

Centralizing Fieldwork

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Fieldwork is a central method of research throughout anthropology, a much-valued, much-vaunted mode of generating information. It is all the more surprising then, that its nature and process have been seriously understudied in several branches of anthropology. One goal of this book is to ameliorate that imbalance.

Social anthropologists are the exception to this history of neglect. Since the mid-1980s they have made critical scrutiny of their practice a legitimate and revealing topic of study. They have inquired, among other themes, into fieldwork relations and rapport; conflicts, hazards and perils in the field; the continuously negotiated identity of the fieldworker; the blurring of private life and research boundaries; the ethics and the erotics of fieldwork; the status and types of reflexive ethnography; the popularization of the discipline via accounts of fieldwork; and so on (see e.g. MacClancy and McDonaugh 1996; Goulet and Miller 2007; McLean and Leibing 2007; Robben and Sluka 2007). We could make the list longer, but the point is already made. Fledgling research students in social anthropology now go into the field better prepared, because much more knowledgeable about the promise, limits and broad consequences of a panoply of research methods.

In marked contrast, biological anthropologists and primatologists have appeared more reluctant to investigate the wider dimensions of their own approaches in the field. They have long been ready to teach their research students the upsides and downsides of highly particular methods, especially quantitative ones, but have seemed very disinclined to discuss, publicly, much beyond. For instance, in the influential research manual for biological anthropologists, 'Practical human biology' (Weiner and Lourie 1981), we have been able to find only one page (p. 392) which even begins to consider the effect of local cultural dimensions on field research. It is as though primatologists and biological anthropologists thought strength, or perhaps just rigour, came from narrow focus of vision. A key aim of this book is to stretch that view and, in the process, help establish a broad debate about fieldwork as central to the development of their disciplines. Part of the brief we gave our biological anthropology and primatology contributors was to take a step back and to endeavour to look critically at their own research practice. We wanted them to investigate how

much of the broader context of fieldwork is relevant to our understanding and assessment of the resulting academic products, and how much (if anything) should be regarded as mere 'corridor gossip'.

The other, key aim of this book is to further the comparative project within a broadly conceived anthropology. Interdisciplinary work in anthropology is usually justified by reference to evolutionary kinship: what we learn from primatology, biological anthropology, and social anthropology could and should illuminate our rounded understanding of the development, since prehistory, of humans and non-human primates. The trouble here is that while some important cross-disciplinary linkages have been made, the attempt can appear all too often as strained and unconvincing. Experienced anthropologists are only too well aware of just how tricky it is to combine the biological and the social in a revealing and academically persuasive manner. Moreover, in a recent review of the status and future of a broad-based anthropology, the social anthropologists Daniel Segal and Sylvia Yanagisako worry that contemporary 'calls for four-field holism and biocultural integration are often ... thinly disguised attacks on those strands of cultural-social anthropology—specifically interpretive and constructivist approaches—most visibly in tension with positivism' (Segal and Yanagisako 2005: 11). They fear that four-field holists wish to reduce all forms of anthropology into a singular, dominant, scientific paradigm (ibid.: 13). However, this argument poorly represents the kinds of questions we seek to explore here. We seek a real and open dialogue across arenas of anthropological inquiry, here focusing on biological and primatological areas. In North America, where the term 'holistic' rears its head most often, a modern four-field (holistic) approach does not imply an acceptance of any singular dominant paradigm or theoretical approach. Rather, this 'big tent' anthropology is concerned with mutual interaction and discourse across sub-disciplinary and theoretical boundaries in effort to maximize the transdisciplinary potential of anthropological inquiry. Let us be clear: we have no truck with any reductionist programme, however veiled. Instead, the contributors to this book suggest a different, potentially more fertile way to engage these disciplines, one centred around methodology, not theory. Thus the questions we ask are, what in the pursuit of fieldwork is common to all three disciplines? What is unique to each? How much is contingent, how much necessary? In other words, can we generate well-grounded interdisciplinary generalizations about this mutual research method, and are there any telling differences?

To this extent, our book may be regarded as a novel, exploratory way to strengthen the promise of the four-fields approach, which has been historically so promoted within sections of American anthropology. As Lederman puts it in her recent defence of this approach, 'Cultivating cross-subfield accents-identifying affinities and openings that make strategic cooperation possible among the subfields—has been, and may continue to be, anthropology's distinctive disciplinary resource' (Lederman 2005: 50). For this reason, and those above, it is an old exercise still very well worth performing. And in this book, by switching the focus from common theory to common method, we hope to breathe new life into this long-established but much contested cross-disciplinary project.

We have chosen to order the contributions in the following sequence, though our apparent logic may seem arbitrary to some. Either way, this précis can serve as a signposting of sorts, for readers who would like a little forewarning, or merely an indication whether it is worth their continuing. We start with a trio of personal but intellectually very revealing essays. In Chapter 2 Geoffrey Harrison, a distinguished biological anthropologist, reviews his fieldwork career, demonstrating just how much research methods and attitudes have evolved, and giving historical depth to our perspectives. Next comes the primatologist Volker Sommer. In a strikingly candid chapter, he reviews the course of his own field researches in order to compare and contrast central dimensions of our three disciplines. In Chapter 4, Robert Sussman takes an autobiographical approach to discuss key but neglected aspects of fieldwork and methods within the contexts of American primatology. Since both Sommer and Sussman underline how fieldwork can easily lead one into the practice of primate conservation, their chapters dovetail neatly with Lee's contribution. For she discusses whether fieldwork can inform approaches to conservation. She wishes to ask whether the outcomes of conservation practice can justify the continued exercise of fieldwork. Of course, there are no easy answers here.

Our three disciplines are all international, taught throughout the world and practised globally. Chapters 6 and 7, both by Japanese primatologists, bear this out. First Yamagiwa portrays the distinctive, interlinked development of primatology and ecological anthropology in his own country. A senior scholar, he has himself experienced much of the history he outlines. Next Kutsukake, who has been educated in both Japanese and Western universities, takes a more distanced stance. He wants to question just how different 'Japanese' and 'Western' primatologies really are. And where they do appear to be mutually distinctive, just what does a 'Japanese primatology' have to offer foreign colleagues?

In Chapter 8 Fuentes notes that biological anthropologists and primatologists go to the field to collect data to improve our overall understanding of being human or being primate. He highlights the often overlooked element that field data are rarely the representative measures we see them as. That is, the data seldom carry the totality of information necessary to fully interpret outcomes. However, even the most skilled field practitioners can inadvertently create substantial problems during field data collection, such as the conflating of measurement with interpretation. This can become a major element in assessing our own fieldwork and its impact.

The following quartet of chapters are by biological anthropologists. Panter-Brick and Eggerman critically analyse their cross-disciplinary fieldwork in the Gambia and Afghanistan. What they come to advocate is an openended attitude to research. Researchers, they argue, need to engage closely with local communities if they are to collect sufficient data and evaluate them meaningfully. In her chapter Rosetta, who has worked in Senegal, France and Bangladesh, illuminates the various pitfalls to avoid when studying biological variables in human populations. Experience leads her to stress the necessity for discretion, tact, confidentiality, respect and a culturally sensitive choice of the most appropriate method of collecting data. Hladik, in Chapter 11, compares the quantitative measurement of food consumption in both primates

and humans. Since this kind of research, with humans, could be highly intrusive, Hladik has to emphasize the need for the academics to empathize with the locals and to establish a meaningful consensus with them about the research and its value. For the researchers knew that to do work of any value, locals had to be deeply involved in the project and sufficiently relaxed at measurement times. Froment extends this discussion further by posing the tricky questions which arise when researchers wish to take blood samples, and to conserve them.

