I became a part of the separate professional sphere around the development of IT. From these experiences I had a picture of the assumptions and practices that shaped the technologies which were developed and a not inconsiderable although sketchily acquired technical knowledge about how to use the system. In addition, I had a very strong moral picture of the appropriate ways in which the computer-systems developers considered their systems should be used.

In the spirit of 'follow the thing' (Marcus 1995), I set out to find a laboratory where the information systems I had seen being developed were in use. One of the series of interviews I was conducting took me to a mouse-genetics laboratory attached to a large teaching hospital in the UK. The head of the laboratory was friendly and informative at the interview, and expressed strong views on what genetics needed from IT and where it could be improved. The laboratory looked lively, genetics research was going on, and IT was certainly being used. Geographically, the laboratory was ideally placed for me. After the interview, I contacted the head of the laboratory again, asking whether his laboratory might be prepared to host me for a longer period of time. Some faxes and letters later, he agreed.

I was surprised how easy it was to negotiate access. Organizational studies are notoriously difficult to secure, and 'getting in, getting on, getting out and getting back' (Buchanan, Boddy, and McCalman 1988) can be fraught with politics. In this case several factors were on my side. IT was already a problem which occupied the head of the laboratory: he thought that it was not being used as effectively as it should have been. The ways in which I presented the problem had some resonance with the ways in which he already saw it. It is also possible that I was seen as a source of technical assistance, to solve some of the problems more directly, since I had told him about my time with the system developers. Finally, having visitors was, I came to realize, a routine event in the laboratory anyway. This being a prominent laboratory in its field, people would come for varying periods of time to learn techniques and the members of the laboratory were able (until my behaviour became particularly strange and un-visitor-like) to fit me in to that general model of being there to learn how things are done.

So, poised to enter my second field site, I explained to my own project leader where I was going. On hearing that I was off to join a mouse-genetics laboratory he expressed dismay: we had after all gained funding for the project on the basis that we were to study the use of IT in *human* genetics. The answer I gave to his questions raises a more general point about ethnography: the chosen ethnographic site needs to be rendered as an adequate place in which to study a particular problem (Rachel and Woolgar 1995). Given that in these times of funding proposals and dissertation outlines we are rarely free to enter the field and study the problems which arise from the ground, the issue is not how a problem is to be rendered appropriate to a particular field site, but how a field site is to be rendered in advance an appropriate place to study a particular problem.

In the case of human and mouse genetics, the answer depends on a routine piece of transformation work, which my informant had produced at our first meeting. The rationale is that it is difficult to carry out certain kinds of genetic study on humans. Humans do not breed fast enough, and in general you cannot control with whom they breed. Many of the techniques used in genetics depend on being able to control breeding and have access to several generations. Meanwhile, mice do breed fast, and their breeding can be controlled. Mice also share a lot of genetic characteristics with humans, and some portions of genetic material are highly conserved between the two. So, having taken a problem (a potentially faulty gene) in humans, we find a similar problem in mice. We then study the problem in mice to try to find the gene responsible. We then go back to humans, and look for the same genetic trait in affected humans. So, knowledge is translated

back and forth from human to mouse and back again. The mouse becomes a 'tool' in human genetics, or an 'honorary human', to use my informant's term. The setting therefore has no necessary and inherent link to the problem being studied. It is rendered adequate to the problem by demonstrating links: in this case, using the informants' understandings to show that a mouse-genetics laboratory can be an appropriate place in which to study human-genetics research.

In this instance the link was considered convincing and so I joined the laboratory. I was scheduled to go to the laboratory four days a week for a period of several months. My plans at that stage went little beyond get there, hang out, watch what goes on, and try not to break anything!

The Role

On my first day, I experienced a routine which I later found was common for visitors on their first day in the laboratory. On arrival I was handed a timetable, which showed the sub-groups within the laboratory, each with a time when a designated person would explain to me what work they were engaged in. A further member of the laboratory was detailed to take me out to lunch at the Chinese restaurant across the road. This structured and informative reception was far more comforting than most ethnographers may have to face. However, by the end of the day I was a mass of confusions. It was fast becoming apparent that I was no geneticist. I do have a Botany degree, which might be expected to give me some of the necessary background: but as an undergraduate I had found genetics difficult and incomprehensible, and it was clear that nothing had changed. Some of the words and concepts were familiar, but I was unable to follow many of the narrative links between techniques and outcomes which seemed self-evident to the people who were explaining their work to me. People were careful to position what they said in relation to my level of knowledge: they would pause in an explanation to check whether a word or concept made sense and took my possession of a scientific degree as some guarantee that I would understand a scientific but not specialist explanation. I was glad to be treated in some degree as an insider, but, ultimately, I was confused.

Over time in the laboratory, with repeated explanations and illustrations of particular procedures and analytic techniques, I became much more comfortable in that I understood the sense of the stories which people told me about their work. Without these stories, about the search for a particular gene, or the construction of a map of a specific region for a specific purpose, the day-to-day activities in the laboratory would have seemed a meaningless jumble. To the untrained eye many of the activities look very mundane: moving between laboratory bench and computer, incubator and dark room, with trays of tiny plastic tubes or fragile slabs of gel, poring over computer printouts or smudgy photographs, and endless waiting for tubes to incubate or gels to 'run'. It was only by coming to understand the wider narratives into which the work fitted that I could begin to see how the difference between something boring, something disastrous, and a major breakthrough was forged. My understanding of these genetic narratives was hard-won and fleeting: now that I have been away from the laboratory for some time, I can remember that they did make sense, but I would be unable to reproduce those self-evident links. My aim, rather than becoming a genetics expert myself, was to understand how the mundane events in the laboratory became meaningful to the participants, and that involved an appreciation of the wider narratives, the research careers, and the competition with other laboratories within which they made sense.

Some understanding of the procedures being carried out was therefore, for me, crucial to gaining a feel for the life of the laboratory. I had to become, briefly, adept at understanding explanations grudgingly or gladly provided over coffee, overheard in the office, and detailed in scientific papers. However, I was never practically competent in a way that would have allowed me a full part in the work. Precision in genetic techniques is highly valued and results matter: one mistake can mean a week's work wasted. The work is a race against competing laboratories, PhD deadlines, and the end of funding. In this environment, where participants have a highly specialized technical knowledge, there is little that the unskilled ethnographer can offer. As far as the scientific work was concerned, I was very much an observer. Some people who became friends and key informants would take me on for a few days to shadow them or to teach me how to carry out a technique, although there was no expectation that I would ever carry out that technique alone. A lot of my time was therefore spent rather uncomfortably hanging out, trying to look as though I was doing something. I interviewed everyone I could, and spent a lot of time in the office attached to the laboratory where the computers were. I checked my email frequently, and absorbed what I could of the activity going on around me.

As time went on confusion (both mine and my informants') grew about my role. It was apparent that I was not like the other visitors. I found myself gradually becoming constituted as the IT expert. People knew that I was there with an interest in IT and the perception that I must therefore know what I was doing stuck, even though I had not mentioned my experiences with the computer systems developers in my previous ethnography out of a wish not to be aligned with them and not to be placed in a position of carrying tales from one field to another. At first, I was highly uncomfortable with the idea of becoming the local IT expert: after all, I was there to study their use of IT, not to shape it myself. The last thing I wanted to do was to set myself up as an expert and to solve people's problems.

Despite these anxieties I can see, looking back over my field notes, that I did change my role over time and I did accept more and more the role of IT expert.

The day when I showed a few people how their computer could be used to access the World Wide Web stood out as the first time I did anything that seemed to impress people! After this, several people asked me to fix problems with computer programs or to help them in finding information on the World Wide Web and I tried to oblige. Part of this, no doubt, is due to my uneasiness about being a person without a useful role in the midst of a busy environment, and the sense that at least here was something that I could do. Also, by the time I was more familiar with the way in which the laboratory worked, I felt much more comfortable with initiating conversations and making suggestions. By demonstrating a new technique or offering a different way of doing something, I was showing my competence not only with the technology, but also with the culture of the laboratory. By then, I knew how important sharing of skills was, and how to teach someone a new technique. I was also more ready to take a proactive role once I had formed some preliminary observations, and introducing new topics and techniques allowed me to test these ideas with my informants.

Paradoxically, much of what I learned about IT in the laboratory came from not looking at IT at all, if by that is meant sitting next to a computer seeing how it is used. By looking at the work of the laboratory in its entirety I learned things which I would not have done had I taken a narrow view of my problem. In the next section of this chapter I give a brief summary of some observations which trade on the comparison between laboratory practices in general and the use of IT in particular.

Learning from Looking Elsewhere

Learning Practices

I have mentioned earlier the kindness of my informants in taking me on for a day or two to explain to me a technique which they were carrying out, and to teach me to do it. These techniques would typically involve a series of procedures at different locations, moving from one piece of equipment to another, from fume cupboard to laboratory bench to incubator to dark room, with solutions and tubes and gels. I would be given a running narrative of the procedure being carried out, how long it would take, whether we could fit in lunch or coffee at this stage, and explanations of what the various solutions and gels contained in terms of the mice who provided the DNA or the chemical name of the constituents. At key stages, I would be allowed to carry out a procedure and would watch my informant nervously and try to mimic precisely what he did, not clear at any stage which moves might be crucial to our success and which might be trivial. My informant bore patiently with my constant questioning of 'Why are we doing this? Why did you do that?' Finally, when I asked why we had carefully washed a slab of glass down twice with a particularly (so he warned me) hazardous solution, he said, 'Because that's what the person who showed me did, and it works, so I'm not going to mess with it.'

Further experience in the laboratory showed that this account was not unusual. There was a strong preference for being shown a technique by someone for whom it worked. Written protocols giving step-by-step instructions for carrying out techniques were freely available, but these were seen as needing interpretation and as leaving out vital information necessary to make the technique work. Techniques were fragile and prone to stop working for no reason, so it made sense to get as close as possible to the way of carrying them out of someone who had had previous success with them.

Observation and interaction with members of the laboratory using the computers showed strong parallels with the learning practices used for laboratory techniques. New computer skills were often learned by watching someone who already had success with them, and people spoke nostalgically of a time when there were some people in the laboratory who liked computers and knew a lot about them and were able to give a lot of help. In the office I tried and failed to locate a copy of the user manual on which I had spent so much time when with the computer systems developers. Written instructions were treated with suspicion or disregard.

Myth and Magic

Looking at laboratory techniques also exposes the extent to which these procedures are seen as being outside rational explanation. A technique may suddenly stop working for no apparent reason, and, without the researchers being aware of what has changed, they may fail to obtain useful results for days, even weeks, at a time. When this occurs, changes to particular stages of the procedure or new batches of solutions may be suggested, but in the end the advice may consist of a shrug of the shoulders and a suggestion to 'Keep trying'.

A similar event occurred one day at the computer. A researcher sat down in front of the workstation in the corner of the office, and typed in his user name and password. He was refused access. He tried again, and the same happened. Noticing his problem, and taking on my new role of IT expert, I leaned across and asked what was wrong. He explained that the computer would not let him log in. As I started to launch into a potential diagnosis, 'I think you've got Caps Lock on . . .' he cut me off. 'It's OK, it's playing up, it does that with mine from time to time for some reason.' He looked out of the office and called to another researcher who was passing to come and log in for him on her password, because his was not working today.

For this researcher, the computer was not a logical, knowable machine whose problems required a rational explanation. Rather, it was a complex, almost organic being which could 'play up' without warning just like the laboratory techniques.

This perception of computers was not necessarily shared throughout the laboratory. However, the output of the computer programs was often interpreted in a behaviourist way: rather than a logical interpretation based on known processes being carried out, the interpretation of computer results was often discussed in terms of what looked or felt right, and the underlying processes inferred from that. One informant told me of a sequence-matching program: 'I kind of looked at the results and worked out what it must be doing from that.' From the computer systems developers I had gathered that scientists were, or should be, deeply interested in and knowledgeable about the algorithms used by the programs which they used. In the laboratory this turned out to be far from true.

Common Property

According to computer systems developers, users are distinct bounded entities, each of whom has and uses his or her own user name and password. The anecdote I tell above gives the lie to this assumption. Sharing of computer space was common in the laboratory. On PCs in the shared office, files tended to be stored on the hard disc drive where anyone could, and did, access them. Researchers tended to use their own electronic mail accounts, and not to read each other's electronic mail, but for access to external computer systems, including the system whose developers I had spent time with, they all shared the same account in the name of the head of the laboratory. Inspired by these observations, I decided to explore how this view of computer space as shared fitted into ideas of space and property within the laboratory more generally.

In order to discuss the notion of sharing and the use of space within the laboratory, I used a collaboratively produced map. I drew a rough sketch of the laboratory and office space and, during a lunch break, I asked the people who were eating their sandwiches in the office to help me complete it. We shaded and labelled all the places which were thought of as belonging to one person or another. Each researcher had a portion of the laboratory bench which was held to be his or hers, and a place where he or she habitually sat in the offices. This led to a discussion on what was and was not shared. Resources, such as enzymes and solutions, were in short supply at the time. It was generally accepted that these were not yours: rather they belonged to the laboratory and you could not reasonably (morally) keep them to yourself. Similarly, labelling a piece of equipment did not make it someone's exclusive possession, although it did make it more likely that you would return it if you borrowed it. Very little in the laboratory was held to be private inviolable space or property, and even apparent ownership implied respect rather than exclusive rights.

Only in relation to time did it seem that computer usage was viewed differently. Some members of the laboratory told me that they preferred to keep computer work for quieter times: outside normal working hours, when the system might be running more quickly, they were less likely to be disturbed. These were the researchers who were more confident with IT: more likely to be happy working alone, and more likely to resent the communal approach which the less confident members of the laboratory took for using the computer. During the day, computer time was likely to be snatched between stages of a procedure which took a defined time, in just the same way as coffee and lunch would be fitted in with a procedure which would not wait. Laboratory work was seen as the most important feature of the work, and computing work as a subsidiary which could be fitted in at odd moments. I was, however, told by one researcher that she regretted not having done more computing earlier in her project because 'it would have been giving me feedback and telling me where I was going wrong.' Generally, the priority of laboratory techniques over other uses of time went unchallenged.

Looking at time and space tends therefore to reinforce a view of the computer as something which is fitted into the routines of laboratory work, and which is incorporated into pre-existing assumptions about the sharing of space and property. The computer tends to become just another piece of laboratory equipment, less exacting in its demands on time than some other procedures.

Transformations of Representations

The results of computer analysis of data were part of a chain of transformations of representations within the laboratory. On a major scale was the transformation from humans to mice and back to humans which I described earlier. On a smaller scale, laboratory work is about moving from one kind of representation to another, with the adequacy of the transformation in principle questionable at every stage (Latour and Woolgar 1986).

To illustrate this point, I need to return to the experience of learning a new technique. The day, for me, started with the tubes which contained mouse DNA held in solution. (I never saw a live mouse during my time at the laboratory.) To these tubes enzymes were added to cleave the DNA, the tubes were incubated to induce cleavage, and then markers were added. Gel was poured between glass plates to form channels for separation of the DNA, and the DNA samples were injected into the channels. These stages called for steady hands and high manual dexterity so as not to lose several hours of work by breaking the gel or contaminating between channels. An electric current was run across the gel to separate out the differently sized strands of DNA, and the gel was stained, washed, and finally taken to a dark room to be photographed. At this stage, my teacher leaned across the gel eager to see if the day's work had been worthwhile, and I too leaned across, watching his face, anxious that my ineptitude might have inadvertently ruined the results. As I watched he smiled, then swept his hand across the gel,

crumpling it into several pieces. I was horrified at seeing our hard work so casually destroyed. I had made the mistake of viewing the gel as an end result, rather than as a stage in a process of transformations.

On returning to the laboratory bench with the photograph of the gel, the interpretation began. My informant sat and studied the photograph, interpreting it for me in the light of what he expected to see, and what would be sensible to see. He said, 'Either I did something wrong, or something interesting is happening.' There then followed a process of preparing a diagram to show the implications of this result for the genetic map, and comparison with other people's results obtained from published papers or databases. The process involves the construction of plausible stories to account for the observed results, and the results from databases provide one part of the backdrop against which the decision as to what makes sense is made.

Plausible stories involve the alignment of different kinds of evidence. Matching of DNA sequences identified in the laboratory with those sequenced by others around the world and stored in computer databases forms one kind of evidence in leading to the identification of a gene. While the laboratory techniques are used to identify 'candidate genes', matches with similar sequences in databases are used to discuss what would be a sensible candidate, and what could plausibly be implicated in the genetic disorder being investigated. Judgements of the reliability of database information are based on who did the work and where, and how sensible they seem in the light of other stories.

A published paper which announces a new genetic map, or identifies the gene for a genetic disorder, therefore represents the end point of a series of transformations of representations, and of judgements about what is a plausible story and what makes sense. The end point requires an alignment of different kinds of evidence, and no one source of data or information is necessarily privileged. The results of computerized data analysis and database matching are judged on the degree to which they contribute to the construction of these plausible stories. My account foregrounds IT as a specific concern. In the work of the laboratory, IT is only foregrounded at particular times, when there are debates about the working of a particular package, or the applicability or reliability of results. At other times, IT is effectively invisible in the work of the laboratory, and the talk is of pulling out genes and reading sequences. In the published papers of the laboratory, the transformations of representations which lead up to the results being reported are all but absent.

