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The Ethical Fieldworker, and Other Problems

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We are, by definition, moral beings. There are no human groups without morals, however vaguely formulated, however unsystematic these morals might be. There are no non-human groups with them: apes, great or small, might display creativity and fellow concern, and many other primates and social mammals may engage in diverse forms of social reciprocity, but they do not have culturally prescribed standards of ethical conduct. Morality, in this sense, is a distinctive, essential, integral aspect of humanity. Without it, we are regarded as not just inhumane, but as inhuman.

It is surprising, then, that a social anthropology of morality has only begun to emerge as a modern field of endeavour within the last few years (e.g. Howell 1996; Lambek 2010; Zigon 2010; Faubion 2011). It is as though anthropologists did not realize that morality could be studied productively, cross-culturally until very recent times, and had quietly forgotten that Westermarck had inaugurated the very project over a hundred years ago (Westermarck 1906–1908). In a similar manner the interest of most social anthropologists, in the UK and the US at least, in the ethical dimensions of their fieldwork practice has been patchy at best, rising and falling over the decades since the 1940s (Rynkiewich and Spradley 1976; Fluehr-Lobban 1991; Koepping 1994; Caplan 2003). The latest surge of interest appears to be the result of a professional reaction to recent cases of ethical misconduct in the discipline (e.g. Borofsky 2005), the rise in debates about the apparently competing claims of culture and rights (Cowan, Dembour and Wilson 2001), and the implementation of ethical regulation, via Institutional Review Boards (IRBs) in the US and University Research Ethics Committees (URECs) in the UK. Primatologists only seem to have taken ethical concerns on board even more recently. The exception appears to be biological anthropology where, perhaps because of its occasional association with clinical research, practitioners have for some time been aware of the need for rigorous ethical procedures when fieldworking.

However, what has not yet been attempted is the transdisciplinary comparison within anthropology, broadly conceived, of ethics in the field. In other words, until now there has been no consideration of what is common to the moral challenges faced by fieldworkers in social anthropology, biological anthropology, and primatology. Nor have practitioners questioned what might be distinctive to the moral field practice of each discipline, or pair of disciplines. They have similarly neglected to ask what are the significant commonalities, and the telling differences. The comparative investigation of these queries is the primary aim of this book.

A secondary aim is to contribute further towards the modern development of a transdisciplinary anthropology, one pitched, above all, at the level of methods. In this sense, this book is a further effort by the Anthropological Centre for Conservation, Development and the Environment (ACCEND), based at Oxford Brookes University, to give substance to a reinvigorated, broadly based anthropology. This book is a complementary successor to our *Centralizing Fieldwork* (MacClancy and Fuentes 2011), whose focus was on the nature, contexts, and process of fieldwork itself. To repeat a caveat stated there: our concern at revitalizing this cross-disciplinary exercise is not to reduce all forms of anthropology into a singular, dominant, scientific paradigm. We have no truck with any reductionist programme, however veiled. Rather, we seek to stimulate a lively and open dialogue across arenas of anthropological inquiry. Instead of trying to squeeze the variety of anthropologies into a single paradigm, we are more concerned with facilitating mutual interaction across sub-disciplinary and theoretical boundaries. We wish, in sum, to exploit, not to confine, the transdisciplinary potential of our subject.

The range covered by the chapters demonstrates this desire. Melissa Parker, a biological anthropologist, compares the challenges she encountered in two major fieldwork projects, one in a British teaching hospital on AIDS and sexual networks in the UK, the other in East Africa on neglected tropical diseases. Though the two projects were very different from each other, both highlighted the impossibility of predicting many of the ethical issues which may arise in the course of fieldwork, including the need to make decisions which could have life or death consequences for her informants. She argues that the solution is not to impose ever tighter controls on what can and cannot be researched. The following chapter by Matt McLennan, primatologist, and Catherine Hill, biological anthropologist, develops Parker's main finding in a different dimension. McLennan, supervised by Hill, conducted fieldwork in an East African territory. The government had not classified the area as protected, and the endangered resident chimpanzees often came into conflict with local farmers. What McLennan observed, to his alarm, was the effect his presence had on local social and political dynamics. Hill and he conclude that a clear ethical framework is needed for conducting primatology in landscapes which are increasingly dominated by humans.

Primatological concerns continue in the next four chapters. Karen Strier reviews the evolving ethical terrain of her long-term fieldsite. Both the research questions and the animals' demography have changed over time. However, what most concerns her is whether the continued presence of human observers over such an

extended period of time means we can truly regard long-term observational studies of primates as being as non-invasive as they are touted to be. Her fear is that these studies become invasive, as they affect the spaces and ecologies around social landscapes in and around the fieldsites. She thus queries the ethical terrain at the core of primatology: the long-term study. Nobuyuki Kutsukake, in Chapter Five, broadens the debate. He considers just how many sets of ethics primatologists have to contend with: the researcher's personal morality, that of the community with whom he or she is residing, and that of their profession. These different codes may at times run in harmony; often they are at odds with each other. Kutsukake suggests that mutual communication, understanding and negotiation might well solve some of the ethical conundrums which arise during fieldwork, in an almost inevitable fashion. In their chapter, Katherine MacKinnon and Erin Riley review critically the outcomes of several recent symposia and publications dealing with broad questions about ethics in primatological fieldwork. For them, the key concerns are the need to incorporate more ethics-oriented considerations in research projects, and particularly in the teaching and professional training of primatology students. They challenge fellow primatologists to consider more seriously ethics as central to their enterprise. To that end, they propose the development of a formalized code of ethics to guide future primatological field research. Anna Nekaris and Vincent Nijman take a very different tack. Using statistical analysis of Great Ape field sites, they contend that the researchers there have concentrated their work on chimpanzees, gorillas or orang-utans and paid much less attention to primate conservation and other endangered primates inhabiting these sites. Nekaris and Nijman thus breach the usually unspoken wall and ask if it is indeed unethical to favour one species of primate over another at a given fieldsite. Because most primate species are today threatened with extinction, they argue that, to reduce the possibility of humans transmitting disease to primates, it is morally imperative that the use of established research sites be optimized.