After a chapter in which MacKinnon discusses the various productive roles played by primatology field schools, we close with a trio of contributions investigating the ways fieldwork is portrayed in print. Jolly evaluates the pros and cons of a broad, narrative approach to her lifelong fieldsite, compared to the focussed, scientific style she usually employs as a primatologist. Asquith follows. She uses a survey of narrative primatology to argue that her discipline can productively encompass both ethnological and evolutionary perspectives. MacClancy, a social anthropologist, ends the book with an analysis of key themes within popular accounts of fieldwork by primatologists and biological anthropologists.

So much for the individual contributions. In this introductory discussion, we wish to pull out and scrutinize key themes which emerge across the chapters. For expository convenience, we have divided the following into a series of discrete but interlocking sections: an initial historical perspective; international differences in fieldwork; fieldwork as a process and in teams; autobiographical approaches; ethics; studying one group or several; popularizing fieldwork.

A tour around the history of fieldwork

Social anthropologists speak of Bronislaw Malinowski as the key, interwar figure who made fieldwork essential for initiates into their discipline. A Polish expatriate based in London, he was not the first fieldworker, but he was its greatest propagandist, propounding to both academic and popular audiences the value of intensive, long-term research. It was his immediate predecessors who had given up relying on reports sent in from those living in the colonies and made the break into the field. But, unlike them, Malinowski did not stay 'on the verandah', interviewing locals who were brought up to him. Instead he argued social anthropologists should pitch their tents right inside villages, learn the locals' language and endeavour to live as much like them as possible. This is the famous, but impossible concept of 'participant-observation'. It is a magnificent but unachievable ideal, for how can one both participate and observe simultaneously?

In recent years many social anthropologists have felt their subject increasingly under threat, as it has been absorbed into a diversity of other disciplines (most notoriously, cultural studies). For some, social anthropology appears to be losing a sense of its theoretical specificity (e.g. Kapferer in Hirsch et al. 2007: 117). By default, fieldwork has become the most distinctive aspect of a contemporary social anthropology. In this sense, fieldwork is now even more important, more central to the discipline than it was in Malinowski's time. His conception of intensive fieldwork

in one area for an extended period of time, usually at least one year, dovetailed well with an idea of sedentary populations with clearly identifiable cultural boundaries. But in recent years, with the increased movements of peoples around the world, some anthropologists have come to laud the value of multi-sited fieldwork, i.e. following the group studied wherever they go. It is felt that this, potentially much more demanding style of fieldwork is more appropriate if studying forced displacement, labour migration, transnational corporations or other forms of mobile populations.

At the same time it is no longer possible to think of a 'culture' in unproblematic terms. Too many people contest the way they are represented by others, or even by members of their own group. Here fieldworkers can choose to make this contest of representation the very focus of their study. Others may choose to research the social life of a thing (say, a carpet, a foodstuff or a piece of art), serially investigating the ways it is used and understood in each of the arenas or cultures it is made to pass through. The sum result of these contemporary changes is that fieldwork remains crucial within social anthropology and has richly diversified in its forms.

The history of field methods within biological anthropology has yet to be written. We can only sketch a segment of it here, greatly assisted by an interview generously granted by Geoffrey Harrison to MacClancy (conducted 6 xi 2007), backed up by his own contribution about different team research trips he was himself involved over the course of his career. The Emeritus Professor of Biological Anthropology at Oxford University sees its postwar development in both Britain and the USA in three overlapping phases. First, the 'descriptive phase', which continued up to the 1950s, where biological anthropologists, usually acting alone, measured stature and blood groups in order to document the variety between and within human populations. As more diagnostic characters were discovered, biological anthropologists formed teams to measure these different dimensions. To begin with, only a small minority fieldworked for more than a few weeks; most of the work was later done in the lab; any subsequent comparison was made within a broad evolutionary framework.

Harrison characterises the second phase as the rise, from the 1950s on, of 'cause and effect approaches'. Here biological anthropologists looked at either the cause or effect of one dimension of variety on human populations. This kind of work necessitated much more fieldwork than descriptive work. For instance one of his students, Melissa Parker, spent two years in the Sudan, investigating the effect of schistosomiasis on the economic productivity of women who worked in the cotton-fields. By day she measured their activity; by night she examined their stool samples. A much longer study was that by Paul Baker and his team, at Pennsylvania State University, who over a ten-year period investigated the effects of altitude on human health, in the Peruvian Highlands (Lasker 1999).

The third phase identified by Harrison is research into 'how populations work'; what is their structure? What effects do they have on the environment, and vice versa? This kind of fieldwork can be so lengthy that it may outlast the research-life of a single academic. The work of Harrison's own team into the population surrounding Otmoor, just outside Oxford, took fifteen years. A further, excellent example here is the work of Neville White in Arnhem Land, northern Australia, which he started

as a young man and on which he is still engaged, though now in his sixties. Over that time White has documented the health and wellbeing, in physical terms, of the locals; he has also studied their social organization and the natural environment in which they live as well. In other words, he has been investigating, along multiple dimensions, almost the whole ecology of the area for the last thirty to forty years.

How field methods in French and German biological anthropology developed has yet to be investigated. But the main direction of development within Anglo-American biological anthropology at least is clear: since the Second World War, the main shift has been from primarily laboratory-centred work to field studies, usually of ever longer duration (see e.g. Lasker 1993: 2). Within biological anthropology, 'the field' is now more important than ever.

Primatological fieldwork, often considered a subset of biological anthropology fieldwork in North America, but frequently considered more allied with the biological sciences in Europe, shares a bit more with some of the trends in social anthropology than those in biological anthropology. The primatologist traditionally goes to live with his or her subjects, pitching camp where the monkeys, apes or lemurs are, watching them day in and day out in an attempt to understand the inner workings and general patterns of the particular primate 'society' of interest. While not being able to question their informants as Malinowski and his social anthropology descendants preferred, primatologists seek descriptive answers in the behaviour and inter-individual relationships observable in their primate subjects. Primatological fieldwork lies, in a sense, in an interstitial zone between the ethnography and the bio-assay.

The American physical anthropologist Sherwood Washburn's petition for a new physical anthropology (1951) sounded the initial clarion call for fieldwork in primatology. Washburn's call for a truly biological anthropology advocated multidisciplinary and interdisciplinary approaches to the understanding of human behaviour, biology, and history. One of the cornerstones of this approach was comparative studies of free ranging nonhuman primates. He urged researchers to travel to the locales where these primates lived, to live among them, to record their behaviour, and to initiate understandings of their societies. Washburn's early students became some of the very first primatology fieldworkers and pioneers in the field of North American primatology (e.g. Phyllis (Jay) Dolhinow and Irving DeVore). More than 60 per cent of the North American primatology Ph.D.s produced since the 1950s were by students of Washburn or of Washburn's early students (Kelly and Sussman 2007).