Conclusions

Ethnography was first notably used in scientific laboratories in the 1970s and 1980s. Up to this point, sociologists of science had tended to be preoccupied with accounting for bad science, where results were believed which later came to be seen as untrue. It seemed that the task of sociology of science was confined to accounting for what went wrong in these aberrant cases. 'True' science was assumed to be beyond social accounting and to be adequately explained by the appropriate application of scientific method. Sociologists and anthropologists began to study the everyday working practices of laboratories, as part of a move to claim the content of (both 'true' and 'false') science for social analysis (Knorr-Cetina and Mulkay 1983).

Establishing the content of science as open to social analysis entails questioning some taken-for-granted assumptions: that laboratory instruments provide transparent windows on reality (Latour and Woolgar 1986); that experiments can be replicated (Collins 1985); that facts can be readily distinguished from artefacts of the experimental process (Lynch 1985); that facts speak for themselves (Latour and Woolgar 1986); that scientific papers straightforwardly report on the process of fact discovery (Knorr-Cetina 1981); and that discoveries are recognized as such at the time that they are made (Woolgar 1976). Ethnographic studies of scientific practice and scientific discourse (Gilbert and Mulkay 1984) aimed to give accounts of what scientists do and how they do it the same status as the accounts of any other culture. In other words, the work of the early ethnographers was to make taken-for-granted features of our own culture seem strange. Scientific 'truths' were to be seen as embedded in and subject to the cultures which produced them (Traweek 1988). Among these broadly ethnographic studies there is considerable disciplinary and methodological diversity (Traweek 1992).

Viewing the content of science as social in this way opens up a whole range of potential questions for discussion. Once the 'sacred' status of scientific truth is removed (at least for the purposes of analysis, if not for everyday life), ethnographers can also begin to track the complex connections between the knowledge-producing practices of the laboratory and the culture which surrounds and permeates the boundaries of the laboratory. Attempts have been made to trace the sociology of scientific knowledge between laboratories (Fujimura 1992), to consider the implications of the apparent boundedness of the laboratory (Knorr-Cetina 1992; Latour 1987), and to make connections between the products of the laboratory and wider social, political, and economic concerns (Charlesworth *et al.* 1989). Recently, the proposal of multi-sited ethnography (Marcus 1995) has been taken up by anthropologists of science who seek to trace the ways in which scientific knowledge travels beyond the boundaries of the laboratory and is rearticulated and reproduced in its new settings (Layne 1998; Heath 1998).

The sociology of scientific knowledge provides a rich supply of experiences on which to draw in conducting an ethnographic study in a laboratory. In my own work I drew on approaches which open the everyday practices of science to inquiry and see them as crucial in the construction of scientific knowledge. I also drew

heavily on a sceptical analytic approach which did not give a priori privilege to any accounts of what was going on in laboratory work. My question, however, in common with that of many current ethnographies of scientific practice, was rather different from that of the earlier ethnographers. Rather than making a strong point about the social construction of scientific facts, I was concerned to trace the role of one particular laboratory technology and the ways in which its design and use were shaped by local experience.

The brief results which I have described here show how, within a short, focused period of ethnographic observation, I was able to gain a rich sense of the practices of laboratory work, and the ways in which IT fitted in as a piece of laboratory equipment. The application of an ethnographic sensitivity, combined with the specific insights of the sociology of scientific knowledge, allowed me to approach the use of IT in the laboratory as a locally meaningful part of the process of scientific work, without making judgements about the truth or otherwise of the knowledge being produced. By focusing on laboratory work as a whole, rather than narrowly focusing on situations in which a computer was being used, I was able to show the ways in which IT fits into and is transformed by the laboratory setting.

The computer system became incorporated into and transformed by laboratory culture, to become a piece of technology almost unrecognizable to the computer-systems developers who designed it. The dual-site ethnography which I conducted allowed me to contrast the perspective of the designers of the system with the perspective of its eventual users. The different beliefs and practices which prevailed in the laboratory allowed a completely different view of the technology to exist from the one which the systems developers would have advocated. Possibly the greatest irony is that the 'users' in the laboratory were highly satisfied with the system.

In common with many other organizational field sites, the laboratory is a site of specialized expertise. This raises some specific problems and issues for the ethnographer setting out to enter and gain analytic purchase in such a field. The foremost of these issues is that of specialized knowledge: how much is needed before entering the setting, and how much needs to be acquired as an ethnographer? Understanding the technical content of the work, and learning to understand technical content are both very useful ethnographically: not in themselves, but because of the purchase they offer on the meanings which participants give to events and for the access which they provide to the details of working practices. At the same time, however, not being a scientist provides the crucial ethnographic purchase which comes from questioning taken-for-granted practices.

Without a high starting level of scientific expertise, it is unlikely that in a short space of time an ethnographer in the laboratory will acquire enough competence to take a full part in the work. Roles are therefore likely to be limited, and without being a full participant it is easy to feel useless or self-conscious. In my own case I tackled this anxiety through a combination of strategies: accepting the discomfort as a price to be paid for being there; adopting an apprentice role where possible; and drawing on alternative skills in IT to contribute to the work. This latter strategy I drew upon only in the later stages and reluctantly, but it proved to be useful in foregrounding IT for discussion at stages when I was keen to explore some of my earlier observations with my informants.

Any ethnography, particularly one in such a setting, is necessarily partial. There will be locations and aspects unexplored. In particular, I felt that I was missing some aspects by not being in the laboratory at weekends, when I was told some people did a lot of their computer work. In addition, many of the researchers had computers at home, and I was unable (or unwilling) to follow them to their homes and see what work they did there. The laboratory is only a small portion of the lives of the people who work there. Strategic decisions have to be made about what is practical and what is likely to be the most efficient use of limited time. In my case, I chose to focus on the physically bounded location of the laboratory and the normal working hours. My ethnography was extended outside that space and time only insofar as my informants told me about their lives outside.

Finally, reporting on an ethnography within a highly specialized technical setting can also be problematic: readers need to be given enough insight in order to interpret the analysis, without subjecting them to a science lesson combined with an ethnography. In part, I hope I have achieved this by allowing enough time to elapse before telling the tale that many of the technical details have been forgotten: what remain are the crucial elements in telling the ethnographic stories at the heart of the analysis.

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Ethnography in the Science Museum, London

Sharon Macdonald

Why carry out an ethnographic study of an institution? What can be gained from an ethnographic and anthropological approach? And what kinds of problems and issues is the researcher likely to encounter? In the following account I attempt to give some answers to these questions through providing a case study based on ethnographic research which I have carried out in the Science Museum, London. My aim is to highlight some of the information and insights an ethnographic and anthropological approach is able to provide, and to discuss some of the difficulties of trying to analyse and write about such research.

The Science Museum is itself, of course, a specific kind of institution and therein lies some of its interest to an anthropologist: Why gather up all these artefacts? Why put 'science' on public display? How do those working in the Science Museum see their task? At the same time it has parallels with many other kinds of institution in which ethnographers of organizations might work. In its creation of exhibitions, it is part of the culture industry – those institutions involved in creating cultural products (including film, television, newspapers, advertisements). It is also part of the leisure and tourist industry; it has a research and educational function; and it establishes links with universities, schools, and scientific and industrial establishments. Furthermore, many museum staff themselves are technically civil servants and members of public service trade unions. Moreover, as a large public institution, it is also subject to many of the same managerial structures, difficulties, and fashions as are other (especially large and public) organizations. And, like many organizations, it has well-educated staff, many of whom are actively engaged in reflecting upon the organization in which they work and who are well able to give accounts of it. This latter raises a question about what an *ethnographic* approach can add. Why not just ask the staff for their account? To address this question, I begin first with a discussion of the ambitions of ethnography. This is followed by some general background to my Science Museum research, its aims and context; and then I offer a more detailed discussion of how these 'ethnographic commitments' were fulfilled (or otherwise) in this research.

Ethnographic Commitments

Ethnographic research is sometimes seen simply as meaning 'participant observation': 'participant observation' entailing the ethnographer participating in, and observing, daily life. While 'participant observation' is generally a key dimension of ethnographic research, most anthropologists understand the term more broadly. This is well expressed by Daniel Miller, an anthropologist who has used an ethnographic approach to subjects such as 'capitalism' and 'consumption' in industrialized and multicultural contexts including Trinidad (1997). He suggests that ethnography is characterized by a 'series of commitments that together constitute a particular perspective' (1997: 16). These are:

1. To be in the presence of the people one is studying, not just the texts or objects they produce.
2. To 'evaluate people in terms of what they actually do, i.e. as material agents working in a material world, and not merely of what they say they do' (1997: 16-17).
3. To 'long-term commitment to an investigation that allows people to return to a daily life that one hopes goes beyond what is performed for the ethnographer' (1997: 17).
4. To holistic analysis 'which insists that . . . behaviours be considered within the larger framework of people's lives and cosmologies' (1997: 17).

In other words, ethnography is not a 'mere method' (1997: 16) but is a broader 'approach' and may itself incorporate other 'methods' besides participant observation. For example, ethnographers may also carry out interviews, undertake historical and survey research, and analyse texts and other representations produced by those they are studying. The aim is to bring together whichever methods seem appropriate to try to understand the social life and cultural assumptions of those being studied. For anthropologists, this is also coupled, implicitly or explicitly, with a 'relativizing' perspective – i.e. trying not to take features of the group being studied for granted but attempting to see what it is that is specific about them through thinking about how they might be otherwise. This may be done, implicitly or explicitly, through cross-cultural examples. By thinking about how people in another part of the world do things differently, anthropologists can raise questions about aspects of social life and local knowledge that might more usually be taken as given and not questioned. In so doing, anthropological ethnographers generally accept that they may find their research moving into areas that they had not originally expected because these turn out to be significant in the worlds which they are investigating. This open-ended flexibility is also an important feature of an ethnographic approach.

Below, I take each of Miller's 'commitments' in turn and discuss each more fully in relation to my research in the Science Museum. My aim is to try to show why each is important and how each can contribute to an understanding of a particular organizational culture, in this case that of the Science Museum. First, however, I give some background to the research.

Ethnography in the Science Museum: Background

The research was funded under the ESRC's 'Public Understanding of Science' programme; and its aim was to look at the kinds of explicit, implicit, and practical definitions that museum staff made about science, how this was reflected in the exhibitions that they produced, and what visitors made of this.[1] The ethnography focused on the making of one exhibition in particular, an exhibition which opened in October 1989 as *Food for Thought: The Sainsbury Gallery*. This is a large exhibition on the subject of food – particularly changes in food availability and choice in Britain in the twentieth century – which at the time of writing is still in place. Ethnographic research on the making of the exhibition took place for the year leading up to the opening; and further research on the wider Science Museum context and on visitors to the exhibition continued after this, with funding for the project coming to an end in September 1990.

Six members of the museum staff were the 'Exhibition Team'. They were primarily responsible for defining the content of the exhibition and for organizing it into being. In doing so, however, they were in turn 'managed' by their superiors in the Museum (the 'Head of Public Services' being their 'line manager', he in turn being managed by the Museum Director), and they were involved with many others who also played a part in the making of the exhibition. These included exhibition designers, nutritional advisers (university professors), industrialists (from whom various food-processing machinery exhibited in the exhibition was acquired), educational advisers, experts on creating interactive exhibits, picture researchers, and many others. Because all decisions about what would finally be included would have to go through the Team, it made sense to be based with them. However, although Team members were based in two adjacent offices and had many collective meetings, there were many days when different team members were meeting different consultants or carrying out different activities. Decisions on whom I would accompany were generally made on the basis of either who was willing to let me go with them (a willingness which was generally shaped by how inappropriate they thought it might appear to those they were meeting to have an ethnographer

1. The research was directed by Roger Silverstone and it was based in the Centre for Research into Innovation, Culture, and Technology (CRICT) at Brunel University.

accompanying them)[2] or how interesting I thought it might prove to be. As all Team members reported back on their excursions at regular Team meetings as well as often discussing them informally in the Team offices, I was generally able to keep a reasonable track of the different activities of members, even where I had not observed them directly, though obviously this was less satisfactory than in those cases where I was able to 'I-witness' (Geertz 1988: 73ff.). The same was the case for telephone calls, an important means of communication in the construction of the exhibition. Lacking phone-tapping equipment, I necessarily relied on accounts related. Nevertheless, at the point where all events had to be 'translated' into becoming part of the exhibition itself – an 'obligatory passage point', as Bruno Latour (1987) calls it – I was able to observe that process.

This direct observational work generally involved me in sitting in a corner of the offices or other location (e.g. conference room, gallery, stall in Hyde Park) with a pen and notebook. This was not just being 'a fly on the wall', however: I took my turn making tea and coffee, accompanied Team members to lunch or to pick up faxes or to check on various exhibits, and sometimes joined in discussion (mostly by asking questions). In the Team offices, I would also use time when not much easily observable was going on – e.g. when all were at work on their own word-processors – to go through the filing cabinets of documents related to the exhibition. I tape-recorded Team meetings and most other formal meetings, such as those with designers. I also kept copies of as much relevant paperwork as possible (e.g. the various 'drafts' of the exhibition 'storyline'); and took numerous photographs of, and many notes on, the actual construction of the exhibition itself. In addition, I also carried out recorded semi-structured interviews with Team members at various stages through the process and after its completion; and with other Science Museum staff (in order to get a broader sense of key debates and issues on the subject of the representation of science and other relevant matters) and various other personnel involved in the construction of the exhibition (e.g. the Education officer, the Divisional line manager, the Museum Director). The study of visitors to the exhibition entailed a somewhat different approach and to some extent, as I have discussed elsewhere, raises rather different questions and, therefore, I do not consider it here.[3]

2. Where they thought it inappropriate it was generally because they had had difficulty negotiating their own access (e.g. to a particular food company headquarters). In some cases, they specifically thought that a person being contacted (e.g. graphic designers) would be interested to know of a study being undertaken and therefore encouraged me to attend.

3. See Macdonald (1993) and (1995).

Commitment 1: Being There

The great majority of analyses of museums focus on finished exhibitions, and generally provide a 'reading' based on the content and form of that which is represented. Such 'readings' tend to assume that finished exhibitions are rather unproblematically the product of dominant class, race, and gender interests. In recent years, such studies have been accompanied by a growing, though still small, number of studies of the consumption of museums and exhibitions. These have served to highlight the possible variety in visitors' own readings as well as, in some cases, particular cultural patterns and readings positioned in terms of class, race, and gender. As yet, however, there has been very little work on the *production* of exhibitions, and that which there has been has mostly been historical – based primarily on documentation – rather than on participant-observation ethnography. The question for ethnography, then, is: What can this give us that we couldn't get from an analysis of the finished exhibition?

For me, one of the interesting and surprising things about the *Food for Thought* exhibition when it opened was the extent to which it did not entirely 'feel' like the kind of exhibition which those making it had envisaged. During the making of the exhibition, a lot of the discussion in meetings as well as informal talk in the offices and over lunch had described the exhibition with adjectives such as 'lively', 'exciting', 'busy', 'buzzy', 'fun'; it had particularly emphasized the idea that there would be 'lots going on', 'lots of hands-on', 'it won't be boring'. The large flip-chart sheets produced during the early 'brainstorming' sessions show bubbles and words –food mountains', 'cosmetics (food for the face)', 'eating out (multi-ethnic/ethos of the chef)', 'hydroponics' – jostling together; and the early sketches produced by the designers have what was described to me as 'a market-stall kind of feel', with lots of different well-occupied and even over-flowing areas and cascading potted plants. In the brainstorming charts, in some of the ideas suggested, in others that circulated during the making of the exhibition, and in some of the suggestions for names for the exhibition, there was plenty of humour. One exhibit planned (and described in a press release) was intended to give visitors the 'experience of being a frozen pea'; there were early ideas for a robot for hamburger-making and an area for tasting foods; and the exhibition includes exhibits such as a giant pot of chocolate mousse, mirrors to make you look fatter and thinner, and a (non-functioning) McDonald's food outlet. Names suggested – many never really intended, of course – included 'Bread and Butter Show', 'Nosh', 'Grub Up', and 'Feeders Digest'. In the Museum the Team itself was often referred to, and sometimes referred to itself, as 'Fruit and Nut'; and Team members enjoyed a sense of 'winding up' more 'traditional' members of the Museum staff with suggestions of 'wacky' exhibits such as a giant cup of tea to be hung over the Museum's central atrium. There was also a sense of willingness to address controversial and politically difficult subjects: world food distribution and famine, industrial food production, food poisoning, diet, including consumption of fat and sugar. In all of this, the sense of 'difference' from previous exhibitions was frequently expressed by the Team during the making, a sense of difference that they articulated to their gender – an all-woman Team being unprecedented and frequently commented upon – and to their relatively 'non-scientific' backgrounds – most having degrees in subjects such as history and archaeology – and to their 'in-touchness' with popular culture and lay people, which they tended to see many other Museum staff as not having.

Given this sense of difference, of controversy, and of fun, that infused so many of the projections of the exhibition, the finished product felt 'rather flat' or 'a bit disappointing in some ways', as Team members themselves said to me afterwards. This shouldn't be exaggerated, of course; many visitors, for example, commented positively on the number of 'hands-on' exhibits and made favourable contrasts with other Science Museum exhibitions; and some reviewers did likewise. However, others also commented negatively on the extensive text ('too much reading'); the amount of text also surprised Team members when they saw the finished exhibition. There was also negative comment both from some visitors and from some reviewers on what they saw as a lack of proper engagement with the politics of food production, particularly with the role of supermarkets (something which they sometimes linked to the fact that Sainsbury's was the main sponsor for the exhibition).