We do not wish the reader to take our opening paragraph to mean that we are not aware of the rich emerging literature on non-human primate social complexity, recognition of inequity, and the possibilities of altruistic action, even justice, in multiple species (eg. Brosnan 2012; Pierce and Bekoff 2012; Sussman and Cloninger 2011). Such recent work demonstrates that other some primates (and species of social mammals) react, with some consistency, to experimentally produced inequities. They also seek out one another for a range of reciprocal support and create long-lasting robust social relationships that are behaviourally negotiated across time. Primates engage in apparently altruistic acts and rely extensively on social networks and relational partners throughout their life histories. So one can indeed argue that there are predictable patterns of social exchange and systems of expected, and negotiated, behaviour in the primates. However, these are not the same as the kinds of ethical challenges and contexts that emerge from our anthropological study of them or of each other. Our stance, and the point of this book, is to look more specifically at the ethical contexts that are produced by human anthropologists engaging in research on primates and other humans. It is in this sense that we see morality as

a distinctive, essential, integral aspect of humanity that results in complex webs of ethical scenarios and conduct.

It is crucial that readers understand our focus. We are not saying that the ethical concerns of primatologists and social anthropologists are identical or interchangeable. We are saying that there is a great degree of overlap between the two, that they share many ethical quandaries, and that this commonality has been insufficiently recognized. At first glance, these research priorities might appear fundamentally different: primatologists by definition are primarily interested in non-human primates, whereas social and biological anthropologists work with humans. But no primatologist can study a group of primates without also taking into account local residents, whether they be hunters, foragers, farmers, villagers, forest wardens, official gatekeepers, regional administrators: whomever. And dealing with fellow humans means ethics is constitutive of any encounter between primatologist and local. Moreover, there are the broader ethical consequences of any anthropological study, again whether that be of one of the more hairy primates or naked apes.

The remaining chapters are by social scientists. Susie Kilshaw, a social anthropologist, considers the ethical dilemmas and issues she faced throughout her research into Gulf War Syndrome. She deals in particular on three challenges she had to contend with: how to present oneself to those whom we fieldwork; how to manage one's ongoing relationships with them; and how to balance both of those considerations with one's relationship with the funders of the research project. Tina Miller, a fieldworking sociologist, looks in particular at issues of access and consent, data collection, and that often neglected topic, leaving the field. What she finds is a misfit between complex social and cultural worlds. This leads her to propound that researchers are more in need of ethical support than regulation. Em Rundall, whose work was jointly supervised by a sociologist and an anthropologist, investigates the ethical dimensions of a new and rapidly expanding arena of study: the internet. In the course of an anonymous, asynchronous websurvey she carried out, she isolated five fundamental ethical principles which had to be confronted: the inequalities of internet accessibility; informed participation and consent; anonymity and confidentiality; the safety of participants and researchers; and security of data. Her chapter provides extremely useful guidelines for academics wishing to research in this field.

MacClancy closes the book with an ethnographic investigation into one of the greatest areas of contention within our broad theme: the process of ethical regulation by URECs in the UK. Given the agitated debate these committees have caused, it is surprising, perhaps sad, that this is the first ethnographic research on URECs. What MacClancy uncovers makes him argue that these committees should stop regulating social scientific research, and restrict themselves to the ethical training of fledgling fieldworkers.

We have chosen the various themes summarized above because they illuminate a host of trans-anthropological ethical concerns, common to primatology as well as to biological and social anthropologies: the centrality of ethics to the research methods of all three; the integral importance of ethical training to neophyte fieldworkers; persistent questions over the benefits and obstructive downsides of a

prescribed code of ethics; the simultaneous presence of multiple moral codes and the quandaries their encounter cause; the dearth of easy, quick answers; the sustained need to constantly negotiate moral complexities, which may well prove irresolvable in a conclusive manner; the enduring ethical consequences of long-term fieldwork, and of their persistence, even after the academic has left the fieldsite. To summarize: as moral beings in morally complex settings, we have to learn to live with ambiguity.

While we wish to underline trans-anthropological commonalities, at the same time we do not expect a perfect dovetailing between the three areas of endeavour. Each has, after all, its own distinctive aims and specific institutional history. Primates do not object to what is written about them, although the repercussions can carry ethical weight for the author. However, the myriad of local humans that primatologists have to work with may well take exception to what is printed. Social anthropologists do not usually fear that studying one group long-term may affect neighbouring peoples, though it is marginally possible to imagine. Biological anthropologists tend to fit somewhere between practitioners of the two other pursuits, as they may study humans or the interactions between humans and primates. Given the medical funding of so much of their research, the majority of biological anthropologists these days tend to do problem-oriented work among those classed as 'vulnerable' groups; this has long made them particularly sensitive to ethical considerations. We are not trying to straitjacket these three modes of enquiry into one fitting. Rather, we recognize that each is distinct in some ways, but wish above all to explore the common challenges their practitioners are all forced to ponder today.

We also chose the above themes because they concern contemporary procedures, and illuminate the troublesome reality of present practice. Some contributors address questions about the increasing prevalence of formalized codes of ethics and the power of those who regulate their compliance (MacClancy, MacKinnon and Riley). Others investigate the ethical queries raised by the ever-more pervasive use of new technologies (Miller, Rundall). Yet others consider the consequences of having a private body fund one's research: an increasingly common and concerning phenomenon in these neo-liberal times (Parker, Kilshaw). To our knowledge, McLennan and Hill's contribution is among the first to consider how primatological fieldwork may inadvertently threaten the future survival of the very animals whose conservation they came to assist.

We have covered patches of, and interconnections across a broad ground. But we have not tried to broach all current ethical issues. Our aim was not to produce an encyclopaedia, but a provocative guidebook to today's problems for anthropologists of whatever ilk planning to go into the field.

Fielding ethical questions

It was perhaps the Nuremberg Trials of Nazi clinicians which most prompted debate about the ethics of research. Lawyers needed an explicit code of what was medically permissible and what not in order to be able to prosecute those who had committed evil acts under the guise of science. At much the same time medical research in

the USA began to expand at a tremendous rate, and continued to do so for several decades. Some of this research was conducted unethically. The most notorious example was the Tuskegee Syphilis Study, where the effects of the disease on hundreds of African-Americans, over four decades, was observed by clinicians, even though penicillin had become available in the interim and could have been used. The example of the study, coupled with the revelation of the abuses committed by cavalier medical researchers, led first to legislation and then to the widespread establishment of Institutional Review Boards (IRBs) throughout American universities. Given the moral concerns about clinical experiments which underlay their creation, it is not surprising that the research model for IRBs was biomedical, and their monitoring procedures demonstrated this bias. Over time the remit of IRBs extended to include the social sciences as well. This extension has generated outrage among many social scientists, who see a biomedical model for research as grossly inappropriate when applied to the work of their disciplines. A parallel process of extension occurred in the UK at the turn of the millennium and has led to a similar degree of anger. But what, precisely, is going wrong here?

