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The 'problem' of ethics in contemporary anthropological research¹

By John R. Campbell (School of Oriental and African Studies)

Why is it that 'ethics' is seen as a problem in anthropology? This paper seeks to explore this question by looking at (a) historical shifts in the relation between ethnographers and their subjects/informants and (b) anthropological practice. I am interested in past anthropological practice to see whether it provides a reasonable guide to future practice, specifically with regard to the ethical conduct of ethnographic fieldwork.

A post-graduate conference examining new and innovative research techniques is not the place to review the checkered history of ethics in anthropological practice. I propose instead to address an issue that emerged implicitly from the silence, on the part of conference delegates, to questions I asked from the podium: (1) were they familiar with the discipline's ethical code? (2) How were they dealing with ethical issues that arose in their fieldwork? It was clear to me that many students perceived ethics as a 'problem': some perceived ethics as a recent and unwarranted imposition by the state/funding organizations (specifically in relation to 'informed consent'); others have not thought seriously about how ethics arises in their work; still others seem to think that their research does not raise 'ethical concerns'.

Perhaps this ambivalence towards ethics should not surprise us given the general tenor of statements on this issue by leading social scientists. To quote just a few:

'Ethical codes arrived late in anthropology and played a negligible role in the professionalization of the discipline' (Pels 1999:110)

'[T]he main ethical issues and debates about ethics ... are not really capable of resolution. This is why the ethical debate has scarcely moved on since the 1960s' (Bryman 2001:476)

'Ethical codes ... are likely to become more important as anthropological research develops new fields, particularly those that bring it into contact with policy makers and other professionals' (Shore 1999:124)

The first and third statements are from anthropologists; the second is from a leading sociologist. Such statements probably represent the views held by most social scientists, but can they all be correct? Pels' 'Prehistory of ethical codes in Anthropology' (1999), and the range of comment which it attracted, illustrates the extent to which the profession

holds polarized views on this subject. Perhaps we need to look no further to understand why today's post-graduates see ethical issues ambivalently?

While Pels is probably correct about the negligible role that ethical codes have played in socializing successive generations of professionals, it should also be acknowledged that professional associations appear to be over optimistic about the approach they now endorse, namely to teach ethics via a 'case-based, ethnographically grounded, debate that is not simply about our professional practice but about our contributions to public debates about ethical principles and practice in the world'.² There are several reasons why ethics has not been constructively or adequately addressed within the discipline. First, as my undergraduate tutor used to say, anthropology constitutes a 'broad church': there are significant differences in training and orientation between individuals within and between countries (e.g. the 'four-fold' nature of training in the US contrasts sharply with other countries), not to forget the significance which sexuality, gender, politics and nationalism has for anthropological practice. Furthermore as the discipline has taken root outside of Europe new issues and orientations have emerged (e.g. Welz 2009).

Second, as the quotations demonstrate, the profession is divided over the significance that should be placed on 'ethics'. This disagreement frequently arises in retrospective assessments of the discipline which depict individual anthropologist's as being – to paraphrase a popular film – exemplars of 'good, bad and ugly' or uneasy virtue (see below)! Needless to say if anthropologists and/or anthropological practice is excoriated, then our professional associations also need to be examined. However most of the writing on this subject fails to address the concern of this paper, namely whether all forms of ethnographic research raise ethical concerns and if so, what are those concerns?

The lack of consensus in the discipline spills over into the way in which ethics is taught (i.e. along a continua between unpacking 'ethics' as a central element of ethnographic research and merely asking students to be aware of the ethical code). The unresolved issues involved are compounded by growing levels of intervention in post-graduate training in the United Kingdom by the Government and research funding councils (notably the Economic and Social Research Council or ESRC) who have dictated the length³ and content of research training for doctoral students⁴ (Spencer 2000; Mills 2003; Spencer, Jepson and Mills 2005).

The past 10 years in Britain has seen a major decline in funding for doctoral research in anthropology via a process of institutional accreditation in which funding has been reallocated to a declining number of institutions who have been deemed to meet ESRC criteria. It is against this background that UK research councils have announced the latest initiative, the 'Postgraduate Training and Development Guidelines 2009'.⁵ Under this proposal the number of 'outlets' is to be further reduced by requiring all higher education institutions – i.e. universities and institutes – to collaborate and provide joint training under the rubric of 'doctoral training units'. In short, to obtain funding for doctoral students departments of anthropology will have to become part of a larger unit – with other disciplines – to provide accredited research training. The net effect of these changes will be to sandwich training in 'ethics' together with instruction in ethnographic research methods in to a one-term course.⁶ Quite simply too little time is available to seriously

consider the issue of ethics in research (not to mention time to learn about interviewing, participant observation, etc).