One interesting question, then, which arose from the study of *Food for Thought* – a question that I had not anticipated – was why there was a disjunction between the process that in cultural studies is sometimes referred to as 'encoding' (Hall 1980) (i.e. the production of the exhibition), on the one hand, and the 'text' (i.e. finished exhibition) and at least some of its 'decodings' (i.e. interpretations by visitors), on the other. What was clearly evident was that just to 'read back' from the finished exhibition would have missed this disjunction and its accompanying complexity. For example, the exhibition could readily be interpreted – in a manner which is consistent with many text-based readings of museum exhibitions (and indeed other cultural 'texts') – as a product of dominant cultural interests, in this case those of food companies and market-led politics. The point is not that such readings are 'wrong' or, more generally, that there is no relation between production and product. Rather, what is interesting is *how* an exhibition envisaged in a particular way might end up being open to other kinds of interpretations. It is here that an ethnography of production – direct observation of the processes of exhibition-making in this case – can help to provide answers. More generally, this can also highlight the *complexity* of production, moving us beyond the rather simplistic, deterministic models typical of much cultural studies (see Du Gay 1997 for a summary).[4]

4. For an excellent, more general analysis of the difference between much cultural studies and anthropological research, see Werbner (1997). She argues that the former retells an 'allegory of resistance' in which 'class, race, gender, nation' are 'treated as analytic terms . . . [which] become stop words which block analysis' (Werbner 1997: 41, 45).

So, in the case of the making of the *Food for Thought* exhibition, when and how did it happen that the exhibition became 'readable' in the ways that it did?[5] To give a full answer to this question would take more space than I have here, and would involve a large number of different features and events, some relatively unpredictable (e.g. the failure of the frozen-pea experience to be completed on time), others somewhat unexpected outcomes of decisions about other matters (e.g. the 'invisibility' of the giant chocolate mousse pot to many visitors because of its size and position within the exhibition – so big one couldn't easily get far enough away to see what it was); others a sometimes unanticipated consequence of the way in which exhibition-making was undertaken (e.g. the primacy of the 'storyline' and the aim to 'convey messages'); and others a function of particular museological assumptions at the time (e.g. that visitors could learn better from 'interactive' than from static exhibits). 'Being there' certainly highlighted the numerous events and decisions that played a part in shaping the final outcome of the exhibition, and the variety of 'routes' – or 'biographies' (Kopytoff 1986) – by which different exhibits might have 'made it' to the finished exhibition. It also, of course, made visible the *exclusions* that happen during the making – artefacts, dimensions, and styles of representation which were once contemplated and perhaps even included until a relatively late stage, but which do not reach the finished exhibition. These 'dead ends', of course, are generally quite invisible to analysts who only read the finished text. Yet, as historians of science have argued of 'scientific failures' (e.g. Gooday 1997), that which doesn't 'make it' is no less interesting for that, and can be highly revealing of the implicit local knowledge and cultural assumptions involved.

To illustrate these points, let me give an example of a particular 'exclusion' or 'dead end', for by following this (as sociologists of science, such as Bruno Latour suggest, 1987) we can see some of the more general processes and tensions at work in exhibition creation. The example concerns a set of exhibits which in some

5. Of course, we might simply say that any exhibition (or other cultural 'text') is readable in infinitely variable ways and that these do not necessarily bear any relation to the text anyway. (This is a perspective sometimes voiced in museum studies and some cultural studies.) This seems to me to be not only analytically unhelpful – all we can do is list the variations – but also misguided. While there is, of course, individual variety in interpretations, and while some bear little relation to the 'text' itself, there are also identifiable patterns and commonalities in 'readings', and it is also possible to identify where a 'reading' is of a different text altogether. (Anthropology is helpful here in making us aware of how people from a different cultural background might interpret an exhibition rather differently, as Errington and Gewertz, for example, describe of readings of *Wuthering Heights* by an American and a Chambri teenager (1987: 128).) Some of the 'commonality' is derived from the common experience of those making the readings (e.g. experience of other media representations, shared cultural assumptions about food) but it also relates to particular features of the exhibition. The task is to identify these and to see how, when, and why the exhibition became readable in these ways.

of the early planning documents is listed as 'Superstition or fad in each area (carrots and seeing in the dark/garlic and Dracula/salt and the Devil/Shrove Tuesday/Ginger Bread men)'. Many Team members were extremely enthusiastic about these themes and one commented to me that this was part of the exhibition that she was especially looking forward to as she found this 'more beliefs and superstitions' aspect of more interest than 'some of the, you know, more production, sciency bits'.[6] So why did it not 'make it?' Looking back, we can see that some of the seeds of its exclusion were sown early on in the exhibition, though at the time this was not evident. Originally, the Team had begun to plan the gallery by using particular foods, or groups of foods, as the focus for different areas of the exhibition. Thus, peas and fruit, say, or bread and sugar, would be linked to particular production processes (e.g. freezing, canning, and jam-making; bread-making, sugar production; pasteurization, and bottling), to particular nutritional components (e.g. vitamins, carbohydrate, protein), and to the 'superstitions and fads'. However, when the Team presented these ideas to a panel of nutritional experts, the latter objected to the idea that foods be associated with particular nutrients. This, they said, was an outdated idea which nutritionists found unhelpful: the public should instead be encouraged to think in terms of foods as containing a mix of different nutritional components. Following this intervention from science (an intervention which we should note shows that exhibition-making was not a linear process of taking scientific ideas and packaging them, even though this is likely to be how at least some of those involved would describe it), the Team decided to reorganize the exhibition to remove the sections on nutrition and to bring these together in their own section of the exhibition, later to be named 'Food and the Body'.

Even at this point, however, the superstitions remained, and indeed, in some cases these sections seemed to be expanding as Team members found more that might be of interest to include in them. Of significance here was an elision which Team members frequently made between themselves and visitors or 'ordinary lay people'. This, in turn, was associated with their sense of difference from other Museum staff, and articulated to their gender and relatively low place in the Museum hierarchy to be allocated the 'jammy job' of exhibition-making. So, if Team members found a subject interesting, they would extrapolate that the general public was likely to do so also. In doing so, they tended also to regard 'science' as likely to be relatively uninteresting to visitors: 'science' needed to be 'dressed up', 'packaged', and enlivened by linkage with 'more interesting', 'history and culture' topics. This too created a rationale for including the superstitions and fads.

6. That identification of 'superstitions' and 'fads' as separate from other dimensions of the exhibition – dimensions which by contrast might be thought to be not a matter of 'incorrect belief' – is itself of anthropological interest here.

Their inclusion was overdetermined by another feature too: a desire to include 'objects' in the exhibition. 'Objects' to museum staff are not simply 'things', they are, in the definitions given to me by two curators, 'things to be put on a pedestal and worshipped' or 'anything with an inventory number'. That is, they are specially selected objects, worthy of becoming part of a museum collection. This does not mean that they necessarily have to be especially valuable or rare: the Science Museum collects many examples of 'everyday' items (e.g. domestic technologies, or artefacts which illustrate the use of a particular plastic or metal) as well as more unusual or historically singular items. *Food for Thought* was not, however, an exhibition with many objects, and indeed those working on it were given job titles of 'interpreters' rather than 'curators' (even though all had been curators previously). This had a good deal of significance within the overall politics and managerial structuring of the Museum for it meant that, in theory at least, this exhibition was not to managed by 'curators', whose primary affiliation would be to the objects (the term 'curator' being derived from the Latin for to 'care for'), but by 'interpreters', whose primary affiliation was to be to 'the public'. In keeping with this, *Food for Thought* was a 'message-led' exhibition defined by the 'messages' to be conveyed to the public, with 'objects' only being included where necessary to illustrate these messages. In practice, however, things did not always work like this for often those working on the exhibition would become attracted to – or even infatuated with (the language of 'love' and 'irrationality' was often used here) – particular artefacts or ideas. Moreover, in response to criticisms from some in the Museum over the likely paucity of 'objects' in *Food for Thought*, there was a desire by some Team members to try to include more objects in order to prove the critics wrong. 'Superstitions' was a dimension which had originally been thought likely to include objects. However, objects proved not to be quite so easily forthcoming or prolific as had been anticipated, and this too was to be a seed towards its later exclusion.

The demise of superstitions came in a particular intervention which was to prove crucial to transforming the 'feel' of the exhibition. A year before the exhibition opened, the Team gave a presentation of their plans to the Museum's Director and their line manager. The outcome of this was that a consultant was employed to work with the Team to help them 'clarify the messages of the exhibition'. 'Clear unambiguous messages' were to become the central determinant of all that was included in the exhibition: anything extraneous was to be removed. Moreover, messages were not to be a set of separate points, but were to be tightly linked together in a pyramid structure, such that more specific messages were to be subsumed under more general ones, and all were to fit neatly under what became the exhibition's 'central message': 'To help people understand the impact of science and technology on our food'. Every exhibit was to be justified by this 'rigorous logic' – a phrase that echoed repeatedly in the offices at that time, enough to become the subject of self-conscious humour (we even drank 'rigorous' coffee on occasion). Moreover, in this period of 'rigorous' editing, 'science and technology' (as in the newly defined aim) was given a renewed prominence in a falling back on readily identifiable institutional status in times of difficulty (a phenomenon which is probably quite frequent in organizations and which we might call 'organizational regression'). Superstitions and fads had no place in this redefined exhibition space.

Following through this 'exclusion' – a following-through based on being there – shows us, then, some of the processes and assumptions at work during exhibition-making: e.g. the consequences of the thematic model and the drive for 'clarity'; the separation of 'science' from 'superstition'. It also helps show at least something of when, how, and why the exhibition became less effusive and challenging than it had once seemed that it would be. But could I not have identified this just from interviewing staff? This brings us to the second commitment.

Commitment 2: People as Material Agents

The second ethnographic 'commitment' – to 'evaluate people in terms of what they actually do, i.e. as material agents working in a material world, and not merely of what they say they do' (Miller 1997: 16–17) – is clearly related to the first commitment. 'Being there', spending months carrying out participant observation, is premised on the notion that researchers can get 'more' this way than by relying on people's accounts of what they do. There are a number of reasons why this is so. First, which takes us directly to commitment 3 ('Beyond performance'), those we are studying may actually wish to dissemble or at least to 'tidy up' an account. In other words, what they say may be shaped through their own expectations of what they think we want to hear, or what they think we should not hear, or what they want us to hear. Erving Goffman has famously written about social life in terms of 'impression management' (1971) and as consisting of a 'front stage', where impressions are relatively managed and polished for an (outside) audience, and of a 'back stage' with more relaxed codes for 'insiders'. While the idea that social life can be neatly divided into two distinct realms, analogous to the difference between a restaurant and its kitchen, is obviously too simple, Goffman's account is useful in helping us to perceive the way in which participants may act differently in different contexts. The ethnographer is interested in both the front stage and the back stage and the interplay between them; whereas an account explicitly related by a participant would be likely to focus on the front stage. One of the points of ethnography is that the ethnographer should become able to understand how those being studied are likely to present themselves in particular contexts and why. In other words, through fieldwork the ethnographer is learning not just what people say and do but also what particular utterances and actions mean. I discuss these further in relation to commitment 3.

Another reason for looking at what participants actually *do* is that it may be very difficult for participants to describe this themselves because they take it so much for granted. They are likely to miss out things which an observer might find highly relevant, e.g. the layout of an office, the physical separation of particular tasks within an organization, the way in which people dress, or how they talk to one another. Moreover, participants may simply be too busy getting on with their tasks to be able to make note of what they are doing, and later they may either still be too busy to spend much time trying to recount what actually happened or there may be too much for them to remember in any detail. Certainly, for those involved in the *Food for Thought* exhibition, it would have been difficult to give a detailed verbal account in retrospect, given that so much actually went on. Any such offered description, as I discuss further below, would likely be framed within a culturally standardized account of the process: it would privilege conscious decision-making and clearly formulated plans over the contingent and messy.

Furthermore, *action* or *practice* is not necessarily readily translatable into words for participants (Hastrup 1995). Although it is easy to assume that all actions are preceded by clear cognitive 'decisions', and indeed this is entirely assumed by much management theory and by the notions of organizational process that operate in institutions such as the Science Museum, this is not necessarily the case (cf. Alvesson 1993). Material actions may be simply *performed* or *done*; processes set in motion may seem to have their own momentum. (For example, when I once asked a carpenter in the museum workshops how he would set about turning the plans by designers into a finished product, he looked at me in surprise and made an expansive, opening movement of his arms which I took to mean that you simply got on with it. He then showed me around the workshops, pointing out exhibits at various stages of construction and explained to me aspects of craftsmanship involved. The direct 'seeing' and *in situ* accounts of particular instances were the way in which he felt my question could be answered.) Moreover, as some sociologists of science have suggested (e.g. Latour 1987), we might want to attribute agency to non-human actors also (see also Chapters Three and Nine, this volume): in other words, in the case of the making of an exhibition, we might see the outcome as a result not just of human decisions and actions, but of those of the artefacts and exhibits themselves. Again, this runs counter to taken-for-granted ways of seeing the matter both for those involved and for the ways in which most social sciences conceptualize 'agency'. Doing so, however, can enable us to attend to aspects of exhibition-making and the outcome of the process that we might otherwise ignore: for example, we might note the weight-bearing demands made by some exhibits which meant that they could be positioned only at certain locations in the exhibition, so infringing on plans drawn up earlier; or the refusal of the pea experience to complete itself on time; or the obstreperousness of the chocolate mousse pot and exhibits nearby in making its very enormity a hindrance to its visibility.

In the case of the making of the *Food for Thought* exhibition, there was a constant attempt by the Museum to make explicit the processes and structures involved through plans and managerial restructuring, mission statements, aims and objectives, and the use of management consultants. Some of this could be seen as part of an attempt to lessen the agency of objects: in particular to create exhibitions which would be much more the result of visible, calculable 'decisions' than as flowing from collections and the relatively closed, esoteric knowledge and practice of a curator. Nevertheless, this 'making explicit' was its own particular kind of cultural account, containing its own selections, assumptions, and omissions. As such, there were aspects of material practice that such an account would be unlikely to note. For example, the 'love' of particular objects that I mentioned above would have no place in the 'rigorous' 'message-based' model that was being worked with. Nor, to take another example, would it be likely to include the numerous adjustments made as the exhibition was being physically installed. Yet all of these aspects would have material consequences in the finished exhibition and in its 'readability'.

Commitment 3: Beyond Performance

The third commitment is to 'long-term . . . investigation that allows people to return to a daily life that one hopes goes beyond what is performed for the ethnographer' (Miller 1997: 17). In our interest in what people actually do, anthropologists aim not to rely on the kind of performances that may be carried out specially for us. Again, this can be expressed in terms of an interest in the 'back stage' as well as the 'front stage': a full picture means trying to get both (and their variations).[7] This is a major reason for spending a considerable amount of time carrying out participant observation. Even though the ethnographer's presence is likely to be something of which those studied remain well aware, it is difficult to maintain a performance for outsiders over a long period. I certainly found during my fieldwork in the Science Museum that some staff, on hearing how long I was likely to be there, said that as this was for such a long time then they might as well tell me things which I would 'find out anyway'. I could also 'feel' the difference in the ease with which those around me talked and behaved as I settled into daily routines in the offices with them: gossip and jokes, and displays of frustration and anger, flowing more freely once I had spent more time there. Moreover, by being present over time the ethnographer should become better able to judge the kinds of shifts in presentation made for different kinds of audiences and so be more aware of the

7. For an excellent discussion of 'front stage', 'back stage', and impression management – and the ethical problems raised by the ethnographer's interest in the backstage – see Berreman (1994: introduction).

way in which he or she might be related to in different contexts within the institution. This 'reflexivity' – the attempt to understand how one is oneself perceived and how this may shape the research – is an important component of what is otherwise a rather 'naturalistic' ambition of trying to observe social life as far as possible in the contexts in which it generally takes place.[8]

Returning to the example that I have already described above, it was evident to me that the disjunction between expectation and the finished exhibition was in many ways surprising to the participants. This in itself suggests that their accounts, ethnographically interesting as they certainly were, would not be enough to provide an explanation. Moreover, when Team members talked about the exhibition after it was completed, the way in which they did so shifted depending on whom they were talking to. For the most part, the accounts that they gave were decidedly 'up-beat': they said how pleased they were with the finished product, how they thought it had a good mix of interactives and traditional objects as they had hoped. They also responded robustly to criticism of the exhibition. For example, when one review said that the exhibition was just what Sainsbury's would have wanted, Team members recalled examples which they felt indicated the contrary; and when they heard that it had been said that the exhibition 'did nothing for the representation of gender', they argued fiercely that just the fact of having some images of women in the exhibition was different from many Science Museum exhibitions but also that 'just because we're women', people should not expect the exhibition necessarily to be about gender politics. The point here is not that these positive and defensive accounts by the Team were in any way 'inauthentic' or even 'not what they really felt'. There was much that Team members were pleased with about the exhibition and for good reason. However, their positive accounting was also part of a more general cultural phenomenon within the Museum: exhibitions and persons were regarded as inextricably intertwined, with exhibitions seen not just as having been put together by particular curators, but as being a revelation of their personalities.[9] Because of this, and because, of course, exhibitions like any cultural product are framed and read in terms of the accounts that circulate about them, Team members worked hard to manage the impressions of the exhibition both during making and afterwards (at least until sufficient time had passed for personalities to be disentangled from products, for exhibitions to be consigned to an earlier 'childhood').[10]

8. For further discussion of naturalism and reflexivity, see Hammersley and Atkinson (1995). See also Okely (1996, esp. chs 1 and 2) and Hastrup (1995) for relevant discussion.