The key initial point is that the overwhelming majority of social anthropologists, as well as a significant number of biological anthropologists and primatologists, do not work in laboratories, in the traditional sense. Laboratories are highly controlled, artificial environments, and those scientists who choose to work in these settings do so in order to control variable factors as much as possible. Their aim is to enable minutely monitored, reproducible experiments whose results can be interpreted and repeated. Fieldworkers operate in radically different environments. They might wish to restrict the number and kind of people that they are researching but they are totally incapable at any time of controlling the movement of individuals, whether in the study group or not, in and out of the field site. Indeed, interaction with locals may well make an anthropologist researcher change her notion of the very boundaries of her designated site. Interaction may also make a fieldworker radically rethink the questions she wishes to pose, and even the very nature of her project. Further, a serendipitous event or comment may illuminate something she had hitherto not foreseen, and so force her to reconfigure her research agenda yet more. The dimensions of the unpredictable multiply. No wonder, then, that so many anthropologists see fieldwork as an essentially exploratory approach, especially in the dynamic contexts which characterize contemporary society, or that a fieldworker is expected to be resourceful, adaptable, ready to follow the data wherever they take her.

The only social anthropologists who do work in laboratories are there to study the culture of science. They are part of an increasing number of practitioners whose approach is more conceptual than physical. They may be working on biopolitics, technoscience, banking practices, public policy, among other topics. To Aihwa Ong and Stephen Collier, these contemporary anthropologists are investigating 'global assemblages': particular technologies, ethical regimes and administrative systems which articulate transformations in the world today (Collier and Ong 2005). To a significant extent, the concerns of these innovative ethnographers overlap with those of Miller and Rundall in this volume, for both of them had to face new sets of

problems created by the ever-expanding use of online communication. If fieldwork is seen as an exploratory process whose endpoint may be hard to perceive, online anthropology is doubly exploratory as both the ethnographers and the studied are learning how to realize the potential of these technologies, increasingly well-established but constantly developing, at an accelerating rate (see, e.g. Boellstorff 2008).

One immediate question which follows is, what is the value, or function, of a code of ethics in research contexts which can evolve in surprising, unanticipated ways, whenever, wherever? Strong defenders of ethical codes argue that their aim is to protect the researched from potential abuse. For the sake of human dignity and physical integrity, the researched need to be safeguarded as carefully as possible. Thus what the researchers plan to do, when, why, how, to what end, needs to be specified beforehand in an extremely detailed manner. Furthermore the researched, as much as possible, must be informed of what is to be done to them. Otherwise, they are ignorant of what they are consenting to.

Fieldworking social scientists counter-argue that these safeguards may well be necessary and justified in the case of medical research but are unsuitable for the kind of investigations they carry out. They readily admit that it is possible that injecting a purple substance directly into a subject's veins could lead to physiological damage. Thus, experiments like this have to be planned with precision and equally carefully monitored. This is not questioned. What is disputed is whether such regulatory measures need to be imitated in the social sciences, whose practitioners do not usually put potentially nasty items physically inside human beings. It is true that exceptions have to be made for those classed as vulnerable, e.g. children, prisoners; statements about maintaining data confidentiality have to be upheld. But these can be seen as qualifying clauses which do not undercut the aims and values of most qualitative field research. Moreover, critics of ethical overregulation underline the incompatibility of forecasting precisely the trajectory of a research project, and fieldwork. In other words, because fieldworkers follow rather than confine others, they cannot predict with any exactitude what they will be doing when during the course of their research. So how can they fill in the forms?

Consent forms raise a further series of issues. The basic idea sounds at first plausible, indeed laudable: informing an interviewee of what is about to occur, why, and of their freedom to end the encounter, at any point, without giving a justification. Problems arise when people may not understand what they are consenting to, especially if they are from cultures or groups where forms are unknown. To many, both interviewers and interviewees, reading and signing preliminary forms is an intrusive formality, an unwanted opener to what they hope will be a relaxed discussion. Moreover, many peoples, particularly those from subaltern strata, are sceptical or wary of all forms. All too often, signing papers is seen as an extraneous bureaucratic exercise which further entangles the signatory in the meshes of the state. On top of that, the form itself implies mistrust, by the producers of the form, who refuse to rely on the professionalism of the interviewer, and by the interviewee, who feels the ethnographer sitting opposite him cannot take his trust as read. Oral consent, captured and preserved on a digital recorder, can suffer the same problems.

In other words, no matter how consent is formally given, the anthropologist ends up caught in the middle, in a situation not of her own choosing.

Of Vampires, Vultures and Other Fieldworking Blood-Suckers

In 1936 the anthropologist Geoffrey Gorer represented fieldwork in starkly superior tones: 'Anthropologists resemble society hostesses in many ways – an anthropologist talking to a "savage" is quite like a lady putting a "not quite quite" at her ease' (Gorer 1936: 27). His condescending attitude may be as out of date as his antique snobbery, but certain fundamental inequities within common fieldwork styles remain hard to shift.

When MacClancy first began fieldwork in Spain, in 1984, he was immediately informed how much indigenous colleagues disliked 'paracaidistas' (parachutists), i.e. foreigners who drop into an area, get the information they need, and then get out, usually never to be seen again. Spanish anthropologists felt their compatriots were being taken advantage of, and given little in return. When Fuentes began fieldwork in the Mentawai Islands, Indonesia, in 1989, he was immediately questioned by indigenous officials and locals whether or not he, like earlier researchers, would simply stay for a short while, gather information and then depart, or if he would be making some sort of tangible contributions to villagers' lives. Anthropologists in general are well aware of the potentially exploitative dimension of their work practice: gaining something in exchange for virtually nothing.

The Malinowskian model of fieldwork is all too open to this charge. The idea is that an ignorant outsider goes to live with a group, tries to make friends with them, participates in their daily activities, continually watches everything, and endlessly questions. The traditional justification for this practice was the archival value of the resulting ethnography, and the potential of ethnographically-grounded cross-cultural comparison to generate generalizations about the nature and variety of humanity. For an increasing number of anthropologists, that justification is no longer enough.