The paleoanthropologist Louis Leakey exploited this call and established the most popular and influential cadre of field primatologists, the trio of Jane Goodall, Biruté Galdikas and Dian Fossey. Interestingly, unlike Washburn's students, two of the three of these most famous of the early primatologists (and probably those most responsible for the primatology explosion in the 1970–80s) were not trained anthropologists. However, Washburn's students and Leakey's 'angels' all practised the anthropological tradition of long-term (one year or more) coexistence with the population of study, often under trying circumstances and with great challenges and

difficulties. Beginning in the the 1960s and extending to the modern day, the extended fieldwork trip remains the hallmark of a majority of primatology dissertations (see Fuentes, Jolly, Sommer and Sussman chapters).

Washburn eventually expanded his call to that for a broader biosocial anthropology '... human biology has no meaning without society. For a particular problem in the short run, either biological or social facts may be stressed, but the evolution of man can only be understood as a biosocial problem' (Washburn 1968). This coincided with a linking of biological anthropological field practice with aspects of field practice from primatological and ethnographic approaches, resulting in the relatively recent emergent fieldwork in biocultural anthropology (Dafour 2006; see also Fuentes, Froment and Rosetta chapters).

National styles of fieldwork

A first glance at social anthropology suggests that Malinowski has won. It seems that his promotion of intensive fieldwork has become the dominant model throughout the discipline, worldwide. A slightly finer look, however, suggests that there are at least some international differences. Malinowski's disciples have not triumphed everywhere. According to the Japanese anthropologist Takami Kuwayama, his style of fieldwork should be regarded as only a model, not a norm (Kuwayama 2006: 50). This variety of styles is as uncharted as it is important. As the American social anthropologists Akhil Gupta and James Ferguson point out, 'there might be much to learn from comparing the different kinds of knowledge that such different practices and conventions open up... It seems clear that, in spite of homogenizing tendencies rooted in colonial and neo-colonial histories, practices of 'the field' are indeed significantly different away from the hegemonic centers of intellectual production' (Gupta and Ferguson 1997: 27–8). All that we attempt here is to broach the topic, to move beyond self-limiting Anglo-American perspectives.

The strongest example comes from Brazil. In a much-quoted article, Alcida Ramos states that fieldwork Malinowski-style tends to produce much fine detail and profound analysis, but with little historical depth. Synchrony is emphasized over diachrony. In contrast Brazilian ethnographers, for a variety of practical reasons, rarely practise longstay fieldwork, but make short trips to Indian areas, usually in the summer months (Ramos 1990). One consequence is that few of them learn to speak the local language well. However, since most of them study some aspect of interethnic relations, that lack is not as big a handicap as it might first appear. Moreover Brazilian ethnographers, unlike their Malinowskian counterparts, make many repeated trips to the same group, over the course of decades. They can thus maintain long-term relations with those they study, gradually building up a profile of them, and tracking their chronological evolution. Ramos likens this difference in fieldwork styles to that between a richly textured still photograph and a film, which focuses on movement over permanence (Ramos 1990: 459).

Anthropologists who practise Malinowski's style are often criticized by inhabitants of the country where they study as 'parachutists': they drop into an area, get their

information, and then get out. Some do not go back, and certainly not for repeated visits. But Brazilian ethnographers, who keep on returning to their fieldsites, never really cut off their ties with the locals they work with. These enduring relationships mean that Brazilian anthropologists are almost all actively involved in the struggles of the indigenes with whom they collaborate. This commitment is part and parcel of what they do. As Ramos puts it, 'There is no purely academic research; what there is is the rhetorical possibility and personal inclination to exclude from one's written works the interactive political, moral, or ethical aspects of fieldwork' (Ramos 1990: 454).

Japanese folklorists work in a similar manner. For them, each stay does not exceed a few months, but they visit repeatedly, often for a few decades or even more. In the process they become committed to the welfare of those they study, for example engaged in political activity to improve locals' living standards or informing them about the government's cultural policies and their consequences (Kuwayama 2006). The work of Basque folklorists could be pencilled in here, as the long term studies of these committed amateurs is usually framed within a broad nationalist project. By getting into print the diversities of Basque customary ways they help give substance to a sense of Basque distinctiveness (see, e.g. Azcona 1984).

Midcentury Paris provides our next example. From the 1930s onwards Marcel Griaule propounded the importance of repeated visits, of a few months at a time. But for him the value of going back to his Dogon fieldsite time after time did not lie in a deepening of political commitment or the gaining of a chronological perspective. Instead he saw these return trips as the key way to get an ever more profound insight to indigenous modes of thought; they were the means by which one could serially expose cultural secrets of increasing importance. As the locals came to acknowledge the seriousness of his endeavour, they began, over the years, to reveal more and more of the principles underlying their style of thought (Clifford 1983). Even though a recent re-study of the Dogon has queried the status of material he obtained (van Beek 1991, but see also Calame-Griaule 1991; de Heusch 1991), no one has doubted the ethnographic worth he placed on repeated visits.

Since social anthropology has come 'home' in recent decades, with an increasing number fieldworking within their own country, going back again and again to one's fieldsite, even if only for months at a time, appears to be coming less unusual. Indeed, for many, it is now more and more considered model practice.

In primatological fieldwork practice some similar differences emerge in national schools: most notably between the Japanese and North American schools, and to a lesser extent between the North American and European schools (see Fuentes, Katsukake, Sommer and Yamagiwa chapters). These differences are in part attributable to the home disciplines from which primatologists emerge. In Japan, Kinji Imanishi's hybrid of ethnographic, ethological and ecological approaches contextualized in a particularly complex and holistic view of nature (Imanishi 1941), which influenced methods and orientations. His perspective and training of students laid the groundwork for a long tradition of intensive field observations, a comfort with provisioning as a form of manipulation, and a focus on group social relations as core to understanding primate societies. Members of the North American school, heavily influenced by

Washburn, focussed much of their gaze on the testing of adaptive scenarios and the construction of evolutionary models based on observations and assumptions about the causes of individual primate's behaviour. Finally, the European primatologists were simultaneously influenced initially by the ethologists Lorenz and Tinbergen, and subsequently by ecological approaches and precedence of standardized models for assessing behaviour. Additionally, most North American primatologists were trained in departments of anthropology in United States universities whereas most European primatologists initially emerged from the few major biology or ethology departments in the United Kingdom, Germany, Holland and France.

While major reflection and introspection on practice and impact has not been overly characteristic of primatology in general, since the late 1980s much attention has been focussed on the role of gender in both theory and practice in primatology (Strum and Fedigan 1999). Additionally, the past decade has seen a significantly increased role for reflection amongst primatologists in regard to issues of conservation, engagement with local human populations and the patterns of interaction between humans and nonhuman primates as serious areas for research focus and critical analyses (Fuentes and Sommer chapters).

The social anthropologist Rena Lederman has suggested there may be regional styles in fieldwork. That no matter where an anthropologist comes from, he or she joins a regionally defined community of ethnographers. And each community may have distinct background expectations about scholarly practice, and the conduct of fieldwork, 'e.g. expectations about language competence, conditions for obtaining research permissions..., the duration of fieldwork, and kinds of incorporation into field settings (Lederman 2007: 317). Anecdotal evidence from primatologists suggests that something similar may well be occurring in their own discipline. But, as yet, there has been no systematic study of the phenomenon.