9. This is in interesting contrast to the formal public presentation of exhibitions which is as the work not of individuals but of the institution as a whole. The names of the curators/interpreters who work on them is not mentioned in the 'credits' at the entrance to exhibitions.

10. The notion of 'impression management', and its importance for institutions, has been elaborated by Goffman (1961, 1971). For some particularly interesting and insightful use of it, see Berreman (1994) and Law (1994), the latter dealing with managerial practice in an organization.

The point, then, was just that there was another dimension too which could only really be admitted in private, among Team members themselves and to those who they felt were sympathetic to them. I was privileged to have access to this more 'back-stage' dimension because I had accompanied them through the ups and downs of the making process and because I was there alongside as we tidied up, sorted out loose ends, and mulled over the adventure afterwards.

Ethnography, then, seeks to go 'beyond performance' not because performance is somehow 'inauthentic' but because the ethnographer strives to contextualize performance in order to understand the factors that made it possible. This cannot be done if we have only a single account to go on. How then would we be able to identify an 'extraordinary' event (Cohen 1983)? It is here that commitment 4 – to holistic analysis – is also crucial.

Commitment 4: Holistic Analysis

Holistic analysis, as Daniel Miller explains, 'insists that . . . behaviours be considered within the larger framework of people's lives and cosmologies' (1997: 17). Rather than focusing on particular behaviours or isolated views, the aim of the ethnographer is to try to understand what these mean to people and how they relate to other aspects of their experience. One aspect of this concerns the categories with which we begin our analysis. Although ethnographers today generally set out not simply to look at a particular people but to investigate a specific problem or domain of life, they nevertheless attempt to explore it within the terms and relationships which seem important to those they are studying. This may mean that they find themselves drawn into all kinds of concerns that they had not anticipated.

For example, although the main aim of the research which I carried out in the Science Museum was to focus on ideas about 'science' and how it was constructed in exhibition-making, I kept finding myself drawn into other matters that did not always feel as if they were much about 'science'. The politics of the institution itself, and more specifically the managerial restructuring, was one of these. It was the subject of considerable discussion among Museum staff. As I came to realize, however, it was also crucial to the way in which 'science' was being represented in the Museum. For one thing, the managerial restructuring was largely concerned with changing the priorities involved in exhibition-making, placing visitors rather than collections at number one. This had consequences for 'science', for it led to it being defined not so much as an objectifiable practice, located in particular tangible products, but as a more abstract 'message-based' conceptual matter. This latter, being defined primarily in relation to the visitor, was also often directed not so much at general scientific principles as at more individual experiential matters (e.g. giving visitors the experience of being a frozen pea, inviting them with 'trick' mirrors to see how they might look a little fatter or a little thinner). Moreover, the

very clear-cut hierarchical managerial structures, in which the different museum functions became more precisely defined and separated off from one another (each 'division' in the new managerial scheme being intended to have its own clearly stated aims and remit), was precisely replicated in the editing process which led to 'superstitions and fads' being eliminated from the exhibition and, more generally, to the 'flatness' of the finished exhibition. Just as the rigorous exhibition editing process resulted in eliminating anything which did not fit clearly into the newly defined structure, however much those involved may have thought certain topics or objects interesting or valuable, so too the larger scale 'editing' of the museum staff – through restructuring, redeployment, and sometimes redundancy – meant that only tasks and personnel which had a clear aim and place within the overall structure would remain.[11] The emphasis on 'accessibility' and 'clarity', like the emphasis on 'experience' and 'messages' and the broader matters of managerial restructuring and, beyond that, Thatcherite politics, then, were not somehow separate from science, but were part of the means through which it was constituted for the public.

This is not to say, however, that ethnographers must necessarily delve into every nook and cranny of the lives of those they are studying. It is, rather, a case of attempting to understand the context in which whatever we are exploring operates. Ethnographers do not necessarily, therefore, have to follow those they are studying home (cf. Hine, p. 74). Although one might certainly learn some interesting matters about those who work in an organization by doing so, not only is access likely to be difficult but also, especially if those studied make a distinction in their lives between 'home' and 'work', it may not reveal much about the organization itself. More useful than details of individual lives might be going beyond those who are the direct focus of the study to try to understand other similar or related contexts.[12]

11. I should note that while I have identified a close correlation between the processes involved in exhibition making, the wider managerial restructuring, and wider still processes of a cultural emphasis on 'the public' as 'consumers' rather than 'citizens', I would not wish to give the impression that these would always map neatly on to one another. If this were the case, it would be possible to just 'read off' a finished exhibition in relation to broader cultural politics. However, there always are exhibitions and museums which are critical of, or out of keeping with, the dominant political directions of the time. It is more useful, I think, to see each of the junctures between these 'levels' as a site of potential contest or struggle for definition. Thus, the Museum was not entirely putting Thatcherite principles to work; and nor were those creating the exhibition simply going along with this. This is why the Museum and the exhibition are more complex than the deterministic account would expect. What is interesting in this case, it seems to me, is that in many ways it was the organizational/editing model – something which was regarded as somehow 'neutral' or 'objective' – which was responsible for the 'flattening' of the exhibition.

12. For another example in relation to 'science' which takes a still wider approach than my own, see Emily Martin's *Flexible Bodies* in which she explores ideas about the immune system as far afield as 'outward bound'-style management training courses (Martin 1994).

Thus I found that visiting other museums and heritage sites and talking to staff there, and attending museological conferences, and reading newspaper coverage of museum matters, helped me to understand more about the 'museum world' (as it was sometimes referred to), its cosmologies, and its contests. This in turn helped me to 'situate' assumptions, processes, and contradictions involved in the exhibition which I was looking at in detail.

Anthropological Expertise?

Commitments, of course, are not as easy to keep to as to state and they can raise their own particular difficulties. Being allowed 'back stage' and witnessing 'beyond performance' raises problems of confidentiality, privacy, and trust. Confidentiality and privacy of respondents are likely to be hard to maintain in the case of a singular institution which cannot easily be disguised (cf. Chapman, p. 31). The ethnographer's crossing into domains usually restricted to outsiders can create difficulties over what can and should be revealed. Berreman (1994) describes this as an ethnographic dilemma of trust: being allowed 'back stage' may implicitly entail being trusted not to divulge that which is kept back stage. Institutional conventions, as in the Science Museum, making a sharp distinction between critical reports produced for internal consumption, and impression management – 'good PR' – for external, can lead to difficulties over forms of writing and publication. Such difficulties can be compounded by a specific version of the trust dilemma which is also an ethnographic version of curatorial 'object love': having been deeply entwined in the lives and views of those with whom I spent so much time, I felt a sense of protectiveness towards them. Yet at the same time I also felt critical of some of the things that they had done. These different impulses were not easy to marry; and all made writing an especially awkward process of negotiation and expression. The negotiation was both with myself – between my analytical and protective sides – and with museum staff. Having agreed to host the study, naturally they were keen to read its outcomes.

I have written about some of the reception of, and negotiation over, my accounts of the Science Museum elsewhere (Macdonald 1997). Here, I want to look at the writing problem slightly differently and to ask, as many in the Science Museum did, especially in the face of some of my work, what was in it for them? Being aware of, and highlighting, complexity, or pointing to the commonality with broader cultural changes, may be all very well, and it may well help move beyond certain academic simplifications (e.g. deterministic readings of exhibitions) as I hope to have shown, but is it in any way *useful*? Much though I think that we should resist judging knowledge predominantly in terms of its utility, the question is a legitimate one from the perspective of those who have hosted an ethnographic study. Why should they agree to let somebody hang around for months on end, taking notes, asking questions, and tape-recording?

Of course, this goes back to issues of access. Few organizations are likely to grant this without some expectation of results which may prove useful to them. In the case of the Science Museum research this was implied through the title of the ESRC programme – 'the public understanding of science' – under whose auspices the research was conducted rather than through any particular promised piece of writing, though from early on I said that I hoped to write a report for them and I did so. The reception of this, however, reveals some of the potential problems with an ethnographic approach from the point of view of managers within an organization. Having, through the fieldwork, become well aware of the format of management reports, I attempted to set my own report out as clearly as possible, using numbered sections and short paragraphs; and on the advice of one member of staff who read a draft, I included a bullet-pointed summary of the key points which, my advisor said, was all that would be read by senior museum staff. When I was told by the Exhibition Team manager that she had been told to 'implement all [my] points', I realized my advisor had been rather optimistic even on this score (on that occasion at least), for very few of my points could be turned into improvements to the now finished exhibition. But did this make the research 'useless' to the museum?

The problem here, I think, is a particular version of an old anthropological chestnut: the generalizability of anthropological findings or, as Hastrup (1995) puts it, translation of ethnographic experience into a form which can 'travel' (cf. Clifford 1997). The line manager who instructed the 'implementation' of my report saw a title and research on a particular exhibition and, logically enough, suggested that the outcome be applied to that exhibition. But my account was mainly of its production; and such generalizability as there was concerned *processes* of production. Most of my report was set out in terms of the innovations that the Team saw themselves to be making, how each was implemented, why, and how it turned out. Insofar as such innovations (e.g. the use of the pyramid organizational structure discussed here, or of a multimedia approach) were ones in which many in the museum world were interested, I saw this as offering concrete information on what can happen when you try to put them into practice, together with my own anthropologically informed discussion of some of the possible pitfalls. This was where its generalizability lay as I saw it – as a kind of cautionary, and sometimes inspirational, tale for future exhibition-makers. And this indeed is how it has been taken up by some at least. By comparison with the prescriptive recipes offered by many museum consultants – 'This is how you should do it!' – mine is, however, a fairly modest and even cautious form of travelling. Perhaps I should have been bolder? Perhaps. In some ways that is an attractive proposition. However, the kind of complexity that ethnography highlights also gives good reason to be cautious of simple prescriptive recipes; and, rather than aping much current practice, I think that anthropologists should be bolder in arguing for the potential helpfulness of their perspectives. The museum story that I have told in this chapter is, after all, about problems with simple or 'rigorous' models whose full implications (in this case for the politics and poetics of the finished exhibition) are screened out by the very 'single-focus perspective' that such approaches produce.[13] It is to just such matters that anthropologists can usefully draw attention. And just as Malcolm Chapman argues from his experiences with the business world (Chapter One above), this is something to which many of those working in organizations are likely to be sympathetic, aware as they are from their own first-hand experience that practice is rarely (if ever) as described in textbooks.

But merely to recount a general story of complexity, of actions having all kinds of implications other than those expected, is not enough. As with any anthropological account, we need to be able to 'lift out' more general patterns, connections, and dilemmas from direct ethnographic experience. The ethnographically based text needs not just to recount managers' own experiences back to them but to try to highlight the way in which certain implicit models (e.g. the 'direct through-put' knowledge model inherent in the way in which this exhibition was organized) or cultural assumptions (e.g. that 'messages' need to be expressed in words) lead to unanticipated effects (e.g. the 'screening out' of certain, and in some cases politically important, knowledges or an unexpected predominance of writing in the finished exhibition). It needs to show how highlighting those models and assumptions in future can help avoid such effects. This entails *analytical reflexivity*: a process of careful reflection upon the cultural context and processes examined with a view to identifying the particular formations of knowledge and practice operating within that organization. This is something for which anthropologists are well trained, used as they are to looking and listening carefully, to following their respondents where they lead them (often literally as well as metaphorically), to gathering multiple perspectives and contextualizations, to recognizing that apparent minutiae and mundane activities may be highly significant, and to 'relativizing' that which is observed, i.e. conveying how it might easily be otherwise. All those ethnographies which anthropologists read in training and continuing anthropological engagement provide not only possible models of ethnographic practice and anthropological theorizing, but also intellectually provocative examples of how some of our most basic cultural assumptions – about, say, the nature of personhood, material culture, time, or what is judged valuable in life – are far from universal. They help to highlight not just 'how things are' (or appear to be) but also, crucially, 'how they might be otherwise'. This, I suggest, is what the 'anthropological imagination' (cf. Mills 1959) can, and should, offer.

13. Marilyn Strathern makes a similar point when she writes, 'Anthropologists have no need to aggrandise their own accounts; in any case, to do so runs the risk of failing to see the work that aggrandisement does in human affairs' (1995: 180).

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Part III
Family, Health, and Welfare

–5–

Swords into Ploughshares: Manipulating Metaphor in the Divorce Process

Bob Simpson

Introduction

This chapter aims to show how an ethnographic approach can be used to understand the ways in which complex life transitions are generated and experienced in contemporary Britain. The case in point is that of divorce and separation. Out of the complex tangle of legal, economic, and social threads which make up this increasingly common transition in the life course, it is the day-to-day use of metaphor on which I have focused. More precisely, I demonstrate how an ethnographic approach illuminates different metaphorical complexes at work shaping the experience of divorcing couples as they move between different institutional contexts. Such an approach is important because the experience of divorce and separation is not so much an event as an extremely complex organizational process which effects a significant re-arrangement of emotional, economic, and legal ties and attachments. In recent decades this process and its longer-term consequences have become a common feature of family life but it is one which, in anthropological terms, is under-researched and under-theorized (Simpson 1998).

The particular aspect of this process which I would like to discuss here concerns the legal and welfare mechanisms encountered by couples when they formally end their marriage by means of divorce. When the divorce process is set in train a number of organizations, agencies, and professionals come into play to adjudicate, arbitrate, mediate, or advise upon the difficult and painful conflicts which arise when couples begin to dismantle their joint investment in family and home. Furthermore, the organizational back-drop is far from static. Organizational and institutional configurations change as legislation is adapted and reformed. This was evident, for example, in the ideological shift from 'parental rights to responsibilities' which underpins the 1989 Children Act, and in the move away from conflicted and adversarial divorce, which the 1996 Family Law Act seeks to foster. Change is also evident as different professional groups compete for ascendancy in the growth of occupations stimulated in response to rising divorce rates (McCarthy 1996).

At a time of serious personal disorientation a couple may thus find themselves being dealt with, both as a couple and as individuals, by a plethora of professionals each with their distinctive legal and bureaucratic domains and discourses. In a conflicted divorce a couple may find themselves at different times in discussions with solicitors, judges, barristers, probation officers, mediators, social workers, doctors, counsellors, benefits officers, housing officers, estate agents, and many others. In organizational and bureaucratic terms, the divorce process is not a clearly defined transition. For the couple, it entails passing through a kaleidoscope of organizational cultures which interlink and overlap and which are apt to leave those passing through confused and disempowered regarding their objectives, purpose, and procedures (Simpson, Corlyon, McCarthy, and Walker 1990; cf. Collins 1994). As one man I interviewed put it when being asked about the role of the Court Welfare Officer in his case: 'You'll appreciate, I've been to see so many people in the last three years . . . I didn't know where they fitted into the network and I didn't really know in what order one should be seeing anyone like them in similar circumstances.' At a time of profound personal disruption, men, women, and, on occasion, their children find themselves passing through some unfamiliar organizational contexts each of which shapes, directs, and gives meaning to this complex transition. The sphere of family life, which is usually taken to be private and discrete, is apt to be made uncomfortably public in the pursuit of justice, welfare, and the 'best interests of children' after divorce.

On the face of it, the organizational management of divorce is an unlikely context for an anthropologist to explore and it is my aim in this chapter to illustrate some of the analytical possibilities that open up once the stuff of ethnography begins to accumulate in notebooks, tapes, and the memory of experience. The first part of the essay considers the novel methodological strategies that might be used to conceptualize 'the field' in studies such as this. In the second part, a series of illustrations are given of the way that metaphors are used by professionals and their clients to make sense of conflicts arising from divorce. These illustrations demonstrate that the mechanisms available to deal with conflict are not linear but are themselves the subject of change and contention; the adversarial paradigms which previously underpinned family law have been progressively supplanted by ideologies which promote private ordering and the privatization of domestic life. Careful attention to the language of divorce as recorded in the various contexts and encounters that make up the divorce process enables one to move from the fine-grained reality of, say, an exchange between a husband and wife in the context of a conciliation appointment, to the broader ideological landscape within which divorce is situated. The final section presents a short case study which shows how in one particular divorce case different metaphors were used in different organizational settings. However, before considering metaphor in detail it is necessary to place this research into a broader context.

Ethnography: Process versus Place?

My involvement with the study of divorce and separation began in 1985 when I joined a multidisciplinary team of researchers engaged in a major divorce-related project. On the team were lawyers, economists, and social policy experts. I, a recent anthropology PhD, was brought in to work with another researcher on the qualitative dimensions of client satisfaction with the divorce process in general and dispute-resolution procedures in particular. Earlier research in this field had been criticized because it failed to examine the 'client perspective'. In other words, attention had been paid to professionals, processes, and outcomes, rather than to the fact that there were people being passed through these systems and, furthermore, that they might have things to say about the experience (Walker 1989). The project on which I was employed would be different from earlier ones in that considerable resources had been committed to eliciting and understanding the views of those who were directly experiencing the processes the project was seeking to evaluate. A cohort of couples would be identified as they entered courts and conciliation services and 'followed' as they passed through the domains of judges, registrars, solicitors, welfare officers, and conciliators. In this endeavour an anthropologist was deemed a useful addition to the disciplinary mosaic because it was assumed I would be (a) 'good' with the natives, (b) able to deal with the sensitive situations they would be experiencing, and (c) in possession of knowledge of kinship, and therefore might have something interesting to say about the changing shape of the nuclear family which was ultimately what the project was all about.