Critics of this style of fieldwork, for many decades almost dominant in anthropology, argue that it is unethical because fundamentally deceptive. A dutiful fieldworker strives to make friends, or something close to that; she and the subject of her fieldwork cannot keep up a formal academic relationship throughout the extended period of her stay. Both will relax and talk as though among friends. However, the key difference between the two parties with respect to those conversations is that at least one of them has an ulterior motive. To put this another way, many fieldworkers are uneasy that they are consciously entering into social relationships in bad faith. They might appear to be extending the hand of friendship, but in reality that is the sentimental cover for their hard-nosed end: to exploit this bond for their own instrumental desires. Defenders of this method might counter-argue that they are open in their aims from the very moment of their arrival; they are hiding nothing. The response is to query how many locals understand academic endeavour, whether they realize fieldworkers are never off-duty,

and whether they comprehend that their fieldworker has no control whatsoever over what others may do with the information she publishes. In the 1960s the Anglo-Cypriot anthropologist Peter Loizos carried out doctoral fieldwork in a Cypriot village. When one of his key pundits read the resulting thesis, he said he was surprised at the detail, then added, 'I think you sociologists do exactly what you like'. Loizos took that to mean, 'You are powerful, and you follow your own interests without regard for others' (Loizos 1994: 45).

The exploitative possibilities go further than this. A fledgling fieldworker who does her job well and produces well-regarded publications gets a job and some prestige. The locals get nothing, usually. Worse, they may be left feeling betrayed. For in a face-to-face community where people know one another from birth until death, the idea of making friends with an outsider may well be very unusual, and the locals might not take on board the fact that their relationship with the incomer has, from the beginning, a time expiry date. So when the fieldworker does depart, they may be left feeling surprised, and hurt (MacClancy 2011). In addition, the presence of the fieldworker can easily raise locals' expectations about the benefits of her stay, over which she has little control. Her failure to meet those expectations may become another source of disappointment and pained feelings. In the 1950s several anthropologists in Papua New Guinea encountered locals clearing a landing strip, for the goods-laden plane the fieldworker had commanded (e.g. Lawrence 1964). More recently Catherine Panter-Brick, engaged in a study of Afghan schoolchildren, discovered that so many were turning up for interview because they thought they were being selected for a scholarship to the United States (Eggerman and Panter-Brick 2011).

The potential pitfalls for biological anthropologists can be even worse. Some self-critical social anthropologists, seeking a striking metaphor, call themselves 'vampires', as though going for the jugular of the studied, or 'vultures', because they treat others like corpses to be picked at (Loizos 1994; Koepping 1994: 105). But the term of abuse for biological anthropologists may be much more cutting, because it is far less metaphorical: blood-suckers, collecting sanguinary samples in the name of science. Unlike clinical researchers, biological anthropologists do not put anything into people's bodies. Instead they may take things out: blood, saliva, and other effluvia. The possible interpretations of this practice are manifold, with indigenous groups vigorously campaigning, on various grounds, for the return of any body part removed from a living person and still stored after their death. The biological anthropologist Alain Froment characterizes his colleagues caught in these disputes as trying to achieve a provisional balance between individual and community rights, and the rights of humankind (Froment 2011: 194–95). His colleague, Jonathan Marks, stakes a more resolute position:

> It could be argued that the major biomedical advance of the twentieth century was neither antibiotics or genomics, but rather the recognition that progress in science is great, but when it comes into conflict with human rights, human rights wins, hands down. The nature of those rights and

what constitutes a violation of them are necessarily evolving subjects, but if science is to flourish, it must do so in the context of public ideas about what is fair, decent and appropriate. (Marks 2010: 4)

For primatologists, the context of their impact is double: humans and non-humans. Most primatologists are faced with a number of ethical obligations: to source country collaborators, local communities in which they work, the actual primates that are the focus of the research, and the local ecologies where they ply their trade. In these ways primatologists are enmeshed in both a local human and a local primate community, and often also in a larger conservation community. Many of the contexts noted above for social anthropologists extend also to primatologists. Furthermore, as noted at the beginning, a primatologist must also consider the impact her presence has on the lives and ecologies of the local primates, who are themselves often threatened and/or endangered. All too frequently, the interests of local humans and of local non-human primates are opposed to one another, thus generating conflict over space, food and the use of local ecologies. This is especially true in the contexts of human hunting practices and the exploitation of tropical forests (Fuentes 2002, McLennan and Hill this volume).

Many anthropologists try to mitigate the imbalance commonly experienced in fieldwork by seeking a means to reciprocate. When MacClancy lived in a village on Tanna, Vanuatu, he offered to set up a small chicken-rearing business for his hosts in acknowledgement for his keep. He later overheard villagers explaining his presence to visitors by saying they were helping him in his studies, and he was assisting them with an economic project. The visitors replied, 'It is straight'. When Fuentes worked at the monkey forest site in the villages of Padangtegal and Ubud, in Bali, Indonesia, he helped them to develop resource management and educational programmes in addition to coordinating and providing scientific and veterinary advisory support. The limits of reciprocity, however, may be soon reached. Loizos recounts that one of his study group turned up in London with a sick child, and asked the anthropologist to fiddle his UK status so that his boy would be treated for free. To the man's anger, Loizos refused (Loizos 1994: 47). One partial means to redress the inherent imbalance in the Malinowskian fieldwork style is to take one's manuscript back to the group studied, describe or read out what is in it, and then take account of the comments made by locals. The underlying hope is that the final publication does not misrepresent the population studied and is a somewhat more equitable product of the encounter between fieldworker and the fieldworked. Of course, the final content of the resulting book is still decided by the author; only her name goes on its front; she alone bears the legal responsibility for its words. When MacClancy adopted this strategy with his book on Carlism, it seemed to work suspiciously well (MacClancy 2000). The villagers described made a few comments but accepted the vast majority of what was written. The supposed result was that the locals did not feel excluded from the production of information about them, and the ethnographer gained a more fine-grained account. The lingering suspicion MacClancy was left with was that the villagers had proved as exemplarily polite as ever: they had corrected a few

points but were maybe reluctant to make more incisive comments for fear of, what exactly? MacClancy remains unsure.