Fieldwork as process, as ethical practice, as training

Fieldwork takes time, lots of it. It is not a state but an open ended process, which evolves and develops on site in a manner often independent of any research agenda however painstakingly formulated back in the fieldworker's university. This is an integral part of the messy, somewhat uncontrolled business that is fieldwork, and one reason why it demands resourcefulness from fieldworkers, who must learn to be adaptable or else choose another profession. As Professor Rodney Needham, MacClancy's supervisor at Oxford, said to him as he left for his first research trip, 'Don't worry. The locals will set the pace.' Too true, but they set the agenda as well. Intending to study indigenous cosmology and ritual, MacClancy arrived in a Pacific archipelago (Vanuatu) undergoing the classical throes of an anti-colonialist struggle for independence. Instead of residing there for one year, he stayed for more than three, and finally returned to Oxford with the material for a doctorate on the cultural dimensions of Melanesian nationalism. Fuentes' supervisor at the University of California, Berkeley, Professor Phyllis Dolhinow, warned him of a similar pitfall in fieldwork practice: falling into the trap of 'I would not have seen it had I not believed

it' and reaffirming the consequent. The fieldworker goes into the field well versed in the theory and explicandum related to the topic of focus. But fieldworkers are generally not at all prepared for the fact that their subjects are not indoctrinated into specific academic paradigms or explanatory processes, and thus frequently behave and think in ways that are not best explained via the toolkit the researcher brought to the field. Fuentes went to the field to test a series of hypotheses on the social organization of a monogamous primate. He returned to produce a dissertation and a series of publications demonstrating that few if any primates are monogamous and that most hypotheses proposed for monogamy fail to explain the few primate cases.

The messy, uncontrollable nature of fieldwork forces anthropologists to be resourceful, adaptable. In this way fieldwork is an essentially creative activity, one much more of practice than of prescription. During MacClancy's extended fieldwork on the urban culture of Basque nationalism, a continual task was identifying pertinent phenomena. There had been no earlier work by others in the area, so he had to follow up potential leads and see where they took him. This was a question of recognizing the relevant while repeatedly evaluating processes which unfolded in front of his eyes. Thus at the beginning of his fieldwork he could not have foreseen the contents of his eventual ethnography (MacClancy 2007). Because of this irreducibly creative dimension to our research practice, Judith Okely chooses to call anthropology 'an artistic engagement, not scientific mimicry' (Okely 2007: 359).

Harrison in his chapter, and Panter-Brick and Eggerman in theirs, emphasize this processual aspect of fieldwork in their contributions, even though they come to differing conclusions. Harrison, based on his own experiences, warns against changing the research agenda while in the field. Panter-Brick and Eggerman recommend constant reconsideration of it while fieldworking. They emphasize how they and their team had to learn how to navigate conflicting and contradictory information, how to contextualize the information collected and, perhaps above all, how to assign value to the unexpected. For serendipity is a crucial strength of any fieldwork: the ability to remain open to witnessing an unforeseen event, to work out ways of understanding it and then, if necessary, to adjust accordingly one's comprehension of just what precisely is going on in the fieldsite.

Undergirding the research process there has to be a strong sense of ethics and of good practice. As with all moral debate in a cross-cultural context, there are no easy answers here, no matter how facile the questions may be to pose. The ethics committees of professional anthropology associations may draw up well minded codes of practice for practitioners to implement, but seasoned fieldworkers know all too well that moral judgement in the field is less a simple minded application of ethical verities than a constant, evolving negotiation of responsibility with all those involved in the research enterprise. Researchers do not adjudicate *a priori* precepts; rather they learn to negotiate truths (Meskell and Pels 2005). Froment, Lee, Rosetta and Sommer, in a diversity of different ways, bring up these ethical concerns in their contributions, and demonstrate how easily they can arise in a variety of contexts. Froment discusses some of the key quandaries biological anthropologists can face today: how to negotiate the local perceptions of and the likely reality of benefits

for the peoples studied? Under what circumstances, for how long, and for what end may researchers handle human remains and fluids? How can one attempt to balance individual, community and broader rights? One contemporary response is the 'partnership' approach advocated, among others, by the American biological anthropologist Lawrence Schell, who openly admits his intellectual debt to the postmodern debate within social anthropology about reflexivity. He contends that the populations to be studied need to be involved in the research, design and process, at all stages from the beginning. Even though this partnership style of research is more time consuming, the results can be much richer and more valuable for all concerned (Schell et al. 2005, 2007).

Lee's key question in her contribution is central to us all: why do fieldwork? Given the disruption, however mild, it may cause, she asks how we can justify its practice. She examines fieldwork in terms of outcomes, specifically those associated with conservation practices, and so wishes to enquire who, exactly, benefits from fieldwork: the locals, the local habitat, or a local species of concern to conservationists? The crucial point here is how can we ever assess who benefits, and to what extent? Since it is not always obvious that those who identify problems ought to be the people who produce the solutions, Lee argues that the role of anthropological fieldworkers should be to seek solutions which are emergent rather than imposed. Sommer extends the debate in a related direction by contending that conservation has aesthetic as much as ecological consequences. If we make all other primates extinct, the world will become 'poorer, less beautiful, and thus less good'. But why bother to save them, given the crushing weight of forces ranged against their survival? Taking his cue from Camus, Sommer propounds a transcendental heroism: to not give in, to revolt, and not care about defeat or victory.

Fieldwork is processual, but so is the development of any anthropologist's own academic career. Several contributors take an autobiographical approach in their respective chapters (Jolly, MacKinnon, Sussman, and especially Sommer in a disarming and engagingly frank contribution), and so underline the constant interaction between their teaching and their research practice. Besides its other benefits, the personal focus of these contributions emphasizes the longitudinal, dynamic nature of any long term field research.

MacKinnon, in her chapter, focuses on one way to begin this process: primatology field schools. These schools, which are increasingly recognized as fundamental to the initial training of primatologists, fulfil a host of functions. They are opportunities for students to experience another culture, and so reflect back on their own, and their place within it. They can demonstrate just how intertwined issues in social and biological anthropologies are. Also, they may serve to show budding conservationists the agonizing gap between the way things should be and the way they are. In all, as MacKinnon puts it, these schools prepare their graduates well for 'the challenges and epiphanies' which only fieldwork can produce.

Perhaps the most famed of social anthropology field schools is that established in mid-century Mexico by the Spanish exile Angel Palerm. He saw anthropology both as a critical mode of inquiry and as a means to realize the aspirations of the

Mexican Revolution. For him, fieldwork was a way to test anthropological ideas against people's lived reality, and to disturb one's prejudices and bogus certainties. Since he wanted students to integrate theory and practice as early as possible in their anthropological careers, he set up a fieldwork school, in Tepetlaoztoc, near Texcoco. There, mentored by he and his staff and assisted by illustrious north American colleagues, undergraduate anthropologists, even those in only their first year of study, could work on real problems during the day and discuss them with their professors in the evening. In the process Palerm hoped to develop a style of anthropology more attuned to Mexican realities (Wolf 1981; Garcia 2001; Icazuriaga 2005). The school exists to this day.