As it turned out, what I thought was going to be a brief sojourn in the world of social policy turned into a rather longer stay. Over a seven-year period and under the auspices of a variety of research centres I was engaged in a number of projects related to divorce and spent many hours in discussion with divorcing and divorced men, women, and (occasionally) their children. I attended court hearings and appointments, observed conciliation appointments, and interviewed conciliators and court welfare officers. Research projects undertaken covered post-divorce issues such as custody and child contact, use of welfare and legal services, housing, conflicts and dispute resolution, and most recently a study of post-divorce fatherhood (see for example Ogus *et al.* 1989; McCarthy and Simpson 1991; McCarthy *et al.* 1991; Corlyon *et al.* 1991; Simpson, McCarthy, and Walker 1995).

The research projects undertaken during this period were, to a large extent, driven by the policy agenda of the time. High on this agenda, then as now, were the unprecedented levels of divorce in England and Wales and the substantial costs engendered by changes in the way that families are structured and resourced after divorce. Of particular interest to those responsible for dealing with divorce and its consequences was the question of conflict: 'Who catches the fall-out when the nuclear family explodes?' as one writer pithily expressed it. The social and economic costs of dealing with divorce-related conflicts had risen inexorably and looked set to continue doing so. Disputes over children, finances, and property were finding their way in increasing numbers to the doors of the courts and of welfare agencies. These disputes were seen to create direct costs in the form of legal-aid expenditure to fund the pursuit of settlement through litigation and indirect costs such as rising welfare dependency and increased health provision. Longer-term costs were believed to arise from supposed links between family breakdown, crime, and poor school attainment.

Much of the debate triggered by these developments in the 1980s and early 1990s focused on reform of the legal machinery to process divorce and, in particular, on the theory and practice of conciliation (now more generally referred to as mediation). Drawing upon a broad tradition of dispute resolution which combined elements of interests-based negotiation, community justice, and traditional methods of dispute resolution, the emerging conciliation movement in the UK offered an attractive alternative to conventional adversarial models of conflict management. Its appeal was based on two sets of claims. The first was that conciliation would provide benefits to divorcing couples and their children by reducing conflict, focusing on children, and empowering parents. The second, rather more pragmatic attraction, was that this method of dealing with disputes could save considerably on legal costs and also increase administrative efficiency.

The original conciliation-project research developed out of government concern to evaluate such claims. The two questions which the research team set out to answer were: first, did conciliation save money by making the resolution of divorce-related disputes cheaper to manage and, second, did it improve the quality of the process by generating more effective agreements and relationships after divorce? The latter issue was deemed to be particularly significant in mitigating the impacts of divorce on children.

Throughout this period I was part of a loose network of researchers, academics, and practitioners engaged in the production, promotion, and dissemination of empirical research and its findings. The relationship between the producers and consumers of this type of research is a complex one. The model of the policy machine lubricated and adjusted with the benefit of research input is idealistic to say the least. An apparent hunger for information to service the policy process is contradicted by the fact that government is highly selective and discriminating in what is appropriate material when it comes to policy considerations (Weiss 1986: 221–3). Inevitably, attempts to formulate research agenda with these possibilities and constraints in mind shape the form and content of the research undertaken. For example, research must address clearly identifiable questions, it has to be turned around quickly, its methodologies have to be crisp, and its results easily condensed. Divorce as a complex, contingent, situated process must necessarily be recast as a simple mechanistic one for which it is possible to assume clear commonalities from case

to case. As an anthropologist I was clearly expected to lean in the direction of 'extensive' data and away from 'intensive' data (Geertz 1983; cf. Leach 1967).

Indeed, as an anthropologist working in a multidisciplinary team addressing such questions, I felt as though I was on a steep learning curve which often ran counter to my intuitions about the ways that people, communities, and organizations operate. Stimulating though the experience was, I felt that many of the strengths of an anthropological perspective were at best diluted and at worst rendered completely redundant in this setting. Take, for example, the notion of community. Community is central to any notion of participant observation as typically conceived within the methodological canon of social anthropology. There has to be some ongoing collectivity in which the anthropologist can locate her- or himself; without this it is difficult to develop a holistic picture of how that collectivity is given form and meaning by those who are committed in some sense or other to its continuity. However, as I soon found out, there is no community of the divorced, or at least not in the conventional sense of the term. Entry into marriage is marked by ritual and witnessed by representatives of state and the wider community of family members. What is being celebrated, to a greater or lesser extent, is the induction of a couple into the normative categories of Western domestic, social organization. Divorce, however, is quite the reverse: it is the movement out of these categories into unpredictable social terrains which on the face of it suggest fragmentation, isolation, and finality vis-à-vis the collectivities which went before. The 'kinscripts', to borrow Carol Stack's term (Stack and Burton 1994), available to those who formally exit nuclear family arrangements are still in the process of being written in Britain today. From a research perspective the problem would thus appear to be twofold: on the one hand the emergent collectivities of family life after divorce are difficult to study using 'extensive' research methodologies, but on the other hand their fragmented nature means that they are also difficult to study using conventional participant-observation approaches. In short, an altogether different conceptualization of the issue needed to be considered.

My own work in this field led me to explore ways of studying divorce and its aftermath as an expression of kinship which is not so much 'after nature' (Strathern 1992) as after affinity (Simpson 1998). The co-resident, heterosexual, nuclear family which is stable through time and made up of recognizable and predictable roles and relationships can no longer be taken as the blueprint for domestic life in the West. On the contrary, the life-course as it is centred on family and parenthood is increasingly fragmented and dispersed. The boundaries between public and private, market and household, interest and emotion are redrawn to accommodate individualism and democratization into the cultural and social fabric of family life (Beck and Beck-Gernsheim 1995). The conduct of family life is no longer discretely separated off from a 'heartless world' (Lasch 1977) but is in many respects shot through with it. An important dimension of this shift is the changing role of organizations and institutions in the re-structuring of family life, especially after divorce. Indeed, it would appear that in future the extent to which civil and market institutions will mediate the experience of family and domestic life will increase significantly (Robertson 1991).

In order to capture these fundamental changes in domestic and family life in the late twentieth century, novel methodological strategies needed to be considered. One which carries considerable appeal in this context is the idea of multi-sited ethnography (Marcus 1995). The idea of a multi-sited ethnography has developed out of a recognition that traditional ethnography – typically associated with the intensive study of a single place or context – fails to capture crucial connections, associations, and relationships that transcend particular localities. Failure to incorporate these wider connections into research obscures crucial dimensions of social and cultural life. For example, Hastrup and Olwig (1997) demonstrate the importance of this theme in relation to migration, identity, and the connections which are sustained between migrants across space and time. As many of the articles in their collection reveal, the impact of migration can only be understood by following people, sometimes literally, in order to track their connections. Other multi-sited ethnographic researches do not just present new ways of study but also posit new objects of study, such as in Ginsburg and Rapp's ethnography of reproduction and new reproductive technologies (Ginsburg and Rapp 1996) or Haraway's account of how recent developments in science and technology impact upon ideas of nature and woman (Haraway 1991). I would suggest that many contemporary Western family arrangements might usefully benefit from a multi-sited ethnographic approach. In recent decades the family has become less part of 'the essentialistically based architecture of unambiguous identity' (Beck 1997: 159) and increasingly dispersed, individualistic, voluntaristic, and public in character. As a consequence it is only partially accessible to approaches which presume discrete families and households.

It perhaps ought to be stressed at this point that at no time during the researches which I undertook did I think of what I was doing as a multi-sited ethnography. On the contrary, for much of the time I yearned for a single site within which I could be anthropologically at home. I consistently failed to find such a niche and, like some jobbing social scientist, turned my hand to all manner of methodologies and strategies for data collection as I played my part in actualizing the grand plan known as the 'research design'. Playing my part involved exploring different perspectives on the divorce process: interviewing a parent here, a solicitor there, observing a couple's appointment in a court one day and another's session with a conciliator the next. Throughout my immersion in the burgeoning services to cater for the growing number of divorced and separated couples I continued to note conversations and contexts, and I tape-recorded interviews and conversations for later transcription. It was only with hindsight that the mass of interviews, observations, and encounters began to take some sort of ethnographic shape.

The principal direction in which I have taken my analyses to date has been to focus on the continuities established at divorce. In other words it is not just about endings, but also about the beginnings of new kinds of social relation (Simpson 1998). Here I touch on a different aspect of this process, namely the role that organizations have begun to play in shaping and reinforcing certain kinds of relations after divorce. In the next section, I present an illustration of the analytical possibilities when data is gathered from a variety of institutional contexts all of which contribute to the creation of transition and passage. The data I rely upon is primarily language-based and focuses upon the way metaphors are used in different institutional contexts. The data is the kind which is culled from notes and transcripts of meetings and interviews during the course of prolonged fieldwork which is then sifted through in order to try to answer the question 'What is going on here?' One rarely knows the significance of particular conversations or dialogues at the time they occur and careful recording, both of what was said and of one's immediate and often intuitive commentary on it, is of the essence.

Close attention to the use of language in context, that is, not just what is said but how it is said, provides the means to access broader landscapes of culture, ideology, meaning, and identity. For example, I was able to sit in with a judge during his day-long processing of Children's Appointments (also known at that time as Section 41 Appointments). Such meetings normally took place in his chambers; there were no wigs and no oaths, just a few minutes' avuncular chat in which he elicited information from a sad and often fearful parade of mothers and fathers about their proposed arrangements for their children after the divorce. In the absence of a more rigorous divination, the arrangements so described are quickly rubber-stamped and one case ushered out as the next is ushered in. However, this judge ended each of his meetings with his own particular incantation: 'You may not be husband and wife any more but you are mum and dad for the rest of your lives.' For some reason this simple statement struck me as being particularly poignant and meaningful: the enactment of a special kind of knowledge which produces for the ethnographer what Strathern has recently referred to as the 'dazzle' effect (Strathern 1999: 6–11). Indeed, in my subsequent attempts to theorize kinship relations after divorce, the phrase proved to be an important encapsulation of the problem which divorce poses for the categories of Western kinship and the 'official' solution to this problem. In effect, the judge was asking parents to ponder on the verities of Western kinship: ties made by means of law (that is, being a husband and wife) are reversible whereas ties in nature (that is, being a mother and a father) are not (cf. Schneider 1968). Although divorce re-arranges and terminates the conjugal relationship, parental and, more specifically, paternal rights and responsibilities are expected to continue. In other words, mothers generally live with their children after divorce whereas fathers are expected to maintain economic and emotional links to them. Or, putting this opposition into social-structural terms, notions of patrifiliation are increasingly separated from and brought into conflict with the facts of matrifocality, a conflict which the state is keen to resolve or at least ameliorate.

Metaphors of Conflict and a Conflict of Metaphors

The example of the judge given in the previous section illustrates how, by unravelling a small snippet of linguistic and observational data, it is possible to oscillate between the particular contexts of people's experiences and the broader ideological and cultural frameworks within which these are suspended. Ethnography, as process and as product, is the device whereby events and structures are read one from another and conveyed in written form. In this section I want to take this notion further by paying attention to the way metaphors are used in different contexts within the divorce process. Again, the method relies on recording people's talk in formal settings such as interviews as well as in more informal conversations without necessarily being able to predict what data is likely to become meaningful and informative. Paying attention to these exchanges as discursive talk and narrative rather than as sources of factual information opens up the possibility of exploring 'experience near' concepts, that is, ones which are used 'naturally and effortlessly' by an informant to make sense of experience (Geertz 1983: 57). Paying attention to metaphor is particularly useful in this regard because not only does it reveal how people make sense of experience but also how in different institutional settings different metaphorical uses nudge participants towards different kinds of experience. First, however, a word about metaphor.

Metaphor is often taken to be the preserve of the poet and the writer for whom it provides a means to heightened forms of expressivity. In this sense, metaphors in language are performing a sort of meta-activity. However, there is a more prosaic approach to metaphor which locates it at the very heart of human experience. Following a tradition which includes Ricoeur and Lévi-Strauss, metaphor is taken as a linguistic device which uses experience from one realm in order to make sense of experience from another (the Greek *metapherein* means to transfer). Often the way that metaphor works is to take a difficult concept which is not easily articulated, described, or understood and to render it concrete, tangible, solid, and thereby comprehensible and communicable. For example, from aboriginal clans (Foxes and Bears) to crowds of football supporters (Magpies and Canaries) people make sense of their relationships to one another and to other similar groupings by analogy with natural species and their apparent differences (Lévi-Strauss 1962, 1966). In this sense, metaphor is not simply a linguistic ornament, but is the result of a creative act fundamental to human consciousness and understanding. Metaphors are not simply individual creative acts, however. They also belong to a community of experience and are, therefore, in a sense shared and maintained by the people who use them on a regular basis. This process is a highly dynamic and generative

one with metaphors not merely suggesting passive similarity but actively making connections and asserting similarity. For the researcher, being alert to the way that different contexts are brought together and in particular to the role of metaphor in this process is to glimpse the work of culture. Metaphors thus provide important paradigms for the organization of experience and as such are deeply political; they are an integral part of the power structures prevalent in a society, community, or institution. As Lakoff and Johnson point out, 'Whether in national politics or everyday interaction, people in power get to impose their metaphors' (Lakoff and Johnson 1980: 57). This point is particularly important when it comes to understanding the way marital breakdown is managed within systems of adversarial justice and the current quest for alternatives (Collins 1994). Understanding the role of metaphors in these different settings is thus just one way in which access can be gained to the underlying organizational and institutional cultures which currently structure the divorce process.

In the context of divorce, the important role played by metaphor becomes particularly evident when professionals and clients try to articulate the pain and complexity of post-marital conflict. However, the point I wish to make here is not just about metaphors of conflict, but about how a multi-sited ethnographic approach brings out a conflict of metaphors. Let me illustrate this point by beginning with Lakoff and Johnson's elaboration of the 'argument is war' metaphor (Lakoff and Johnson 1980: 4–5). In the West, they contend, there is a powerful metaphorical complex which pervades everyday language when it comes to thinking and talking about conflict. Quite simply, in talking about arguments, conflicts, and disagreements we slip easily into the metaphors derived from warfare and armed conflict. The language of war thus provides a handy and powerful device to make sense of rather less dramatic processes of argumentation. This metaphorical complex shows a high degree of 'coherence' (Lakoff and Johnson 1980) and, in Schön's terms, constitutes a 'deep' metaphor, that is, one that determines 'the centrally important features' of the system being considered (Schön 1993:149).

My own experience of people talking about divorce does not contradict Lakoff and Johnson's general proposition. Unsurprisingly, professionals and their clients draw upon the service of metaphor extensively as they try to make sense of the conflicts which swirl around the breakdown of intimacy, domestic interdependency, mutuality of parenting, and economic cooperation. For example, probation officers, judges, social workers, and even couples themselves will talk of divorce in terms of 'combat', 'fight', the 'rough and tumble of family work', and 'battle'. The outcomes of such encounters result in 'wounding', 'injury', 'pain', and even 'blood on the floor'. Divorcing parents will become 'entrenched' in their positions and, as one man said in grim assessment of the state of a dispute over access to his son, 'the battle lines are drawn'. In any war there are 'innocent victims' and 'casualties' and in divorce these are often identified as the children. As such, one mother expressed the need to keep her children 'shielded from the flak', that is, protected from the arguments which raged between her and the children's father. However, in the brutality of marital warfare it is not uncommon to find allegations that children themselves are being 'used as a weapon'. In a further elaboration of this metaphorical complex, a recent television documentary talked about how some parents in disputes over contact and residence were opting to use the 'ultimate weapon', that is, making allegations of child abuse against their partners.

The nitty gritty of divorce is enacted in a number of different spaces and these too are understood in metaphorical terms as places of competition and combat. Thus, the space set aside for the purpose of confrontation may be described relatively benignly as a 'playing field' which somebody might feel the need to 'level' or carried out in rather more ominous settings such as an 'arena', 'ring', or 'battlefield'. The professionals involved with divorce may also carry and reinforce these metaphorical usages. One judge commented how a particularly belligerent client had 'opened up the batting' with a question about a social worker's qualifications and competence. Another judge commented: 'It is not the task of the judge to jump into the arena and act as a sort of untrained, amateur, Court Welfare Officer.' In another telling example, an informant spoke of needing his 'gladiator' (i.e. his solicitor) with him in the arena (i.e. the court) for a forthcoming hearing over disputed access.

A particularly rich vein of violent and conflictual metaphors is drawn from interpersonal combat sports such as boxing. Men in particular slip easily into the extensive repertoire of metaphors generated by this sport and its variants. I have heard men complain that in trying to resolve conflict their partners are 'not playing by the rules' or are doing things that are 'below the belt'; they then feel that 'it's time to take the gloves off'. When things become particularly difficult, there may be a realization that 'we're just going to have to slug it out' and when that becomes too much, it may be time to 'throw in the towel'. Finally, the idea of divorce as some kind of pugilistic contest is given state reinforcement by the tendency of courts to report divorces publicly in the same way as all other civil disputes, that is, as a one-to-one encounter of the 'Smith v Smith' variety.