The controversy in the early 2000s over the conduct, while among the Amazonian Yanomami, of the biological anthropologist James Neel and the social anthropologists Napoleon Chagnon and Jacques Lizot did much to develop anthropological debate about fieldwork ethics. Robert Borofsky, in his analysis of the issues raised, has argued that the old anthropological dictum 'Do no harm' is no longer sufficient. It is too passive, implying that anthropologists only need to avoid causing problems. Instead, he contends, anthropologists should be more actively engaged, by seeking to practice just compensation. As he admits, this approach is both 'far more involved and expensive'. It indicates that fieldworkers need to help improve the lives of those they live among, just as they, by giving information, help anthropologists build professional careers (Borofsky 2005: 87–89). How many contemporary practitioners are prepared to practise this is another question – a fact which raises its own question about the present constitution of social anthropology.

Some biological anthropologists seek to redress the imbalance of fieldwork by providing a degree of medical care to the population studied. This otherwise laudable aim has raised much controversy. Some argue that fieldworkers should aim to provide services which are adequate by local standards. Others contend that medically-oriented researchers are obliged to offer the best globally available treatment (Aagard-Hansen and Johansen 2008: 18). Anything less is not just patronizing, it is profoundly unethical.

In recent years, some anthropologists have attempted a seemingly more radical mode of redress: collaborative ethnography. The idea here is that a fieldworker does not go to a fieldsite with her research agenda already more or less clear, and with some ideas about how to reciprocate her hosts' assistance. Instead, she visits a group, states what she would like to do, and consults the locals about what research they would like carried out, what benefits the project could bring to the locals. They then enter a negotiation where a mutually agreed programme of research is formulated. The parties also establish an agreement that the field notes and material will not be used for any other purpose without the prior consent of the studied. The biological anthropologist, Lawrence Schell, who has practised this style of fieldwork with the Mohawk Nation of Akwesasane, upstate New York, states that the consequent research process was noticeably slower but the results richer, and executed in the knowledge that it was as ethically equitable as can at present be conceived. At the same time, and of equal importance, this style of ethnography may, or at least should, improve the lives of the people studied in some way, particularly the economic (Schell et al. 2005, 2007, personal communication, 2 May 2011). In social anthropology Serge Elie, among others, has argued for a comparable approach grounded on a 'reciprocity principle' which underpins his ethnographic practice in order to ensure that the final product has a practical effect on the social conditions of those studied. His wish is that his research product will 'serve as "resources of hope" for the improvement of my research subjects' lives (devoid of any condescending missionary intent), and not merely to fulfil the exigencies of making a living in

an academic institution' (Elie 2006: 68). Some anthropologists have gone further. For instance, the Maori anthropologist Linda Tuhiwai Smith has argued that one effective way to 'decolonize' research methods and so help to redress the present ethical imbalance in anthropology is to enable more indigenous peoples to fieldwork their own communities, setting their own research priorities (Smith 1999). In reaction, several commentators on her highly successful *Decolonizing Methodologies* have worried what role her proposed strategy would leave for non-indigenous researchers (Staehelin 2000; Wilson 2001).

Perhaps this discussion is wrong-footed from the start, where what appears to be the anthropologists' general lack of commitment to the locals is in fact an Anglocentricity, a consequence of the anti-applied approach of Evans-Pritchard, the UK professoriate of his postwar time, and some of their transatlantic colleagues. In fact anthropologists in some other countries practise very different modes of fieldwork. In Brazil, Ramos emphasizes that she and the majority of her colleagues are expected to campaign on behalf of those they study (Ramos 1990). Many Argentinian social anthropologists regard their role as a political agent battling against the State and the powers that be (Guber 2008). In Israel, Emmanuel Marx stressed the commitment of fieldworkers to the fieldworked, the former acting as intermediaries between indigenes and the Government when the bureaucrats wanted to institute major change.[1] In South Africa, anthropologists are obliged to state in their research applications how the proposed fieldwork will benefit local populations (Ainslie, personal communication) In this case, it is the national government which enforces the committed style: it only funds fieldwork which is relevant to policy.

Fieldworkers like Schell and Elie have made an ethical point of collaboration. It can of course be argued that a collaborative approach has long grounded fieldwork. It has just not been placed at centre stage nor lauded as a central research method, until recently. Further, and to our collective shame, it is very likely that some anthropologists were formerly unwilling to acknowledge the extent to which locals themselves set the research agenda or themselves fulfilled the fieldworker's brief. While some late anthropologists are now infamous for hiding their debt to their key assistants, e.g. Melville Herskovits (Price and Price 2003), some others were ready to admit it. The best example here is Franz Boas, whose concern to acknowledge his local collaborator led him to name the latter as a co-author of some of his ethnographic works (e.g. Boas and Hunt 1905, 1906).

Concern that fieldwork be as ethically equitable as possible should not, of course, blind us to the long-known fact that some locals are working hard to take advantage of their fieldworker as much, or more, than she is exploiting them. Kummels and Schäfer recount how, although they went to work with all the inhabitants of one Amazonian village, they unknowingly fell in with one of its factions, whose members were only too willing to use the interlopers to strengthen their already powerful position (Kummels and Schäfer 1994). Such tales are common. MacClancy's original aim in Vanuatu was to do fieldwork among the Big Nambas of Malakula Island. Since they regard cultural information as property to be traded, he was immediately instructed that they consider anthropologists to be thieves; most doors were shut

to him. When one independent chief did decide to take him into his village, he very quickly learnt that his task was to list the 'trouble-makers' there so the colonial officers could identify and remove them. MacClancy left within a week. The work of Fuentes and subsequent students in the Mentawai Islands resulted in significant financial support for one village, and one extended family in particular. This exacerbated a split among a prominent clan and villages, a split which preceded Fuentes' arrival at the site. This fact was only discovered a few years after the original project.