The fieldworker alone?

Malinowski liked to promote the romantic idea of the ethnographer as an intellectual alone in the bush. Susan Sontag later turned that into the flattering image of 'the anthropologist as hero', a pretentious trope waiting to be punctured (Sontag 1970; MacClancy 1996: 28). Though the practice of fieldwork as a solitary activity is still presented as the dominant style within social anthropology, there has long been competing alternatives. As Gottlieb put it:

There are strong signs of a macho ethos that pervades the intellectual orientations of many anthropologists, both male and female.... The Marlboro Man-like impulse to celebrate individual achievement rather than collective collaboration may be one component in this tendency ...

And yet, we are all aware that anthropologists often work in pairs-with a spouse or other domestic partner-and often, as well, in teams of researchers (Gottlieb 1995: 22. See also Foster et al. 1979).

The famous Torres Strait Expedition of 1898 was a very early example of team research, composed in this case of a linguist (Ray), an anthropologist (Haddon), an experimental psychologist (Rivers) and two of his students, plus a medical pathologist (Seligman). Ironically, even though the work of this expedition turned Rivers and Seligman into renowned anthropologists, it seems to have been the sole significant piece of team fieldwork in early British anthropology. And in 1912 Rivers himself came out against teamwork as too disruptive of local life. The only noteworthy exception in subsequent decades was the teamwork method developed during the Second World War by Max Gluckman. Based in Northern Rhodesia, he created groups of white social scientists and African assistants as an effective (and probably the only) way to study the multi-ethnic composition of the Copperbelt (Schumaker 2001; Kuklick 2008: 65, 77-8).

In interwar Paris Marcel Mauss, the leading French anthropologist of his day, told his students that teamwork was the desideratum. In his *Manual of Ethnography*, he argued that the ideal expedition would be composed of a geologist and a botanist,

as well as several ethnographers (Mauss 2007). Griaule, especially in the first decades of his Dogon work, also trumpeted the advantages of teamwork. Social reality, he argued, was too complex for any one anthropologist to capture. What was needed was a group of specialists who divided the ethnographic tasks to hand between them. Also, a team, using pen, paper, still and movie cameras, could simultaneously record different aspects of a large, fast moving ritual, though it is hard not to believe the presence of six observers did not change the nature of the ceremony for its participants. Furthermore, members of a team could ask different locals similar questions, in order to check data, account for individual bias, and fill in gaps (Clifford 1983). In this way, his team's short stays could be as intense as, if not more intense than, the prolonged residence of a Malinowskian fieldworker. Though his ideal of teamwork did not become extensively popular, at least one of his followers, Germaine Dieterlin (1903-99), maintained this fieldwork style even into her nineties (Parkin 2005: 204-5). And while Mauss's younger colleague Paul Rivet saw the potential of teamwork to hold a broad based anthropology together, very few in fact practised this collaborative style (Rivet, Lester, Rivière 1935: 516; Blanckaert 1999: 37).

In Japan Kuwayama has similarly praised the benefits of teamwork, among local folklorists. Though different team members play different roles, they usually work together throughout the whole fieldwork period and frequently share information, so they can correct each other's apparent misunderstandings and oversights. By listening to members' opinions, a team may transcend individual subjectivity, by forging a collective inter-subjectivity. In this way, Kuwayama contends, postmodern concerns about fieldworkers' generation of 'partial truths' (Clifford 1986) are, at least, partially overcome (Kuwayama 2006).

Teamwork comes, of course, with its own set of problems. Members of a team, subject to the typical stresses of fieldwork, may turn out to not cooperate in the ways usually expected; expectations may be different; tolerance can fracture as unforeseen difficulties arise. Selecting members of a team becomes a delicate but central task. The American biological anthropologist J.V. Neel said he chose 'people who had been Boy Scouts, or looked as though they had been' (G. Harrison pers. comm.) Evon Vogt, Director of the Harvard Chiapas Project, confessed his team had suffered 'a few celebrated intra-project conflicts' (Vogt 1979: 294). The veteran anthropologist of Papua New Guinea, Mervyn Meggitt, gently understated the challenges of long term teamwork:

A real problem exists in devising efficient and ethically acceptable procedures for fitting additional investigators into an established or even a nascent program. Such an arrangement must demand delicate handling to ensure that the effectiveness and productivity of the inquiry are not impaired, while at the same time the individual egos (and some anthropologists appear to cherish remarkably tender psyches) remain unbruised and the actors' perceptions of their own integrity are not rudely contradicted... Equity and humility must prevail if the undertaking is not to dissolve into dissension and back biting (Meggitt 1979: 115. See also Lee 1979: 308-9; Scudder and Colson 1979: 234).

Problems can multiply if the team is interdisciplinary. Each member may individually carry out exemplary fieldwork, but putting the results together to generate further insights can prove tricky. Thus the biological anthropologist N.G. Norgan, in an overview of a large landmark study into human adaptability in Papua New Guinea, noted that while the project fulfilled its basic objectives, it was less successful at integrating information from different disciplines (Norgan 1997: 106). The distinguished French practitioner Igor de Garine is candid that collaboration between nutritional and cultural anthropologists 'is not easy to manage':

Biologists... are not usually trained to establish human contacts. Social anthropologists tend to investigate... factors which are difficult to quantify. 'Hard' scientists are dubious about the scientific validity of the work of 'soft' social scientists, who in turn do not consider their colleagues to be very subtle and object to the intrusiveness of their techniques upon the populations under study (de Garine 2004: 18).

He feels forced to exclaim when mentioning that a team, of which he was a member, 'managed to collaborate for over fifteen years, often working together in difficult environments on fourteen different Cameroonian tribes – and these researchers still talk to one another!' (de Garine 2004: 18. See also Schumaker 2004: 119). Problems may continue once home as, he notes, social scientists tend to take longer than biologists on publishing their data. 'The latter, in order to comply with the publishing style of their specialities, are eager to publish their results very rapidly before others publish similar results' (de Garine 2004: 23). Fledgling team-researcher beware!

Problems are not just internal to the team. Rosetta argues that the quality of the data collected in any project in biological anthropology directly depends on the rapport created by team members with the peoples studied. It is therefore crucial to train participating research students and local assistants well and with sensitivity. Panther-Brick and Eggerman echo this point: the cooperation and engagement of the people being studied is fundamental to research. That does not mean it is usually easily achieved, or maintained.

Starting with biological anthropology in the 1960s and becoming commonplace in primatology by the 1990s, team-based research projects are now in the majority in each of these disciplines. In part due to the radical increase in toolkits (genetic collection and assessment, physiological measurements and assessment, GPS, computer based behavioural analyses, etc.) and the expertise required to utilize and oversee them, one simply cannot do biological anthropological and primatological field work as the lone fieldworker any longer. Both of these areas have also undergone radical expansion in connectivity with affiliated fields such that teams often consist of biological anthropologists and/or primatologists, ecologists, geneticists, student assistants, social anthropologists, and even economists, psychologists, geographers, or botanists.