In the following example, several metaphors are brought together in one informant's attempt to explain to me the differences and similarities between going through the courts and going through conciliation meetings to settle a dispute over child access. In a discussion about the role of the conciliator operating in the court the man was critical of what had happened because the conciliator had not been a good 'referee' and he went on to comment: 'It was the difference between a street brawl and a boxing match: same end result, you end up with black eyes and bloodied noses. Same end result, just a different way of doing it.'

The above metaphors were all noted in conversations about a legal system which is essentially adversarial, that is, one which deals with divorce as a kind of contest

or dispute which is governed by rules of legal procedure and which is presided over by a judge whose role is that of ultimate arbiter. However, few cases ever reach adjudication by a judge and they are in fact resolved by a combination of private agreement between parties and negotiation on their behalf between solicitors and, on occasion, barristers (Davis 1988, Mnookin and Kornhauser 1979). In most cases the law provides a remote backdrop against which the day-to-day business of adversarial negotiation and contestation between husbands and wives and their legal representatives is carried out. Nonetheless, it is at this level that we encounter the extensive systematicity of metaphors among professionals and their clients. As I have indicated, the dominant metaphorical complex which occurred in general conversation about the divorce process was one which tends to see divorce in terms of battles to be fought and victories to be won, and the majority of disputants would seem to have little problem in casting the process in these terms.

One of the major claims of advocates of conciliation in the UK has been that it provides an alternative to the above system (Parkinson 1986: 67; Walker 1991: 262). Many such advocates took their cue from the Finer Report of 1974 (Report of the Committee on One-Parent Families) which was acclaimed at the time as bold and farsighted in identifying ways that the legal process might impact negatively on the lives of divorced people and in particular those of their children. The Report advocated radical alternatives for dealing with family matters in the courts. It was in the Finer Report that the seed was sown for conciliation as an appropriate way of 'assisting the parties to deal with the consequences of the established breakdown of their marriage' (Finer 1974: para. 4.288) and thereby 'civilising the consequences of the breakdown' (ibid. 4.311).

However, as King and Piper have argued (1990: 83–6), much of the energy which has gone into establishing the legitimacy of conciliation has involved a denigration of the law, or at the very least an attempt to displace legal discourse with ones imported from psychology, psychotherapy and other 'psy' discourses. These discourses operate with relative rather than absolute notions of truth and seek to reframe conflict by focusing on interests rather than rights. Parents are advised that court procedures are slow, unhelpful, disempowering, and likely to be bad for children (King and Piper 1990: 85). By contrast, conciliation offers the opportunity for parents to retain control of their disputes and to discuss their differences rationally in ways that will minimize damage to their children. High on the conciliation agenda is joint decision-making and joint responsibility for the future welfare of children.

Not surprisingly, the 'argument is war' metaphor is not particularly compatible with this approach to dispute resolution. Indeed, attention to the metaphors used in mediation settings reveals a subtle but significant shift in language use. Mediators endeavour to introduce a different metaphorical repertoire to describe the disputes presented to them and, furthermore, will try to challenge their clients when they slip into the 'divorce is war' complex. The metaphors preferred in mediation settings are consistent with an ideology that draws upon ideas of freedom and personal expression rather than the regulation and direction which informs the legal management of marital disputes. Mediators will seek to undermine and discredit clients' use of warlike metaphors and replace them with metaphors of organic growth and movement. As one mediator pointed out, 'We try to use the experience of divorce as an opportunity for personal growth, not defeat.'

Whereas the divorce is war metaphor is apt to leave people 'stuck', 'entrenched' in 'a war of attrition', the mediation intervention draws heavily on the image of divorce as part of a journey. The primary objective of those dealing with divorce in this way is to keep their clients moving. As one judge put it: 'The right word at the right time might set the wheels of conciliation turning.' The idea of motion is very important because it is itself a metaphor for inner states of growth and change. Such metaphors flow thick and fast in mediators' descriptions of their practice: 'We moved quite quickly', 'We were going too quickly', 'I went as far as I could go with . . .', 'I felt we had reached a milestone', 'We were on the right lines', 'To do real work with her she would have had to knock down those barriers', and so on. Parents themselves are apt to realize the switch in the dominant metaphor. One man commented regarding the inappropriateness of an adversarial stance in a conciliation appointment, 'It was as if we were trying to score points off one another.'

The shift in metaphorical usage identifiable in the move between different contexts is readily apparent in the practice of mediators. However, it was evident that in some contexts different metaphors were brought together like immiscible liquids. Conciliation is ideally a voluntary and non-coercive process. However, when practised in a court setting or non-court setting by people strongly associated with authority and adversarial proceedings, it is apt to acquire some of the baggage of those processes. For example, I have heard judges talk of welfare officers adopting a 'conciliation stance' or referring to the 'deployment of conciliation'. The militaristic and hierarchical feel of the process is further reinforced in court settings by referring to those involved in this process as 'conciliation officers'.

Finally, the conflict of metaphors is something that mediators themselves are increasingly alert to. John Haynes, a leading trainer of mediators, incorporates an awareness of metaphor into his writings on mediation theory and practice. His training materials (Haynes 1996) provide direction on the way that mediators might consciously manipulate metaphors to manage and resolve conflict. He provides examples of how in a mediation session a mediator might move towards the resolution of a dispute by continually re-framing client metaphors, that is, turning their swords into ploughshares.

The Case of Mr and Mrs Tate

Mr Tate left his wife and two children (8 and 11) suddenly and without warning. There followed a difficult period during which communication between the couple was almost non-existent and his contact with the children was infrequent and irregular. After three and a half years Mrs Tate applied for a judicial separation because she wished to formalize arrangements for the children's residence and their contact with their father. Both Mr and Mrs Tate were keen to pursue their different grievances over one another's behaviour through the courts. Although initially reluctant to go down the judicial route each had come to the conclusion that getting clearly defined orders made was the only way to deal with what each perceived as the other's recalcitrance and unreasonableness. He wanted to bring under public scrutiny her vindictiveness in preventing the children from having contact with him. She wanted to highlight her deep concerns about his long-running problems of alcoholism and the parental irresponsibility that this engendered. In court, neither of these issues was aired. At the Children's Appointment (S.41) the judge quickly identified the couple's lack of communication as an impediment to any agreement over arrangements for the children. He referred the case to the Court Welfare Officer (CWO) who spoke to the couple outside the court. He suggested that they should find a way of arriving at an agreed solution or run the risk of the judge making directions that neither of them would find acceptable. This rather ominous threat was presented alongside suggestions that the couple might benefit from the use of the local conciliation service. This view appeared to be reinforced in the couple's subsequent discussions with their solicitors. Mr Tate's solicitor had phrased it to him as follows: 'If you bounce ahead and get an order, it'll be against a backcloth of bitterness and resentment so you might as well try to get where you have to be through agreement rather than forcing it on your wife.' The whole court episode caused a good deal of frustration to the couple neither of whom felt they had been heard. Mrs Tate in particular felt that the judge and the CWO had 'taken sides' with her husband.

Mr and Mrs Tate each approached the idea of conciliation with suspicion and some trepidation. Indeed, Mrs Tate refused point blank to enter into a joint meeting with her husband. She was concerned that whenever they presented as a couple he always seemed to appear the more plausible and reasonable of the two and therefore gained the upper hand in negotiations. Two single appointments were thus set up which were, despite Mrs Tate's apprehensions, followed by a third joint appointment. Following this appointment, and a good deal of careful shuttle diplomacy by the mediator, regular contact between the father and the children resumed and the relationship between Mr and Mrs Tate improved significantly. In interviews some five years after the appointments the couple each spoke highly of the importance of the mediator's intervention (Mrs Tate described it as a 'turning point').

What the mediator appears to have achieved in this case was twofold. First, a context was provided for the couple to air (particularly in the individual appointments) all the issues which they felt had been overlooked and made to appear irrelevant, unreasonable, or silly in the court setting. However, as is invariably the case in such conflicts, major personal issues were blocking the possibility of resolution. He had grievances about finance and property, had acquired a new partner, and was coming to terms with the end of a long-running alcohol addiction. She had lost both her parents around the time of the separation. She was also angry that due to him she and the children now found themselves living in poverty which had a serious impact on the children. Both parties were nursing substantial levels of pain and anger following the ending of the relationship. The mediator had some success in getting each of the parties to acknowledge the other's feelings and to understand that their actions were not borne of irrational and unthinking malice; as Mrs Tate put it, 'she took both sides equally'. Second, the mediator had been able to bring the focus onto the children as the object of their shared rather than their competing concerns.

The dominant metaphor used throughout the accounts of Mr and Mrs Tate and of their mediator is one of movement. There is recognition that in the court they were 'in deadlock', 'at loggerheads', and 'going round in circles'. The move into the conciliation process saw them 'coming through the tunnel', 'moving towards agreements', and 'breaking the deadlock'. As Mrs Tate commented, 'It's been a slow journey to where I can chat with him.' In the mediator's account the image of a journey also appears frequently. The mediator described herself as having had to 'make a lot of the running' and in reflecting on the case drew together a cluster of metaphors of movement and direction:

> It would be very easy to get, you know, totally bogged down . . [] . . and in the end we might not have got anywhere with it. And I felt I had to keep on being quite persistent and I felt that with both of them. Certainly the husband, his instinct was to back off and she tended to side-track all the other issues . . [] . . . it was difficult to keep her on track, I mean I couldn't do it at all . . . [but] . . . because there were such big issues between them and yet at the same time I had to keep her on the main track of things and deal only with bits I could actually deal with.

Conclusion

In this chapter I have drawn attention to the role of institutions and organizations and their role in shaping the experience of divorcing couples who come into contact with them. Thus, what I have presented has been not so much an ethnography of organizations – that is, one that focuses on the spatial and bureaucratic existence of organizations – but an ethnographic account of organizational effects. In other

words, organizations are made up of rules, values, ideologies, strategies, and objectives which are enacted in practice on a daily basis and which have varying degrees of impact upon those who come into contact with them. In methodological terms, I have highlighted how careful attention to linguistic usage in different settings shows how couples going through the divorce process may be predisposed to see their conflicts in different ways. The notion of a multi-sited ethnography is presented as one possible way of capturing this aspect of complex life transitions in contemporary society. My analysis reveals a tension between constructions of the divorce process as enshrined in traditional, adversarial legal processes on the one hand and alternatives to that process such as conciliation/mediation. The latter are informed by an amalgam of ideas drawn from negotiation theory and various 'psy' discourses; they direct their users toward self-conscious revision rather than acceptance of normative prescriptions (Giddens 1991, 1994; see also Rose 1990). The shift to 'deep' metaphors of growth and movement in the management of post-marital disputes is thus pedagogic and not merely instrumental; the process not only resolves disputes but educates 'clients' in how to individualize and democratize the arrangements which increasingly characterize contemporary family life.

The processes I have described here have been in train for well over a decade and continue to be worked through in present-day legislation. One of the most recent manifestations of the shift towards private ordering and privatization of the domestic sphere and away from state regulation of the family is to be seen in the Family Law Act 1996. This Act has as one its major objectives an aspiration first mapped out in the 1974 Finer Report, namely, burying dead marriages decently rather than subjecting men, women, and children to processes likely to exacerbate already painful situations. The way in which this is to be achieved is by seeking to minimize distress to the parties and their children should separation occur, and by promoting continuing relationships, particularly between parents and children. The emphasis throughout the legislation is on a conciliatory approach to marital breakdown and its aftermath. The Act seeks to formalize and clarify the networks of professionals identified in the earlier parts of this chapter. In future, parties to a divorce will experience a combination of information giving, marital counselling, and mediation through which the pain of divorce will be assuaged, and its unpleasantness sanitized. Under this legislation, parties will be steered away from traditional, adversarial legal forums and encouraged to seek out the kinds of expert discourse which will enable them to fashion their personal and family relationships anew. Legislative developments of this kind pose new challenges for understanding the way law and policy shape family life. As Donnan and MacFarlane suggest, anthropology should be concerned with 'cultures of the policy professional, in penetrating and uncovering the perceptions and work practices of those who seek to make their definition of the world stick' (1989: 6). An ethnographic approach which can incorporate the dispersed networks and partial connections which characterize the working of modern legal and welfare institutions is an important step in understanding not only how definitions stick but how these definitions are contested and subject to change.

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– 6 –

Observing other Observers: Anthropological Fieldwork in a Unit for Children with Chronic Emotional and Behavioural Problems

Simon Pulman-Jones

Introduction

This chapter discusses the opportunities presented by some similarities between the practice of ethnography and the life of organizations. It is likely that the ethnographer of organizations will share much, if not most, of the social and cultural background of the people being studied in the organizational context. The question of such broad affinities, covered by the literature on 'anthropology at home', is not what will be considered here. Rather, I shall focus on two factors of particular relevance to the study of organizations. The first is the likelihood that the ethnographer will share a common *intellectual* background with those she or he is observing in the organizational context: that the broad tradition of sociological understanding that informs the ethnographer is also a significant factor in the identity and purpose of the organization. The second is that ethnographers and organizations share a common *'instrumental'* nature.

The common intellectual background and the common instrumental nature make it likely that the ethnographer will find that she or he will be using an intellectual toolkit, a set of fundamental intellectual resources, that also plays a significant role in the life of the organization. Organizations, and ethnographers, make interventions in the 'natural' continuity of social life. Unlike social forms such as family or kin groups whose primary purpose, if they can be said to have a purpose, is to reproduce themselves, most organizations have an instrumental purpose which is the basis of their identity. They have to define the world that they work on before they can set to work, just as do professional practitioners of social description such as ethnographers. Their engagement with the world therefore involves a basic repertoire that begins with naming/identifying/defining, then proceeds to involving the phenomena defined in the 'work' that they exist to do, and finally makes syntheses or decisions. They are likely to share the predicament of professional social describers in having a limited repertoire of logical possibilities for describing the social world on which they work. The phenomena with which they engage will be apprehended either as discrete atoms linked by relationships, or as a continuous fabric of relationship awaiting separation. And amidst all of this, in common with ethnographers and other intellectual workers, they will measure what they do to some extent through the difficulty involved in getting it done.[1]

The common intellectual resources to be considered are therefore those related to the requirements of organizations and ethnographers in performing instrumental processes on the world, and to the repertoire of possibilities available for describing the social world in terms of continuities and ruptures, of relatedness and separateness. What follows is a case study illustrating the affinities between the ways that ethnographers and organizations make beginnings with their material, and the ways in which theoretical preoccupations familiar to anthropologists may also play significant roles within organizational contexts.

Case Study

The case study that follows is based on fieldwork carried out at the Child and Family Department Day Unit of a large mental health clinic in a large city in Britain between April 1992 and July 1994. In order to have access to the Unit I worked as one of a handful of unqualified volunteers. I have changed all names of people, places, and institutions in order to preserve the confidentiality of my informants. The Unit provides a therapeutic school-like environment for twelve children between the ages of five and thirteen with severe emotional and behavioural difficulties. Children are referred to the Unit after coming to the attention of a local government Education Authority and being 'statemented' by an educational psychologist as having special educational needs. As a placement of last resort the catchment area of the Unit is the whole of the northern part of the city. The broad aim of the Unit is then to provide education in a therapeutic environment, combined with specific treatments such as psychotherapy, in order to allow the expression and management of the child's perceived problems with a view to return to 'mainstream' educational provision. In practice few children return to the mainstream and most of the work at the Unit is focused on management: helping children and parents or guardians to stabilize rapidly deteriorating situations, even if this means no more than providing a secure and calming environment during the school day so that problems are not as bad as they might be at home during the evening and night.

1. In the course of this chapter I do not mean to suggest that the parallels I draw between ethnographers and organizations are definitive. Organizations can be seen, and see themselves, as both 'instrumental' and 'natural', working both on the world and to ensure their own reproduction. The question of the relative balance of these elements in an organization's identity is a useful one. I am emphasizing certain similarities as a strategy for enriching the ethnographic process rather than as a way of defining ethnography or organizations.

This setting provided a situation in which there were psychologists, psychotherapists, and psychiatrists who work within a broadly similar tradition of social description to that of anthropologists, with similar fundamental intellectual resources and from a similar professional position. They manage the social reproduction and occasional innovation of ideas about how to treat the prototypical category of person, the child. It provided me with an opportunity to look at the relationship between the production and the consumption of concepts of the person, while remaining close to the material processes of social exchange in the movement of children, and responsibility for children, back and forth between families and professional carers.

Children who are placed at the Day Unit of the Clinic's Child and Family Department stay there for a period of up to three years. There are no specific rules or procedures governing the length of stay of a child at the Unit, this being decided by the interplay of several factors. Ideally, from the point of view of the Unit itself, a child would stay until he or she had been able to take full benefit from the Unit, leaving when staff began to observe a maturity and composure significantly beyond that of the majority of the other children. This is felt to take approximately two to three years. In practice, however, the child's period of stay at the Unit is determined by many other factors, such as the effects of budgetary constraints on the referring Education Authorities, which have to meet the costs of keeping the children at the Unit, or the progress of arrangements with fostering or adoptive homes for the children. As a result, the period that a child stays at the Unit may vary from a few months to over three years.