Some anthropologists argue that collaborative approaches do not go far enough, that it is to be expected, perhaps even encouraged, that locals should make the most of fieldworkers. They contend that anthropologists should be prepared to commit themselves to assisting those whom they fieldwork in their political struggles, indeed in whatever campaigns they may be engaged in. In 1990 Philippe Bourgois accused his mainstream colleagues of not merely dismissing researchers' responsibilities to uphold human rights, but of condemning these issues as ethically problematic. In pointed contrast to the then current postmodernist stand-offishness, he called for anthropologists to assume their 'historical responsibility' to address large moral issues because their traditional object of study – exotic peoples – were being violently, traumatically incorporated into the global capitalist economy (Bourgois 1990). Since then, an increasing number of anthropologists have advocated an activist approach (e.g. Hale 2007; Armbruster and Laerke 2008). But taking sides, however praiseworthy, has its own problematic. It is, for example, all too easy for a fieldworker to learn that the side she decided to work with is not as simple or simply identifiable as initially thought. Perhaps she has to choose between competing factions or viewpoints. She may have to ask the question, to whom is her primary allegiance due? Perhaps fieldworkers, especially those in conflictive zones, should be prepared to take a political stand, even if that means taking a choice between 'competing complicities' (Pettigrew, Shneiderman, and Harper 2004: 25). Shannon Speed, who conducted fieldwork in Chiapas, Mexico, tackles a related issue when she argues that 'one virtue of activist research is that it makes the interaction open to definition and the effects open to scrutiny by both the researcher and the community' (Speed 2006: 72). When the community she studied was divided on certain issues, she had to make a personal ethical decision about where her alliance lay. The difference, she contends, is that such decisions are more explicit and transparent. The result is not just an engaged activist anthropology, but a critical version as well.

A somewhat different issue emerges when 'studying up'. If a fieldworker sees her primary responsibility as lying with the vulnerable, the unvoiced, and the oppressed, then any study of an elite must, by definition, be critical. Moreover, it is unlikely that an elite is monolithic in operation and thought, but will be divided by debate and different interests. In these contexts, it is improbable that all members of the elite will approve the resulting ethnography (Stirrat 2005; Schwegler and Powell 2008: 6–7). Thus, we should not expect them to. When the British anthropologist David Mosse tried to get his analysis of a long-term aid project funded by the Department for International Development published, employees who had been criticized in the analysis tried, unsuccessfully, to halt its release. In his defence, Mosse argued that

the publication of studies such as his is a precondition for the understanding that will assist international development to develop (Mosse 2005, 2006). Perhaps, as Tara Schwegler and Michael Powell suggest (Schwegler and Powell 2008: 7), we should rethink the nature of relationships between an anthropologist and those she studies, in ways which facilitate dialogue.

The upshot of all this is that not only is traditional distancing of anthropological fieldwork practice problematic, several of the proposed solutions are equally so. Some fieldworkers have become moral vigilantes to the point of no longer 'observing' the people they live among. Others have formed their own NGOs in order to take a more direct approach to advocacy. In the process the very nature of what it might mean to be an anthropologist is laid open to repeated question. The following is one example, among many: the Oxford-trained anthropologist John Palmer did his doctoral fieldwork with the Wichí of the north Argentinian Chaco. Post-doctoral, he returned in 1991 to 'accompany' them, as he puts it, in their sustained struggle through the hierarchy of courts to retain control over any of their resources which regional capitalists wished to exploit for their own ends: land, timber, oil, etc. His work is externally supported by Chacolinks, a small, international charitable organization. While he continues to write on anthropology, as well as dealing with his burdensome and diverse legal caseload, his publishing priority is to produce work of legal benefit to the Wichí. He remains an anthropologist, although one who is self-subordinated to the Wichí cause.[2]

The Malinowskian ideal was that one should participate as much as possible. But his professional charter was in fact always subject to limiting riders. Certain assumptions were not stated because it was understood by the gentlefolk majority who first filled the ranks of British anthropology that they did not need to be voiced. Perhaps the most notorious limitation was the taboo on sex with the locals: in the 1930s the only person prepared to openly dispute this, at Malinowski's seminar, was the self-promoting iconoclast Tom Harrisson, co-founder of Mass Observation (MacClancy 1995). It is true that a few anthropologists have entered into enduring relations with locals (e.g. Good 1991; Kulick and Wilson 1995; Guha 1999), but most have kept away from, or kept quiet about, such relations because they know they are generally viewed as exploitative. In contrast, in primatology there are at least a few well-known examples where the researcher has allied herself with prominent indigenous peoples via marriage, in order to subvert the hold of external, national authorities over local forests and primates. On top of that, there have always been some societies in which it has been difficult for fieldworking anthropologists to avoid matrimony or sexual relations with the locals. Among the nomadic Tuareg of sub-Saharan Africa, for instance, both marriage and divorce are easily entered into and all tents are owned by women. Thus a male anthropologist doing fieldwork with them has had to choose whether not to marry and so be regarded as a boy, or to enter a tent as its owner's husband. Moreover, there are today an increasing number of groups whose sexuality is central to their identity (e.g. Kirtsoglou 2004). For many of these peoples, a fieldworker who does not participate intimately is seen as stand-offish and guilty of an Olympian disregard for local mores. They are not wanted.

Anthropologies: Military, Commercial, Commissioned

July 1940: Evans-Pritchard, then an officer in the Sudan Defence Force, writes to his friend Meyer Fortes:

> Neither of us are under any illusions about the place which anthropology occupies in the minds of Government officials, but I was a little surprised they did not easily admit that such activities as intelligence and sabotage are those for which an anthropologist's training benefits him. (Quoted in Goody 1995: 65)

If there is one style of fieldwork which has raised controversy in recent years, it is the re-emergence of social scientists collaborating hand in glove with the military. To find anthropologists working for the army of their nation is nothing new. In 1919 Franz Boas publicly denounced the activity of a group of his American colleagues, who went unnamed, because they had used their field research as a cover for spying on behalf of the national government. During the Second World War many US-based anthropologists, openly and without apparent qualms, participated in the war effort. They thought the nature of the conflict justified the exploitation of their skills (Price 2008). In the postwar period many anthropologists continued to receive funding from US military organizations, though not quite so openly (Price 2004). During the Vietnam War anthropologists who did fieldwork among rural Vietnamese advised generals and Department of Defense officials on ways on achieving US objectives in the country. At much the same time, some anthropologists participated in Project Camelot, a research-grounded counterinsurgency project, which was much criticized by those who thought that it subverted the ideals of social science. The strength and number of these criticisms led to the cancellation of the project. It is not yet known how many British anthropologists have acted similarly. Evans-Pritchard, though frustrated in 1940, did eventually get his way, exploiting his anthropological skills for military ends, in the Middle East and North African campaigns.