Anthropologists without anthropology, but just as rigorous

It is all too easy to think of anthropology as practised exclusively, or to any significant extent, by professional anthropologists who were trained in the subject at university and have gained research degrees in it. But even a quick glance at our history reveals that there was a well grounded anthropology before its establishment in centres of higher education, and that educated non-professionals based in the field continued to contribute very productively to the subject even once departments had been set up in universities. The unlikeable truth appears to be that professional anthropologists downplayed the role of their field-based predecessors and contemporaries, in order to boost their already elevated status, to make their own work seem that much more distinctive, and of course to justify their salaries.

Rosalie Wax, in a historical sketch of 'proto-anthropology', has argued that the first Europeans 'to collect and record genuinely useful ethnographic data about alien people were missionaries of the Catholic church and certain ambitious and imaginative merchants', whose exemplar is Marco Polo (Wax 1971: 23). In other words, field ethnography is primarily a product of the thirteenth century. This practice never quite died out, and revived spectacularly in the late nineteenth century, when colonialist expansion stimulated a dramatic renaissance of amateur ethnographic work, some of which remain 'unsurpassable as field descriptions' (Ellen and Harris 2000; Wax 1971). Throughout the British colonies, educated expatriates acted as informants to home-based scholars and published ethnographies, e.g., the long-serving missionary in Fiji Lorimer Fison; the geologist A.W. Howitt who worked with Australian aboriginals; the South Pacific planter, Ewan Corlette, who did ethnographic work among his labourers; the colonial officer E.H. Man, based in the Andaman Islands, whose ethnography was notoriously exploited by the renowned theoretician of structural-functionalism, Arthur Radcliffe-Brown. Brown's own ethnography of the area is generally regarded as inferior to Man's (Corlette 1935; Needham 1974; Tomas 1991: 98; Kuklick 2008: 57). In some British colonies, such as the Sudan, administrators were trained in ethnography and encouraged to submit ethnographically rich reports, some of which, as Evans-Pritchard acknowledged, contained 'interesting, often shrewd observations' (Evans-Pritchard 1940: 1; Johnson 2007).

In the second half of the nineteenth century and subsequent decades, French military administrators were charged to produce tribal *coutumiers*, learned, often highly dense accounts of customary law grounded on native exegesis and sustained fieldwork (Salemink 1991; Parkes 2008). By the early twentieth century, in France there was such a substantial body of colonial administrators who wrote ethnography that 'they created a small army of researchers, and a force that led to epistemological change in the social sciences'. Indeed because so many of Mauss's students were killed in the First World War, these administrator-cum-ethnographers 'were virtually the only persons available to do research at the very time that field research' was defined as central to anthropology. The first methodological essay on field research, published 1925, paid homage to a colonial officer-turned-Africanist, Maurice Delafosse, as much as to Mauss (Sibeud 2008: 101, 106–7. On Delafosse, see Amselle and Sibeud

1998). In Holland, the first prominent anthropologists of Indonesia (ex Dutch East Indies) were both colonial medical officers, A.W. Nieuwenhuis and J.H.G. Kohlbrugge (de Wolf 1999: 3123). Of course, a notable number of missionaries from throughout Western Europe were also important ethnographers, as were the odd planter and adventurer-explorer (Cinnamon 2007; Harries 2007; Hunt 2007). And, as some were well aware, long-term colonial residents often came to know the locals better than bookish visitors who only did fieldwork for a year or so (L'Estoile 2007: 106–107).

These proto-anthropologists did not work alone. Some French and German administrators collaborated with African colleagues, e.g., in French West Africa local schoolmaster-ethnographers, and from German colonies African teaching assistants based in Berlin and Hamburg (Jezequel 2007; Pugach 2007). The same applied further afield. As an expansionist Japan began to colonize its neighbours from the late-nineteenth century on, its government made use of a striking diversity of its expatriates and colonized intellectuals for the production of ethnography: army officers, explorers, formerly imprisoned Marxists, policemen, colonial administrators, linguists, merchants, naval commanders, writers, railroad employees, native scholars untrained in anthropology (Bremen and Shimizu 1998: 6; Nakao 1998; Shimizu 1998: 123, 138; Walraven 1998: 221–3). Perhaps the most surprising example of this anthropology before anthropologists is the 'ethnographic populists' of late nineteenth century Russia. Exiled to Siberia for their activities, politically committed students exploited the opportunity to do fieldwork in local communities there. Their aim was twofold: to contribute to debates on peasant socialism by generating ethnographic material, and to raise local consciousness by taking it down a revolutionary path (Grant 1999; Ssorin-Chaikov 2008).

The first generally acknowledged field primatologist was Clarence Ray Carpenter, who in the 1930s passed several months observing howler monkeys on a Panamanian island. Yet he remained the central pioneer in this work until the 1950s. But non-professionals had already done primate fieldwork before that time. Sussman notes that in the 1890s R.L. Garner, an amateur zoologist, while on an ape-collecting trip, spent '112 days in a cage waiting for gorillas and chimpanzees to wander by' (Sussman 2000: 88). In the 1940–50s the distinguished entomologist A.J. Haddow carried out epidemiological work in East Africa into the interrelations of yellow fever and local primates, especially bushbabies, which led him to carry out sustained field research on them (e.g., Haddow, Dick, Lumsden and Smithburn 1951; Haddow and Ellice 1964). Perhaps the best example, however, of excellent field research in primatology before the subject was established is Eugene Marais, a South African lawyer who carried out sustained observation of baboons in the Outback (Marais 1939). But the intellectual consequence of his innovative work was limited as his major work was not published until decades after his death (Marais 1969).

The chronological process of research development has been similar in biological anthropology. For example, Huss-Ashmore and Ulijaszek, in a review of field research in the Gambia, note that 'It is one of the ironies of human adaptability research in Africa that some of the most interesting work has been carried out by

investigators whose primary concern was not in human adaptability' (Huss-Ashmore and Ulijaszek 1997: 82). In other words, excellent field research on an eminently biological anthropological theme was conducted in the Gambia by nutritionists, ecologists, economists, as well as medical and agricultural researchers. There was not an anthropologist among them.

The historical record, as sketched above, suggests that in almost every country where universities created departments of anthropology non-professionals, or the self-trained, produced valuable, at times excellent work in both social anthropology and in primatology, while some of the best work in biological anthropology was carried out by academics from other disciplines. What does this say about us?

If we are to recognize the intellectual contribution made by expatriates, we should at the same time acknowledge that made by locals. Thus, social anthropologists no longer talk of gaining data from 'informants' while in the field, but of collaborating with local 'co-workers'. They now accept that some classic tomes of ethnography were as much produced by their co-worker(s) as by the distinguished anthropologist. It is argued, for instance, that several of Boas's works on the people of the Northwest American Pacific Coast should be authored as 'By Franz Boas and George Hunt', his long-serving local research assistant. A further example: Richard and Sally Price in 2003 re-examined the field diaries Melville and Francis Herskovits kept during their famous 1920s expedition to the Saramkas of Suriname. The Prices note that throughout their time in the field, the Herskovits were accompanied by non-Saramakas, paid for both menial labour and information about the locals, yet their contribution and that of the Herskovits' main Saramaka co-worker are, 'to a startling extent, simply "disappeared" in the publications' (Price and Price 2003: 42). Sanjek provides a depressingly long list of other examples, from around the world, of local fieldworkers who collaborated with famous professional anthropologists, and whose important contribution has still to be properly recognized (Sanjek 1993).