Methodological Affinities between Ethnographer and Organization

The business of social description and prescription which is carried out by the Unit is directly comparable to the project of social description in anthropology. The first stage involves the defining of terms: classifying the child; setting out the objectives and points of reference of ethnographic research. The second stage involves establishing the relationships between the previously defined terms: the participation of the child in the life of the Unit; the participating ethnographer's observation of the subjects' lives in process. The final stage involves a synthesis which includes the terms and the relationships between the terms in a formulation which can be translated into and used in other contexts: the child's prognosis; the ethnographer's theoretical observations. This sequence is common to intellectual activity in general. The value of drawing attention to it in this context is that the ethnographer of organizations may be able to observe that just as she or he faces the practical problem of marshalling available resources of attention to best effect, so too the organization, in fulfilling its instrumental objectives, has to structure and direct its attention. In the context of my fieldwork at the Unit this similarity was particularly evident because of the specifically forensic nature of the work done at the Unit. Just as I was struggling to be a successful participant in the Unit while also trying to discover aspects of life at the Unit which I could relate to the body of anthropological theory that had brought me there in the first place, so too the staff of the Unit were struggling to manage the practicalities of dealing with severely disturbed children while at the same time saving some attention in order not to miss any signs of abuse or pathology which, if they constituted adequate evidence in the relevant statutory or medical context, might entail significant consequences. In this case the affinity between the ethnographer and the organization is easily seen, both sharing investigative, forensic aims. Most organizations, being instrumental, share this characteristic to some extent. In organizational settings where there is a task to be delivered or a problem to be solved it will be useful for the ethnographer to be aware both of her or his own problems of marshalling resources of attention and of similar problems faced by the organization and those who work in it.

Social Definitions

The arrival of a new child at the Unit is a process that begins with an educational psychologist working for a local Education Authority or a local government Social Services department making a referral to the Clinic's Child and Family Department. The referral is the point in the process at which the Clinic first becomes aware of the child, and is the beginning of a period during which, if the child is judged to be an appropriate case for treatment by the Unit, and the referring authority is able to fund the placement, the Unit will get acquainted with the child. The process of acquaintance takes several forms and operates at many different levels. At one extreme is the exchange of psychological assessment information between the referring educational psychologist and the educational psychologist at the Unit. Less formal is the process by which the history and diagnosis of the prospective new child is communicated through a chain of meetings to the staff of the Unit. This process involves some of the technical descriptive terms of the psychological reports, but also relies heavily on an anecdotal shorthand that identifies the new child by comparison with children already known to the Unit. At the other extreme is the process by which individual staff and children weigh up the new child as he or she gradually becomes part of the Unit. Between these extremes are many other contexts in which the child and the Unit have to get to know each other, reflecting the fact that the Unit is a highly complex organization, the purposes of which are worked out at all available registers of social life.

This process of acquaintance is a necessary consequence of the fact that the Unit is an instrumental organization functioning as a part of the interventions made by the state in the lives of its citizens. The Unit is directed towards social phenomena (the child and family), which it describes in order to accommodate them to its

own terms of reference. What follows is a process of continual redescription until a final prescription (the parting diagnosis and prognosis) can be made.

The Acquaintance Phase

It might be said that it is inevitable that a newcomer will be given a different kind of attention to that given to someone who is well established within a particular environment. In the case of the newcomer, information is required which could not have been gathered until the arrival within the new environment. I would argue, however, that the mode of attention paid to new arrivals at the Unit does not entirely correspond with the need to put together a picture of the newcomer which suits the Unit's aims and terms of reference. The distinctive form of attention paid to a newcomer does not relax once enough is known for him or her to become a part of the Unit: it is not just a question of information. Rather, the intensity and duration of the period of acquaintance are dependent upon the relationship between the different modes of attention that the Unit operates through the career of a child in the Unit. At any one time, the Unit operates a variety of modes of attention in relation to the children, which can be divided into three basic phases. The first phase is that of defining, of labelling, and of static definitions, the most extreme form of which is the Statement of Special Educational Needs which places on an Education Authority the statutory obligation to provide appropriate education and care for a child. The second, central, phase is that of process and narrative, in which attention changes from the level of labelling and definitions to the relationships between those partaking in the life of the Unit. This is where the Unit feels its unique ethos to reside, where the organization is most strong and comprehensive. The third and final phase is that of the diagnosis in which the significance gathered from the second phase is translated through the layers of staff meetings and discussions to establish a point from which the child can move on.

Focus on Symptoms

Gemma was a seven-year-old girl, large and strong for her age, who had been placed at the Unit because her behaviour had become too difficult for the special school for children with moderate learning difficulties (MLD) at which she had previously been placed. Though she could appear calm and content, her speech and learning were severely below the norm for her age, and she was prone to sudden violent tantrums. She spoke in a bizarre and idiosyncratic way, beginning a sentence with a few intelligible words, but then trailing away into jumbled words and phrases, accompanied by knowing looks from coyly up-cast eyes, as if she were posing a riddle to be deciphered.

Gemma's mother was from the isle of Harris in Scotland. She had moved to Perth at the age of thirteen and her parents had divorced at about that time. She had moved to London with Gemma's father about five years before my fieldwork and had subsequently split up with him. She lived with Gemma and two younger children, and the father of the younger children, in a high-rise council flat which Social Services' staff had described as very untidy and dirty. They shared the flat with several pets which Gemma's mother said that she kept to compensate for the open spaces and 'nature' she missed in Harris. There had been long-standing fears by Social Services about Gemma's mother and step-father's violence, and about possible sexual abuse by Gemma's natural father, who made occasional visits to the flat.

At the beginning of her time at the Unit almost every aspect of Gemma's behaviour was a focus of concern; indeed when the Unit's senior child psychotherapist gave a brief presentation of her work on ritual satanic abuse, mentioning that one of the factors associated with cases of satanic abuse was the keeping and misuse of exotic pets, the fact that Gemma had mentioned that her step-father kept pet tarantulas in the flat gave rise to a frisson of suspicion that satanic abuse might be involved in her case. By the end of Gemma's first month at the Unit it was well established that there were significant grounds for suspicion of sexual or physical abuse. Attention shifted away from anecdotal evidence about Gemma and her family, and away from the existing education and Social Services case files, to the discovery of fresh evidence of on-going abuse, particularly in the form of bruises and other marks on the body.

As a result, staff started to focus less on the analysis of the pictures Gemma was drawing during the day in the classroom and more on her physical appearance as she arrived each day at the Unit after the night at home. Each morning when the children and the staff assembled for 'news' (morning assembly) in the hall space at the start of the day, the staff would exchange glances as they looked at Gemma for any signs of fresh bruising. On many days there were new bruises to be seen on Gemma's arms, legs, neck, or face and Gemma, aware of the attention, would, coyly and without making eye contact with any of the staff, turn her body and lift her skirt or jumper so as to make her bruises visible. On the occasions when no bruises were visible, the staff's attention would often fall on the fact that her clothes were either very worn or very dirty, causing further concern that she was being neglected. On those occasions when other children were commanding the attention of the staff she could make herself the centre of attention by such oblique strategies as falling off her chair or running into the alcove around the fireplace, behind the ring of chairs the children and staff used for the meeting. Direct attempts by the staff to question Gemma about her bruises caused her only to turn coyly away, or to respond with incomprehensible mutterings. During this period the attention of everybody was closely fastened on the signs that could be detected on or from Gemma.

The End of the Acquaintance Phase

The particular form of attention that I have described as being operated by the Unit during its period of acquaintance with a new child is not entirely a function of the need to gather information. If that were the case then one would expect that form of attention to last for as long as the need to gather information about the child. What other factors might bring about a change in the form of attention paid to the child?

Gemma was the focus of serious concern about possible sexual abuse from the moment she started at the Unit, this concern manifesting itself principally in the daily monitoring of bruises and signs of neglect such as dirty clothing. There was a clear and pressing need to do everything possible to protect Gemma and to stop her being hurt. Enquiries made by the staff directly to Gemma's mother on the occasions when she came for meetings at the Unit, and through her 'Home/School Book', which Gemma (like all the children) took home every night for her parents to read the class-teacher's comments about the day, were answered with tales of Gemma's clumsiness at home, given as the cause of all the bruises. Indeed Gemma *was* clumsy, often falling over as a quite transparent way of attracting attention, but concerns persisted and the matter was referred to Social Services. In the meantime, though, I noticed that after a couple of months the attention paid to Gemma's bruises began to wane. I was very struck by the fact that though, as far as I could see, Gemma was as bruised as she had been at the height of concern about her, she was no longer the subject of the attention of the Core Team staff at the morning assembly, and that attention paid to her cryptic comments in the classroom had also fallen off. Attention was now paid to Gemma for a different reason, for she had begun to display some of the behaviour that her previous school had found difficult to cope with and which until this point she had not shown at the Unit. She started to have tantrums which developed without warning (or so it appeared) and which would begin as a sulky unresponsiveness. When asked what was the matter, or asked why she was not joining in with the rest of the class, she would jump up and run away, shouting angrily, 'Leave me alone, I hate you, I hate you.' When this occurred in the classroom she would run out into the hallway and then up the main staircase and into the winding corridor that passed through several fire doors and unlit sections through to the far side of the upper floor where the therapy rooms were. When somebody followed her to check that she was safe, and to bring her back if possible, she would scream and shout even louder, kicking and struggling violently if any attempt were made to take hold of her hand, and repeatedly demanding what, in the circumstances, was impossible: to be left alone.

Gemma had begun to explore the Unit's reaction to the behaviour which had seen her rejected by her previous school, and in so doing she demonstrated that she had left the period of acquaintance, of close and anxious attention, and moved on to the main, central phase of the Unit's career with a child, that in which the repertoire of relationships with staff and other children is explored within the context of the closely defined spatial world and timetable of the Unit. The signs and behaviour which had drawn such anxious and intense attention when Gemma first arrived remained and, it might be assumed, required just as urgent attention, but from now on they were more or less overlooked.

I observed the same thing happening with other children. A six-year-old boy, an elective mute, was the focus of excited attention during his early period, as staff vied to discern whether his mutism stemmed from a genuine inability or an arch refusal to talk. Yet when he too started to run off into the building, and put himself in danger by climbing onto precarious high places, interest in the minutiae of his expressions when trying to communicate waned. In many other cases I observed chronic anxiety about minute symptoms of behaviour or affect suddenly fade as the children became incorporated into the life of the Unit.

What explanations can be given for this observation? It would certainly be true to say that in the case of a child such as Gemma, for whom there is serious anxiety because of possible sexual abuse, it is to be expected that when the initial urgency of the suspicions of abuse has been succeeded by the frustrations of trying to establish evidence and proof of abuse in conjunction with the statutory authorities, discouragement at how little can be done, despite the very distressing strength of the conviction that the child has been abused, will make the maintaining of an urgent vigil for signs of abuse seem of little use. When the child has newly arrived at the Unit it is understandable that everything might seem to be at stake; both in terms of the future of the child and in terms of the reputation of the Unit in relation to the referring authorities and the previous place of education. When, after a few weeks, it is apparent that the world will not fall apart if the Unit does not immediately reveal the truth of the child's condition and history, it is also understandable that the anxious close focus should be relaxed.

But I think there is a different explanation for the change between the close attention of the early phase and the broader attention of the middle phase. Because of the way the Unit has to work, the initial phase of naming and defining is inherently unstable. The process that begins with the Statement of Special Educational Needs and ends with the anxious gaze of a member of the Unit staff looking for definite signs of abuse or psychological disturbance – a process which aims to fix a picture of the child, so that he or she can become an identifiable and appropriate item to be moved between parts of the education and mental health care system – is anathema to the central process and ethos of the Unit which – far from fixing positions and identities – is designed to provide a safe and tightly defined context within which to allow the elements of the child's psychological identity to shift and reform into more manageable arrangements. The paradox is that the Unit needs somewhere to start, but from that point onwards aims to undermine the tyranny of

such starting points by working to multiply and shift perspectives. Such a range of perspectives may be seen in relation to a particular item of behaviour such as incontinence, which might be seen in the home environment as a rebuke to the authority of the parents; in the classroom at the Unit as a sign of a desire to withhold consent to participate in social groups and in the 'growing-up' of education; by the Unit's social worker as the occasion for a sharing of opinions from the whole of the family network; by a paediatrician as possible evidence of urinary tract trauma with slight possibility of sexual abuse; or by a psychotherapist as the articulation of a desire or intention largely unrelated to outside contingencies but of immense significance at that point in the narrative of that particular session. The basis of the ethos of the Unit is that as large as possible a range of different perspectives on the child will be found by the staff in the course of the Unit's daily life and that the child will be helped to manage life more easily if he or she is able to internalize a broader repertoire of perspectives.

I have already drawn on the fact that the Unit shares a common intellectual tradition with the social sciences, and operates, in a highly practical and contextually embedded form, a system of analysis that shares basic structural features with the social sciences. I have suggested that parallels can be drawn between the phase during which the Unit becomes acquainted with a child and the necessary starting point of sociological analysis in which social phenomena must be differentiated and defined. The necessity for a point of departure is in both cases a necessary evil: flawed and provisional. From the psychodynamic perspective, the early persistent and anxious focus on the newly arrived child is an invitation for something to *happen* with the child rather than for it to submit to definition. From this perspective it might be said that definition is perversely insisted on in the early phase, until the child, and the Unit's view of the child, are provoked into breaking free into the daily life of the Unit.

Affinities between the Ethnographer and the Organization

Having discussed some basic 'methodological' affinities between the ethnographer and the organization, I now turn to theoretical affinities. I have already noted that in my fieldwork at the Unit I shared an interest in the child as a prototype of personhood (from my anthropological perspective) with the people that I was studying (from their clinical perspectives). Awareness of theoretical debates was very much a part of the life of the Unit, even for those members of staff with no formal theoretical expertise.

By being equally an educational and a psychiatric/psychotherapeutic institution, the Unit takes on the contradictions between these two approaches. The main contrast in approaches is between one which sees the child as part of a network of family and wider social relationships in the context of a life career, and one which sees the child in isolation as the vehicle for the expression of symptoms of his or her psychological pathology. This contrast can be described as being between an extensive outlook and an intense internal focus and, while it is most often seen as the difference between the perspective of the teacher and that of the psychotherapist, it is equally present in the debates and struggles within the psychiatric team between behavioural and psychotherapeutic approaches, and in the competing claims of differing educational philosophies or requirements. Containing the contradictions between these different approaches (rather than being a Behaviourist *or* a Therapeutic Unit) is often talked about by the staff of the Unit as almost impossibly difficult, but ultimately it is felt that the difficulty is worth enduring because it allows the fullest possible treatment of the children. This difficulty presented itself in many different ways, but particularly where psychotherapeutic treatment, practical management of difficult behaviour, and disciplinary measures in response to seriously unacceptable behaviour came into conflict. During the period of my fieldwork, violence within the Unit was felt to be permanently at crisis level. Physical assaults, the throwing of sharp or heavy objects, and spitting (directed both at other children and at members of staff, predominantly the teachers and nursery nurses) happened several times daily, but periodically there would be an incident of such severity, or the sustained level of violence would become so intolerable, that the teachers and the nursery nurses would demand that a formal set of procedures be drawn up so that violent behaviour could be responded to by the temporary exclusion of children from the Unit. The psychiatric team vigorously opposed the exclusion of children, maintaining that the therapeutic environment must offer the children total containment. On several occasions the teacher in charge of the Unit took a unilateral decision to exclude a child, insisting that she *had* to protect her staff. On these occasions the psychiatric team made it clear that they felt that the teachers and nursery nurses were failing the children. On a day-to-day ad hoc basis there were 'behavioural' elements to the treatment that the children received. That is to say that the children were treated as if they were separate from the Unit and their behaviour could be modified if the Unit acted on them through such measures as the withholding of access to the garden at playtime or, *in extremis*, being excluded for a few days. But the open-ended therapeutic commitment to regard the children as belonging to the 'psychodynamic' whole of the Unit as a therapeutic environment was maintained in the ultimate refusal to draw up any guidelines setting out specific responses, such as exclusion, to unacceptable violent behaviour. Only through keeping faith with an unwritten treatment ethos was it felt that the Unit could continue its unique blend of treatments.

In the work of the Day Unit there is a link between the staff's ability to balance and reconcile the contradictory models of personhood – those reflected in the educational and the therapeutic elements of the treatment – and the child's ability to know himself or herself more correctly in order to be able to live more successfully.