These applied forms of anthropology have become, yet again, so polemical because in the mid-2000s, the US Army established a Human Terrain System. Its aim was to establish small Human Terrain Teams, mixed groups of military and social scientist personnel working in areas where US troops were deployed. Their task was to gain understanding of local socio-cultural conditions. Commanders would employ the information gained to achieve objectives pacifically, without the use of military force. Their level of success has been varied and questionable. Their defenders have argued that their use is ethical when their military superiors are engaged in a 'just war', that the Teams 'save lives', and that the application of 'socio-cultural knowledge reduces violence, creates stability, promotes better governance and improves military decision making' (McFate 2007; Lucas 2009). In response to the ensuing debate within academia, the American Anthropological Association established in 2006 a Commission on the Engagement of Anthropology with the US Security and Intelligence Communities to gather data on and assess the nature

of the System and its Teams. In its Final Report, released in December 2010, the Commission highlighted 'the lack of a well-defined ethical framework of conduct for the program'.[3] As several had already pointed out, and the Commission iterated, given 'the inability of HTT researchers to maintain reliable control over data once collected, the program places researchers and their counterparts in the field in harm's way' (Gonzalez 2009; Network of Concerned Anthropologists 2009).[4] In other words, anthropologists who are embedded in the military cannot predict how the information they garner may be used and so potentially endanger themselves, their fieldworking colleagues, and the reputation of the discipline.

A key point generated by this debate about the Teams is that anthropologists in general have all too often been nonchalant about the possible uses of the information they publish. Too many have not thought through the consequences of including sensitive data in their papers and ethnographies (see Price 2004: 350). A central difficulty here is just how far into the future anthropologists need to think. Recent work has demonstrated the ways in which even ethnographic data published decades ago is today being exploited by locals in unexpected fashion. For instance, in Vanuatu, John Layard's *Stone Men of Malekula* (Layard 1942) is today being used by some Malakulans to strengthen their land claims, so deepening the rift between rival claimants (Geismar 2009). This might appear an extreme example (Layard did his fieldwork in 1914), but the point is still worryingly clear: we cannot accurately foresee how our words will be utilized, by whom, when, for what end, with what result.

These caveats apply not just to academic and military anthropology, but to all externally commissioned fieldwork. If the research of an anthropologist is funded not by an academically-oriented charitable foundation but by an external organization, the resulting data do not usually belong to their generators, but to the funders. Thus Judith Okely, whose fieldwork on gypsies in the UK was financed by the Department for the Environment, learnt to her surprise and shock on completion of her research that all her fieldnotes were the property of the British Government (Okely 1987). Working for a government department gave her access to otherwise confidential documents, but that generated yet more problems. When she learnt that a County Council was considering the creation of an inter-agency body of highly dubious legality, she felt too intimidated at the time to blow the whistle and only revealed the content of the relevant document decades later in a relatively unknown Scandinavian academic journal (Okely 1999). The ethics of anthropologists who work as consultants for business organizations can be just as complex (see examples in Cefkin 2009; Jordan 2010). The chapters by Parker and Kilshaw exemplify these moral quandaries in illuminating detail. The consequences of highlighting institutional incompetence may be all too clear. There is evidence that anthropologists employed as consultants on international aid projects can fail to win further contracts if they make their criticisms too loudly. One adaptive response to this negative outcome is for the suddenly underemployed anthropologist to re-present herself under the guise of a new consultancy group.

It is a general problem that many of the organizations for whom anthropologists act as consultants include gagging clauses in their contracts, which prohibit

the publication of unwanted research conclusions. In addition, research funded by charitable bodies, who have their own funders to answer to, may be subject to ethical regulation by IRBs in which special interests are deeply problematic. The consequence is reactionary: here, institutional protection may be not just inhibiting academically, but in effect act as a gatekeeping procedure which ends up, over time, structuring bodies of knowledge. In other words, the desire of those who run institutions to keep the funders happy and the grant money rolling in frames and directs what kind of research activity are advanced. To put that another way, public-private neo-liberal tendencies shape the allowable. A systematic investigation of this skewing has yet to be carried out, although its significance is patent. One immediate question: who would fund this project?

The Virtuous Anthropologist, Robust Defender of the Moral Low Ground

Fieldworkers have to negotiate their research path simultaneously through at least three sets of ethics – their own personal one, that of their discipline, that of the locals – and maybe more, e.g. those of gatekeeper organizations who oversee access to the population studied, whether national, regional, provincial, ethnic, religious, professional, institutional, commercial, and so on. None of this negotiation is easy. In the same way that fieldworkers have to be prepared to question radically their research goals, methods and most cherished concepts, they must also be ready to revise their ethical parameters and be disposed to rethink their passage as they tiptoe across a complex moral terrain. To put this another way, if fieldwork is today a moral minefield, all contemporary fieldworkers are obliged to act as amateur bomb disposal experts. The question is, how to make them better at that job?

Some contributors put forward further ethical guidelines as a possible solution. The usual course here is to propose that the professional code be made more robust, detailed, responsive to contemporary concerns. The trouble is, as will now be very clear, that no code can specify what to do in every situation, as not every situation, even at a general level, is foreseeable. To this extent, the level of detail in a code verges on the irrelevant, and indeed is frequently misleading.

Moreover, as the profession wishes to practise and to display the highest standards of integrity, most codes are couched in near absolute terms, as though the ethical grounds on which they rest are of the firmest, and frequently with pretensions towards the timeless. The difficulty here is that all ethical codes, whatever their claims, fail to transcend the contexts of their production; on the contrary, they are products of their own time and circumstances, and need to be openly recognized as such. For instance, the 1971 'Principles of Professional Responsibility' produced by the American Anthropological Association (AAA), declared that members have to put the interests of the peoples they study first, and that clandestine research was taboo. However lofty-minded these principles might appear, they were at the same time the product of lobbying by anthropologists who disagreed with their government's use of their discipline in counterinsurgency operations. In the next decade, a different faction of anthropologists argued successfully for the ban to be softened.