In a similar vein, contemporary primatologists are today much more openly aware of the contribution made by locals who maintain their research sites during their absence. For example, since the 1960s and 70s primary investigators (American, European or Japanese) at many of the best known long term primate research projects in Africa and Asia have had to leave the sites for political, economic and other reasons for varying periods of time. At such prominent sites as Jane Goodall's chimpanzee study at the Gombe Stream Reserve in Tanzania and Biruté Galdikas' orang-utan study at Tanjung Puting in Kalimantan, Indonesia, the projects relied almost exclusively on local trackers and field assistants for their upkeep and continued data collection for significant amounts of time when the foreign researchers were absent from the sites (see also Jolly chapter). The presence and actions of these local participants in the projects enriched the research via their particular insights and perspectives. This core local involvement provided opportunities for researchers to view their primate subjects and the research questions through the eyes of those who lacked the professional training but lived through long term exposure and cultural contexts in which primates were central or at least permanent fixtures of the local ecologies and sometimes mythologies (e.g., Fuentes and Wolf 2002). Hans Kummer

makes a comparable point in the closing pages to his popular 'In quest of the sacred baboon'. There he argues that 'the crucial aspect of the scientific approach' is 'to strive for a differentiated view, free from contradictions, one that does justice to the object observed and corrects our prejudices' (Kummer 1995: 308). His best example of this comes from his encounter with Mahdi, a twelve year old Bedouin. The lad amazes the academic with the precision, range and incisiveness of his baboon observations. Here 'was a nomad boy who had never been to school and, purely for his own interests, had observed a wild animal and understood it just as I try and do ... Mahdi was a born scientist, more than I am' (Kummer 1995: 312).

Either way, whether discussing self-trained non-professionals or indigenous savants, the general point is still clear: it is not only university educated outsiders who produce anthropological knowledge, and we ignore that at our peril. Today not all locals fill this role of the non-professional. In countries where primates reside naturally the 1990s saw an increase in indigenous primatologists receiving degrees in North America, Japan or Europe and, increasingly, (in the last decade) in their own countries. In many primate-rich countries, such as Indonesia and Brazil, the majority of researchers engaged in primate studies will soon be nationals as opposed to foreigners. This has led to some friction as the passing of the post-colonial baton has been slow and not without controversy.

Fieldworking anthropology, broadly speaking

The fieldworking specificities of each sub-field may be particular, but some commonalities are already clear:

Length of fieldwork

Social anthropologists like to pride themselves on the duration of their fieldwork: one year at least; two years better. An increasing number make repeat trips, but there may well be long gaps between those visits. But even that level of commitment pales in the face of primatological practice. For some primatologists maintain field research sites where a Director may supervise, over the course of decades, the continuous observation of the primate population by an overlapping series of graduate students and local workers. Sommer, in his contribution, describes the establishment of one such site.

The benefits of repeated, or better protracted fieldwork are obvious. What might have seemed lasting, on an anthropologist's first trip, may turn to be historically contingent on a subsequent one. In primatology, Sussman (in this volume) argues similarly: he underlines the worth of very protracted fieldwork, conducted by several researchers, because local history can be just as influential as ultimate, selective explanations.

A further benefit of long term fieldwork is the potential of an illuminating serendipity: the unexpected event which can make us revise our understanding of local life. Its value has long been noted and lauded in social anthropology, where unforeseen occurrences have radically altered many fieldworkers' comprehension of

life around them (see, e.g., MacClancy 2002). Kutsukake, in this volume, discusses the central significance of the previously unseen, and how it can change primatological fieldworkers' approach to the locals they are studying.

But there are of course downsides to very long term team fieldwork. A key worry here is who is habituating to whom, and to what effect? One very striking example: by 1975, the Harvard-Chiapas project, which had involved over 140 anthropologists, into the town of Zinacantan had lasted so long that half of its 11,000 or so inhabitants could not have known what it was like to live without the scrutiny of an anthropologist. In the words of one commentator:

It is difficult to believe a community of this size ... exposed to twenty years of outsiders prying and probing has not been profoundly affected by it. A body of trained and paid informants has grown up, two of whom can 'type in both Tzotzil and Spanish, and have learnt to footnote when they come to a difficult passage to translate'. On occasion an informant has also been shipped to Cambridge, Massachusetts, 'to be available for interviews to students'.

It seems safe to assume, however, that it is in the interests of some Zinacantecos to keep the information flowing and the project rolling (Anon 1977).

At times like these, the obsessions of anthropologists can take on surrealist tones, and the status of the information generated appears to float off into uncharted realms. In a similar fashion some primatologists have voiced concerns that the chimpanzees at Goodall's original field site, Gombe, have been habituated for so long and so intensively that we can legitimately query the origins and contexts of the behaviour studied.

Fieldwork reality and representation

Many social anthropologists and some primatologists might still try to maintain the fiction of the lone intellectual in the bush or other challenging environment. But this literary pose has been well exposed for the rhetoric that it is. Even if not working within a team of colleagues, no field anthropologist is ever truly alone. Malinowski might have lived with the Trobriand Islanders for years, but he also spent valuable time with nearby planters and missionaries (Young 2004). The same applies to primatologists. As Jolly and MacClancy point out in their contributions to this volume, one is rarely studying only one group of local primates, whether human or non-human. No matter our original focus, other groups can easily impress themselves onto us, and enter our field of research vision, whether we desire them there or not. But such is in the very nature of field research. In their comparative work on field research, the historian of anthropology Henrika Kuklick and the sociologist of science Robert Kohler underline this point. Field sites, unlike laboratories, are natural locations. They are public spaces, never exclusively scientific domains, and

fieldworkers share those spaces with all their residents, whether indigenous or not. Scientists may be able to exclude all others from their labs; fieldworkers cannot do that. In the field, the distinction between the purportedly 'professional' and the often dismissed 'amateur' becomes much, much more blurred, and usually to the benefit of the discipline (Kuklick and Kohler 1996: 4).

Schumaker makes the related point that early anthropologists had to find ways to make their fieldwork-centred approach acceptable to gatekeepers in the field (Schumaker 2004). Fieldworkers were not to indulge in behaviour which might endanger their continued stay in the field. This caveat applies just as strongly today, and just as strongly to field primatologists and biological anthropologists as well. It is a further reminder that we are not alone in the field, but must take account of those within it, especially those in positions of power over us.