Being couched between contrasting models of the person, the atom-self of boundary-drawing behaviourism and the inter-subjectivity of the psychodynamic, therapeutic tradition, the practical, working psychology of the Unit is a constant process of testing and reproducing the prevailing conditions of and for the nature of personhood, of the balance of boundaries and continuities in personal and social life. Strong, though only partly rationalized, links are made between the effort to understand and the effort to live successfully, so that a part of overcoming the gross difficulties of the lives of very unhappy children is the overcoming of meta-physical difficulties by the professionals charged with responsibility for the children's welfare. It was true for a majority of the children that their behavioural problems were so severe that they, their families or carers, and the staff of the Unit were faced with the constant imperative that if no improvement could be made the children would have to be, for example, taken into care, or moved to a secure psychiatric ward, or faced with drugs-based medical treatment. And thus practical problems of how to deal with the children during their day at the Unit were forced together with the problems of understanding both what was 'wrong' with the children and what could be done for them. Many of the children, including Gemma, were thought to present varying degrees of autism. But the theory that autism might exist in varying degrees is a controversial one. Some of the children presenting autism-like symptoms were thought by some members of the psychiatric team to be suffering from neurological problems that required urgent drugs-based medical treatment. Other members of the Unit's staff were convinced of the significance of a mild form of autism in these cases, while yet others just saw a desperately unhappy and withdrawn child. These diagnoses were thrown into constant competition through the system of daily and weekly meetings in which the staff, in groups varying in number from two to the whole staff, discussed how the Unit dealt with the children. The result of this was that people were aware both of the troubled history of the children outside and within the Unit and also of the various competing explanations for the problems, and the attendant different solutions. So in the daily, constant crises with the children, for example, in instances such as when it began to become clear during the course of an early morning that something was very much amiss with Gemma, the energies of the staff would be mobilized simultaneously to providing practical containment and comfort and to achieving some form of resolution to the intractable questions about diagnosis and treatment that the worrying behaviour gave rise to. Staff at the Unit live with the sense that if they could only, in that moment of crisis during the day, understand *the truth about the nature of, say, autism and the relative merits of psychotherapeutic or behavioural approaches to treatment, then they could offer some hope of relief to their desperate charges. The urgency of the predicament thus makes it seem that the overcoming of difficulties of understanding is very closely related to the practical problems of managing a specific child in the Day Unit.*

These theoretical concerns were very much a part of the staff's constant discussions about the children and about the nature and adequacy of the treatment that they were receiving. The sense of the importance of theory did not depend on consistent and coherent understandings across all members of staff; it consisted more in the awareness that there were always two basic and fundamentally opposed approaches. Terms such as 'behaviourist', 'therapeutic', 'psychodynamic', 'systemic therapy', or 'phenomenological' were used, not always strictly accurately in theoretical terms, to recognize the existence of antagonistic approaches which often entailed mutually exclusive rationales.

The importance of these 'theoretical' preoccupations was not limited to abstract discussion. They were reflected in the daily life of the Unit and in some of its most intractable problems.

The division of the staff of the Unit into two separate teams was both the most controversial issue and that which caused the problem that seemed most inescapable during the period of my fieldwork. The names themselves caused frequent grumbling. The Core Team (the teachers, the nursery nurses, and the administrative secretary) felt that 'core' as opposed to 'psychiatric' implied 'basic' as opposed to 'specialist'. At the same time the Psychiatric Team (two psychiatrists, a social worker, an educational psychologist, and a child psychotherapist) felt that 'core' implied 'indispensable' while 'psychiatric' implied 'external, optional extra'. On several occasions I heard discussions of the origins of the two labels, there being no consensus as to who had initiated them or how long they had been in use. There were two alternative labels, Educational Team and Clinical Team, which were occasionally used, pointedly, in meetings, but they were never used as the universal shorthand for 'the other lot'. There were indeed significant descriptive shortcomings to both labels. Only two of the five-strong Psychiatric Team were psychiatrists. Nevertheless, Psychiatric Team and Core Team remained the labels by which the two groups were known and in which rivalry and resentment continued to echo.

One basic fact divided and distinguished the two teams. The Core Team were with the children all the time and the Psychiatric Team were not. For the Core Team attendance at the Unit meant arriving before the children and leaving after they had left, united in containing the children throughout the day. For the Psychiatric Team attendance at the Unit meant leaving the main Clinic building down the road and arriving at the Unit in order to attend regular meetings, therapy sessions, or one-off meetings that occurred during the Unit's day. Whereas members of the Psychiatric Team tended to come and go individually, the Core Team arrived and left all at the same time. They gathered together for cups of tea or coffee before the children arrived and after they had left, their solidarity at the beginning and end of the day resolving its uncertainties.

The conflict between the continuous solidarity of the Core Team and the instrumental intervention of the Psychiatric Team was frequently evident in disagreements

that occurred during the weekly Community Meeting, or during subsequent discussions about the meeting. The Community Meeting, in which all of the staff and all of the children met together in the dining room seated around the walls in a circle for exactly half an hour for the acting out of the community's 'psychodynamics', was often extremely disturbed and chaotic, giving rise to very worrying behaviour from the children. It was to be expected that the Psychiatric Team, for most of whom this half-hour session represented their only direct contact with children other than those with whom they were involved in therapy sessions, would be more alarmed than the Core Team by the extremes of behaviour often seen in the Community Meeting. The Core Team could more easily see such behaviour as the expressive climax of a period of anxiety or unhappiness that a particular child was undergoing, and have confidence in the fact that the child would return to a more manageable state, whereas the Psychiatric Team were inclined to see such behaviour as a sign that things were far more out-of-hand than they, in their absence from the Unit, had assumed. The result would be that the Psychiatric Team would insist that a particular child, or sometimes the Unit as a whole, had reached a state of crisis which required an urgent response and wholesale re-evaluation. By contrast, the Core Team would respond that no such crisis existed and that the behaviour that had so alarmed the Psychiatric Team was either the result of particular circumstances, such as the temporary absence from home of one of the child's parents or a build-up of anxiety before the half-term holiday, or part of a period of emotional acting out that had been observed over a longer period and which the Core Team were confident would be resolved positively by the understanding and strategies that observation had given rise to.

The different responses to worrying behaviour in the Community Meeting illustrate the contrast between Core Team and Psychiatric Team attitudes: continuous solidarity, observation, and long-term strategy were opposed to the isolation of significant symptoms of behaviour and the need for urgent instrumental intervention. In practical terms this is an effective and complementary opposition, the deficiencies of each outlook being offset by the other, but in the conflict between the two staff teams it was rarely seen as such.

If the Core Team were the custodians of contingency, the Psychiatric Team, for their part, offered the benefits of being freed from contingency. They saw the children in highly formalized contexts which were sufficient in themselves, without any continuous supporting context. In contrast with the Core Team's internal life in the Unit, which was based on a shared knowledge and experience, with shared responsibility, of all that went on, the work of the Psychiatric Team was often unseen by the rest of the Unit. Members of the Psychiatric Team arrived at the Unit during the day for appointments and, having been let in through the front door by the administrative secretary, they passed unnoticed by the children and the Core Team staff to the rooms on the upper floor where they held meetings with each other or with children and members of their families, or where they had therapy sessions with the children. The practical basis for the Psychiatric Team's experience of time in the life of the Unit was the model of appointments in a daily diary, characterized by specific times for beginning and ending, with the absolute separation of one appointment and the next marked by the logistical requirement to travel between appointments in different buildings, or in different parts of the same building, and to keep on time. The theoretical model underlying this practical fact, and reinforcing it, was that of the 'therapeutic hour' of fifty-minute consultations, the beginning and end of which are observed rigidly and absolutely so as to mark the absolute distinction between quotidian time and therapeutic time.

The Psychiatric Team did not, therefore, share the Core Team's sense of continuity with the child. Seeing children on separate occasions, they saw them differently each time, noticing changes more readily than the Core Team (or, as the Core Team often insisted, seeing changes where none had really taken place). Whereas it was the role of the Core Team to manage continuity it was the role of the Psychiatric Team to enable change. Within the firmly policed boundaries of the therapy session new meanings are created for child and for therapist.

If the Core Team's continuity forms a part of the linear progress of the child's social career in life, and this is seen as a horizontal continuity, the therapy session, by-passing the threads of contingency, aims a vertical probe into the psychological history of the child. Whereas the Core Team staff member in the classroom is placed within the spatial disposition of other containing presences and the certainties of the unfolding timetable, the Psychiatric Team member in a therapy session is alone with the child in a bounded world in which space and time are collapsed into the therapist's focus on the psychodynamic significance of the child's behaviour.

The contrasting models of time and space that I have described above in relation to the roles of the Core Team and the Psychiatric Team, which each imply different models of personhood in their attitude to the children, are not entirely exclusive to each team. The antagonistic pairing of the two teams, together with the requirements of their respective roles, commit them towards one model rather than the other, but both, as mutually reinforcing opposites, are always co-existent.

'The Split'

So far I have described the way in which a basic repertoire of complementary ideas about personhood, rooted in the same traditions of social description as inform the ethnographer, structured the treatment the Unit offered the children. But this binary opposition between logically complementary poles was not restricted to the level of ideas about the work done, for it was underpinned by the structure of the organization itself.

During the period of my fieldwork at the Unit it was undergoing a change in its organizational structure which amplified its anxiety about its identity. When I began my fieldwork, in the spring of 1992, the Unit was run jointly by the local Area Health Authority, through the Clinic, and Council 'A' Education Authority. Council 'A' employed the teachers, then numbering three full-time and one part-time, covered the cost of educational materials, and provided the children's lunches. The Clinic was responsible for the health component, employing the nursery nurses, the psychiatrists, the educational psychologist, the psychotherapists, and the social worker, and providing and maintaining the building. The division within the Unit between health and educational components of its treatment was thus underpinned by an organizational split. Though the staff of the Unit worked closely together they were divided by relationships with quite different employers.

At the time of my arrival at the Unit two factors seemed to threaten its continued existence. A financial crisis, combined with a change of policy, led Council 'A' to withdraw its support for the Unit. At the same time a debate had begun within the Clinic as to whether it should opt out of the control of the Area Health Authority and become a Trust under the provisions of the government's Health Service reforms. A draft proposal for the structure of the Clinic as a Trust was being drawn up and it had become apparent that the large Edwardian house that was used by the Unit would represent one of the Trust's most significant realizable assets. This implied that the building should either be sold to raise money for capital investment in the Clinic, or be kept in use for a purpose commensurate with its high asset value. There was doubt as to whether the Child and Family Department Day Unit could generate enough prestige and fees to justify its use of the building.

The climate of uncertainty and anxiety over the institutional status of the Unit persisted until after the end of my fieldwork in July 1994. It provided a permanent thread of controversy through all of the many staff meetings, and though finally resolved by the Clinic gaining Trust status in the autumn of 1994, the new pressures of survival in the Health Service's 'Internal Market' mean that certainty about the Unit's current institutional status is mitigated by an unpredictable market in which Education Authorities placing children at the Unit now have to pay fees which are nearly ten times what they paid when the health component's cost was being absorbed by the Health Authority.

The changes that took place between 1992 and 1994 had profound implications for the treatment ethos of the Unit as well as its institutional structure and the job security of its staff. As Council 'A' Education Authority gradually withdrew from the Unit the balance of power shifted towards the health component of its treatment. The institutional split between the Clinic and Council 'A' had maintained a balance of power between the Core Team and the Psychiatric Team. This allowed their relationship and the definition of their respective roles to remain to a certain extent uncontested. The withdrawal of Council 'A' put the Core Team on the defensive and exacerbated conflicts over roles. The Psychiatric Team were concerned to defend their role as interpreters of the children's behaviour by defining the teachers' role as primarily educational as opposed to therapeutic. The Core Team, on the other hand, criticized the Psychiatric Team for their lack of practical involvement with the children, believing that the Psychiatric Team's reluctance to spend time with the children outside their own consultations reflected their attitude that the 'educational' component of the treatment should be merely that and that the teachers and nursery nurses should not attempt to interpret the behaviour of the children.

It was in recognition of the extraordinary strains that the Unit was undergoing that the head of the Child and Family Department at the Clinic agreed for some money to be provided for a therapeutic consultation for the staff of the Unit. An outside therapist would visit the Unit to take sessions aimed at working through the difficulties of the relationship between the two teams. This move was a logical extension of the fact that the relationship between the staff groups was regarded as being one of the most significant psychodynamic entities in the life of the Unit, with the weekly meeting of the whole staff, the 'Friday Meeting', being regarded as the arena in which the psychological dynamics arising from the two teams' work with each other, and with the children, could be explored in order that they be better exploited in the service both of the children and the staff. The children's day at the Unit ended after lunch on Fridays in order to make time for this staff meeting, and the consultation took place in this Friday afternoon slot, without the children.

The first session of the consultation was in November 1993. It took place in the large Edwardian house used by the Unit, though the experience of exposing the troubles of the two teams to outside attention was difficult, and different, enough for there to be much discussion about which room in the building would be most appropriate. In the end, for want of any more suitable space, the consultations took place in the hall space where the children had their morning assembly, or 'News', and in which the Friday Meetings, which had become so bitterly contested, took place. Though there was a wish to find an alternative venue to the place of the staff's weekly battles, finally it was acknowledged that the bitterness of the split could not be escaped merely by finding another room and might as well be faced in its usual setting. The session began by everybody introducing him- or herself to the consultant, Roberta – with the exception of Mark, the psychiatric senior registrar, who arrived fifteen minutes late. Roberta said that Mark's lateness might be able to stand for everybody's reluctance to face the problems of the two teams' relationship, a role which he accepted with good humour. Roberta then set the session in motion by asking everybody to go away and draw a picture to represent their view of the Unit. Spread out in rooms throughout the Unit, the staff spent the next twenty minutes drawing pictures on large pieces of paper, then bringing them back to one of the classrooms, where Roberta hung them on the

wall so that they could all be seen together. The novelty of the experience brought the whole staff together, excited comments being made about how well everybody had done to produce such interesting pictures. Roberta looked at the pictures one by one, asking for people to speculate as to who was responsible. At this stage the atmosphere was still good-humoured, the unfamiliar experience of forensic attention normally directed at the children being directed at the staff themselves causing a solidarity of defensiveness of the whole staff group against Roberta. But more negative and searching comments began as the predominant theme of the drawings became clear. What was most striking was how many of the staff had represented themselves in isolation.

Once each of the pictures had been identified the euphoria caused by the novelty of the situation began to subside. There was a general sense of shock that so many of the pictures showed only the person who had drawn them, without any reference to the wider work of the Unit. 'What a difficult job you all have, feeling so alone in your work', said Roberta, and the discussion slowly polarized in response to this observation. Members of the Psychiatric Team said that it was typical that so many of the Core Team had drawn themselves with the children without any reference either to the Psychiatric Team or to the world outside. On the other hand, the Core Team resented the fact that members of the Psychiatric Team had represented the Unit as a whole as a problem. They felt that the Psychiatric Team's detachment allowed them to view the Unit as a problem rooted in the Core Team. The meeting ended with Roberta suggesting that it was a positive step that everybody had been able to acknowledge how much they all suffer from the bad relationship between the two teams, but despite this attempt at a positive gloss the atmosphere was depressed and angry.

The consultations with Roberta continued through into 1994, with sessions arranged approximately twice a term, at irregular intervals because of the need to secure continuing funding from the Clinic. Though the sessions were unanimously felt to be very valuable, it could not really be said that they had a positive effect. What they had in common was that they always served to emphasize and polarize the split between the two teams.

Eventually Roberta suggested that there must be a reason for the staff to cling so tightly to the split; after all, in session after session they had shown more determination to identify and define it than to heal it. She suggested some reasons for the necessity of the split in the Unit. Splitting, she said, was an important concept in psychodynamic theory and work. The child split the mother's breasts between the good and the bad; the one that provides and the one that denies. The children placed at the Unit were tormented by deep splits between the good elements of their parenting and the bad elements, the abuse. Maybe it was the burden of containing such deep splits which drove the children mad, and the reason for the split between the staff teams was that they were relieving the children of the burden of unendurable splits.

Or, from another perspective, it could be thought that the children were projecting into the staff a split between the good, desired parent and the bad parent. The staff would then be, maybe a little too unwittingly, playing out the children's fantasies of warring and destructive parents.

Roberta's suggestions were welcomed by the staff, who agreed that they seemed destined to take on and to live out a deep split. But the question remained, was this a good thing? Were they amplifying the torment of the children by perpetuating the split? Might it not be more important for the children to be presented with a model of unified containing care? Was the staff split, therefore, dangerously failing the children or providing them with the invaluable opportunity of being relieved of an unendurable burden? There seemed to be no answer to this question.

Difficulties were perpetuated rather than resolved.[2] The consultation ended with the recognition that if the split between the two teams serves to relieve the children of the difficulty of living with deep internalized splits, the difficulty has only been removed to another level. The split which relieves a difficulty becomes in turn the new difficulty. Of course it is a fact that many difficulties of the work of the Unit cannot be resolved, only better endured. Every day, situations with the children confronted the staff with conflicting imperatives, imperatives that counselled intervention and non-intervention, often with equal and simultaneous urgency. The difficulty which was so characteristic of life in the Unit was the measure of being torn, or balanced, between the conflicting imperatives, and the difficulty of antagonism between the two staff teams became the paradoxical measure of solidarity in the service of the children.

Conclusion

It was clear to me as I undertook my fieldwork that I was not the only social theorist at the Unit. Indeed, at the same time as I was applying my knowledge of social theory to what I was observing, the staff of the Unit were acting out social theory in the daily business of their work. We were, truly, close intellectual cousins, and the nature of our affinities and our differences was mutually illuminating.

This affinity between social scientists and organizations has deep roots in sociological ideas about social structure. When Mary Douglas describes van Gennep's metaphor of society, 'as a kind of house divided into rooms and corridors, the compartments carefully isolated and the passages between them protected by ceremonial' (Douglas 1975: 55; van Gennep 1960), she might be describing the labyrinthine Edwardian house of the Unit, or indeed the interior of many organizations. It is important that the ethnographer is not blind to this affinity.

2. Similar enduring staff conflicts are described by Melissa Parker in Chapter Seven below.

The case study that I have given in this chapter is of an ethnographic context in which it was very clear that the ethnographer and the subjects of the ethnography had much in common, but it will be equally true in other organizational contexts that awareness of certain basic similarities between the ways of working of the ethnographer and of the organization will enrich the ethnographic process. Seeing others as both expert and 'folk' social theorists allows the ethnographer to be better aware of her or his own position as both specific expert and participant in a much broader tradition of social description.

References

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