These and other examples from the history of the AAA suggest strongly that internal debates which are apparently about ethics are often as much about political sniping from different sectors within the Association. Indeed Carolyn Fluehr-Lobban, in a review of these debates, goes so far as to state, 'A uniquely political history of the discipline can be discerned by examining closely the issues of ethics and professionalism in anthropology' (Fluehr-Lobban 1991a: 26). In other words, if you wish to understand the shifting alignments and power struggles within the profession's largest, most prestigious association, just follow the crooked path of their ethical wrangles. Here, ethics is not above politics, it is a key to it. Goodbye, absolutes.

If ethical codes are products of their time and place, then as committed anthropologists, we should expect cross-cultural comparison to reveal how peculiar each one is. In fact, the projected formulation of these codes can be so polemical that the anthropologists of some nations elect not to have one at all. In the 1990s German ethnologists, in an extremely enlightened moment, chose not to decide on a code because it would limit their ethical discussions, while an anthropological association in France used a workshop to broadcast its abhorrence of American-style ethical codes (Pels 1999: 101). The projected research of Dutch anthropologists is only subject to review if it places 'a demonstrable physical or psychological burden on its subjects' (Bosk and De Vries 2004: 259). And, as always, we need to attend to the silences as much as to what is stated. For instance, a comparison of three codes in biomedicine with those of the American and British anthropological associations showed that, unlike their biomedical counterparts, anthropologists tended to focus on the powerful leverage that commissioning agencies could exert over them, while quietly downplaying the strength of their own position over those they studied (Aagaard-Hansen and Johansen 2008: 19).

We can also query the effectiveness of any such codes. It might be consoling to the morally sensitive to know that their discipline has a code of ethics, but the important question is whether their professional association is prepared to implement it when necessary. The historical record is not reassuring. Anthropologists in interwar Germany had a well developed gentleman's code, but that did not prevent the majority of them later assisting the Nazi regime, and some of them participating in every phase of the Holocaust (Schafft 2007). Similarly David Price, in an exemplarily well-documented exposé of the AAA during the McCarthyite period, found the Association, bar rare exceptions, guilty of repeated inaction when some of its number were attacked by government agencies: 'instead it buried its head in the sand, ignoring anthropologists being fired, blacklisted, and taught the valuable lessons of self-censorship' (Price 2004: 69). If we cannot include British anthropology in this unfortunate list, that is most likely because this dimension of its political history is yet to be examined. The only indications MacClancy has found so far of such practice are the statements by the veteran anthropologists Ronnie Frankenberg that in postwar Britain Marxist anthropologists were banned from working in UK colonies, and by Peter Worsley that Evans-Pritchard prevented him from taking up a post because of his left-wing political beliefs (Worsley 2008).[5] In sum, it is difficult to laud professional codes of ethics in the Anglophone world when the

actions of professional bodies and prestigious individuals there have been at times pusillanimous, if not downright subversive of radical, anti-government positions.

Codes of ethics and attempts to implement them can also have grievous consequences for the very disciplines they were designed to assist. For the whole process of ethical review and regulation is having a profound impact on research topic choices. At first, in the UK, this affected medical anthropologists wary of NHS barriers against working with patients who are all by definition 'vulnerable'. Now it has become a question of established anthropologists discouraging prospective research students from topics, however promising they might be, which require burdensome, impractical forms of clearance. Similarly, today, those in the higher ranks of UK university hierarchies remind their employees of their need to perform outstandingly in the Government's periodic research assessment exercises. Of course, ethically troublesome research may well not easily fit this bill. The insidious consequence is that both the tenured and the upcoming are pushed none too subtly away from ethically complex subjects. As Foucault recognized years ago, monitoring is not neutral; it is distorting.

Codes of ethics are reactive documents, co-produced by members of a professional association in response to a threat or crisis. The relative status of these regulations might undermine claims to ethical absolutes but does not affect the pressing nature of ethical considerations. Of course, the nature and style of these carefully wrought codifications are necessarily different from ethics as practised by all of us, every day. These daily moralities, to give them a term, are lived, embodied practices, employed and debated in a quotidian fashion, and whose fundamentals are rarely reflected upon in a sustained, critical manner. To a significant extent, it is misleading to call them 'personal ethics', as that term brushes aside the essentially social nature of these moralities, the continuing products of interaction between humans in communities, however constituted, fractured, or open they may be. In a related mode, Lynn Meskell and Peter Pels speak of 'embedded ethics', where morality, codified or not, is a pervasive dimension in the actions of all parties to the anthropological endeavour: anthropologists, their overseers, the studied, the media, the publics addressed, and so on (Meskell and Pels 2005). Here 'truth' is but an aspirational term, and endless negotiation the day-to-day reality.

In these shifting, dynamic contexts which characterize the contemporary situation, anthropologists and primatologists in the field have to juggle, at the very least, their own personal but socially encompassed ethics; professional codes of practice, however flawed; and the moralities of those studied, to the extent that they understand them. Perhaps the best that can thus be desired is that fledgling fieldworkers be trained to be as ethically aware as possible. And, given that they cannot be trained in every situation, because so many are unforeseeable, they can at least be schooled in the types of dilemmas they may well face, e.g. the local reactions to primatological research discussed by McLennan, the difficulty of leaving the online field highlighted by Miller.In these circumstances where moralities are multiple and all absolutes relative, claims to wear a badge of virtuous certainty, as though it conferred an aura of ethical superiority, are but pretentious postures, to be dismissed

or researched. They are anathema. What is much harder, and more realistic, is for the would-be virtuous anthropologist to stake out an ever-negotiated claim as robust defender of the moral low ground. This is all we can claim.

Notes

1. See the video, http://sms.csx.cam.ac.uk/media/1125921. Accessed on 5 July 2011.
2. Chacolinks, an Oxford-based, international charitable organization, supports legal action in defence of Wichí land rights, against the threats of logging, deforestation and intensive cash-crop production. It is committed to raising awareness and letter-writing campaigns. In addition it provides funding to assist the setting up of small-scale projects to relieve poverty, and raise the standard of health among the Wichí (see www.chacolinks.org.uk). MacClancy admits to a special interest: he has been chairperson of Chacolinks since its founding.
3. http://blog.aaanet.org/2009/12/08/aaa-commission-releases-final-report-on-army-human-terrain-system/. Accessed on 2 May 2011.
4. The quotation is from the webpage cited in note 1.
5. See the video and transcript, http://sms.csx.cam.ac.uk/media/1116760;jsessionid=F59E0A1E28381B1292BF9EF49590B133. Accessed on 10 May 2011.

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