Moreover, the research practice of many social anthropologists is often much more varied than they would give us to know. Instead of relying almost exclusively from living with locals in the field, many have in fact also gained from a somewhat wider diversity of sources. Judith Okely, who investigated social anthropological practice, found that fieldwork, in reality, 'included searches in archives, scrutiny of national legislation and participant observation among local officials' (Okely 2007: 360). Lederman argues that it is for this reason of diversity that a renowned paper by a leading commentator of contemporary anthropology, George Marcus, praising the power of multi-sited fieldwork (Marcus 1995) has been so multi-cited: Marcus's paper was, unintentionally, less a prospective programme than a summary of much present activity (Lederman 2008: 315). Primatology also participates in this trend to an extent. No longer is most primatology practiced simply via observations and recoding of behaviour. Today practice in primatology ranges from basic observations, to field experiments involving the manipulation of food and individuals, to genetic analyses via faecal collection, to follows of animals via global positioning satellite (GPS) enabling the researchers to view the animals through a computer screen rather than a pair of binoculars. Additionally, although not 'multi-sited' ethnography, primatologists are realizing the substantial importance of studying the same species at different locations and in different ecologies in order to better understand the patterns and ranges of variability. However, despite some parallels with multi-sited fieldwork, a major difference between modern primatology and modern social anthropology (vis-à-vis Lederman 2008) remains that only a small minority of primatologists actively promote a concept of diversity both in location and in theoretical perspectives in theory and practice (i.e., Fuentes 2007 and Fuentes chapter here).

The morality of it all

These days it is made patent to all fledgling fieldworkers, no matter their discipline, that ethics is not a latter-day 'add-on' to research. On the contrary, continuous ethical negotiation is central and primary to all styles of fieldwork, no matter the discipline. The respective learned bodies may draw up 'Ethics guidelines' for field practitioners, but it is generally accepted that these prescriptions, despite their authors' best intentions, must remain by definition generalized guides. While striving to maintain

key principles from the beginning to what is seen as the end of research, neophyte fieldworkers have to be forewarned that they will probably encounter moments when principles may diverge or even clash, and that they may well be forced to choose which ethical road to go down. In these moments, inaction is not a withdrawal of responsibility, but a chosen form of action in itself, which can be subject to ethical assessment. Once in the field, there is no easy escape from ethical involvement.

The qualitative and the quantitative

Most social anthropologists shy from any suggestion of quantitation in their studies, as though the very act of measuring numerically was itself suspect. And in many cases their fears are well-grounded, for their aim is to discover in an open-ended mode other people's visions, and not to triangulate them via questionnaires constituted in anthropologists' categories (MacClancy 2002). Statistical data, if needed, are only generated in the latest stages of fieldwork, and even then in a very diffident manner. In sharp contrast quantitative material is integral to most primatology. Qualitative observations, as Kutsukake notes in his contribution, are usually regarded as secondary. His question is whether that is appropriate.

Contemporary biological anthropologists, as Panter-Brick and Eggerman underline, work to integrate the two. Quantitative research gives them the numerical data they require, while qualitative work guides what data they wish to produce, and helps them interpret the significance of the data they do generate. And, as Hladik demonstrates in his chapter, there can be common quantitative approaches between biological anthropology and primatology: the statistical work of one can illuminate that of the other.

Kutsukake exemplifies a different style of crossing boundaries: the use of primatological field techniques to study both human child development and other animals, such as meerkats. In this way the interconnections between the research styles and aims of our three disciplines can be shown to be as unexpected as they are multiple.

Who knows tomorrow? Quantitation may yet become fashionable again within social anthropology.

The academic, the popular

All too often academics distance their writing from allied, but popular work. It is as though they regarded non-academic books as unworthy of academics' attention, even if they covered the same terrain. Yet it is very easy to argue that this academics' division of labour is short-sighted, self-serving and dangerously misleading (MacClancy 1996). While there are very few popular works in biological anthropology so far, both social anthropology and primatology are well-known to a broad non-academic public thanks to the bestsellers produced by a series of practitioners who each had a story to tell and knew how to tell it. They have portrayed fieldwork, whether among gorillas or witches, chimpanzees or crack dealers, as an exciting adventure of self-realization and academic advance. In the process they have swelled the ranks of

our disciplines and, perhaps, facilitated their sustained funding. Both Asquith and MacClancy, in their contributions, focus on popular publications in primatology, exposing the key moments in this literary style and the underlying agendas. Taken together, this pair of chapters demonstrates that, from the beginning, primatologists have looked beyond the boundaries of the academy for their audience. Just as was the case for social anthropology (MacClancy and McDonaugh 1996), if we wish to understand the development of primatology in a rounded manner then we need to examine how literary-minded primatologists have expressed themselves to others.

* * *

Perhaps what has surprised us both is the number of dimensions of fieldwork common to all three disciplines: the need to habituate oneself as well as the group studied; having to deal with multiple groups; the permeable boundaries of the field; accommodating for the unexpected; bonding with the studied; fieldwork and its ethics as process; and so on. Even that great, supposed divider – language – is revealed as only a relative difference, with primatologists gawping at their occasional ability to engage in inter-species communication. Also, we can identify national styles of fieldwork within both social anthropology and primatology, while at the same time recognizing that these styles are dynamic, may well subsume a diversity of practices, and may influence one another. They are not fixed categories.

It is simplistic to characterize any one discipline in unitary terms. There are always competing theories, different schools of thought vying for pre-eminence. Our space, however, was limited. We could not give room to all. For example, we have not included a comparative discussion of field notes – what is taken as conventional and sufficient for fieldworkers in each of our disciplines (see, e.g., Sanjek 1990). Nor did we wish to mask difference within a discipline. Thus some of our contributions point in different directions; a few are even opposed to one another. We regard this diversity of viewpoints positively. We wished to stimulate discussion, not foreclose it. We wished to open up our comparative topic, not impose an authoritarian, bogus unity. Similarly, we have not covered all topics related to fieldwork which cut across our disciplines. Our aim, after all, was to instigate debate, to inspire primatologists and biological anthropologists to step back and critically assess their fieldwork practice. In the process they just might get us all to reconfigure the comparative project within anthropology, broadly speaking. For this is a project which could benefit us all.

Acknowledgements

We are grateful to Simon Bearder, Peter Parkes and Stan Ulijaszek, and two anonymous reviewers for comments and suggestions.

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2

The Dos and Don'ts of Fieldwork

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Research in the biological anthropology of recent human populations has largely focused on documenting the great variation that occurs within and between such populations and in attempting to explain the causes for it. Where the variation is mainly of genetic origin, as for example in blood groups, the principal cause of similarity between groups is recent common ancestry: so analyses of such characters will therefore tend to reveal evolutionary relationships between populations. Where, however, the variation has a large component of environmental determination and arises directly from the particular conditions under which individuals develop, similarity and differences may have little to do with ancestry. For many years such variation was perceived as a nuisance, but increasingly it has been recognized that it provides insight into the quality of environments and into characteristics which facilitate survival in those environments, i.e., it is adaptive.

Examination of these issues clearly requires fieldwork and biological anthropologists have examined and measured people all over the world. Initially, such work was undertaken by individuals but as the number of characteristics which needed to be taken into account increased, studies were conducted by teams composed of various specialists. Periods spent in the field, however, tended to be short, rarely more than a few weeks, and most of the time was spent analyzing data back in laboratories. This chapter is concerned with contrasting the experience of two expeditions in the 1950s and early 1960s: one to Namibia and the other to Ethiopia.

The original aim of the Namibian study was to document the genetic variety among the various indigenous populations of that country, Bantu, Hottentot, San Bushmen and some hybrid groups, and to determine affinities between them and with people in surrounding countries. Quite a lot was known of physical appearance characters, but only a little of genetic markers. At the time many new blood markers were being discovered: more blood groups, various serum proteins and red blood cell enzymes. Special interest was to be given to the so-called Black Bushmen of the