In section (i) I discuss the context in which the American Anthropological Association (AAA) – the oldest professional anthropology association in the world – drew up an ethical code, and the events which triggered the Association to re-define and re-iterate that code. Section (ii) attempts to move the argument forward by looking at research in the 1990s when, as a result of 'studying-up' to examine powerful institutions, a shift in the relationship between the ethnographer and his/her subjects occurred which has allowed our research subjects the right to object to our interpretations and accounts. Section (iii) looks at an example of contemporary research to understand how this continuing shift in the relation between ethnographers and their informants/subjects has given some of our subjects considerable power over what we write, say and do. The paper concludes by drawing out lessons for the next generation of anthropologists regarding the intrinsic value of ethics in fieldwork.

i. Ethics – a view from the academy?

The American Anthropological Association (AAA), which was founded in 1902, drew up its code in 1969 in reaction to accusations by university students that anthropologists had been involved in counter-insurgency research funded by the Pentagon (under the rubric of 'Project Camelot'; Beals 1967).⁷ The creation of an ethical code emerged painfully following considerable internal controversy including the rejection of an internal inquiry into the issue by a former President, Margaret Mead.

There are two points worth noting about the AAA's decision in 1969 to create a code, and indeed its subsequent modification of that code. First the code was a direct response to events outside the discipline which were potentially damaging to the reputation of the discipline. A very similar defensive reaction occurred in 2000 in relation to the publication of an exposé of anthropological practice in the Amazon (*Darkness in Eldorado* written by Tierney)⁸ and again in 2009 with respect to the employment of anthropologists in the Pentagon's 'Human Terrain Systems' Program.⁹

Second, as Pels (1999) has argued, ethnographic research has historically depended upon a degree of 'duplexity': an ethical code was/is used to invoke the rhetoric of 'scientific truth', objectivity and/or lack of bias when in fact research is premised on an unequal 'dyadic relationship between ethnographer and people.' Pels argues that the rhetoric of ethics 'masks' the politics of ethnographic research; that it obfuscates the production and politics of knowledge in which ethnographers are more powerful than their subjects.

This raises an important question: how have anthropologists dealt with ethical issues? In a view that apparently represents the AAA's position on this subject, Wax¹⁰ has argued that until World War II anthropological concerns about ethics were largely philosophical and/or were 'constrained by the methodological ideal of the natural scientist, who was intrinsically detached from the objects of study' (1985). He goes on to say that during WWII the majority of American anthropologists redefined 'ethics' as a willingness to fight in the cause of 'the Free World' and agreed to work for the US Office of Strategic

Services (the precursor of the CIA). However the next generation of anthropologists, who did fieldwork in developing societies, adopted a more skeptical view which was reinforced by events surrounding Project Camelot and US military actions overseas. It was at this point 'the pendulum swung far in the direction where 'ethics' were defined as a refusal to have any dealings with the military side of the [US] government, or with any aspect of government that seemed to sustain an imperialistic orientation' (Wax 1985).

Wax's 'history' is, however, problematic because it presents American anthropologists and the AAA as having acted honorably in the scientific pursuit of knowledge (with the single exception occurring in World War II when national ideology proved to be a stronger call on the individual than science). However Wax also fails to consider the changing relationship between ethnographers and their subjects.¹¹

Contrary to Wax's one-sided account, it appears that anthropologists have routinely ignored or set aside concerns about professional ethics to pursue their own interests. Thus Wax omits the debate on this issue raised by the founder of the AAA, Franz Boaz, who expressed public outrage at the activities of four anthropologists who, during World War I, used the cover of their professional research to gather military intelligence. The AAA censured Boas for his statement and failed to investigate the incident (see Fluehr-Loban 2003:2-3).

Furthermore, it is clear that during WWII *the majority* of American anthropologists redefined 'ethics' to allow them to fight in the cause of 'the Free World' by working for the US Office of Strategic Services, the Federal Bureau of Investigation, the Office of War Information, and the War Relocation Authority (which interred 120,000 Japanese-American citizens; Price 2008b). During the 1950s and 1960s the CIA covertly funded a great deal of social science research using research foundations as fronts for their work; in this way anthropologists and other social scientists undertook – some unwittingly, some knowingly – research which informed CIA policies including improved methods of torture (Price 2007a & b). It is against this alternative reading of anthropological practice that anthropological complicity in Project Camelot needs to be seen (and subsequent calls by a small number of anthropologists to redefine the discipline; Gough 1968; Asad 1973).

Why, then, did the AAA take so long to define and agree on a professional code of ethics? The answer would seem to be that only in the face of a sufficiently powerful external 'threat' (to borrow a metaphor from a well known ethnography) was it possible for the discipline to set aside internal differences to unite against external threats to its reputation and authority. Its defensiveness has characterized subsequent objections to the imposition of Institutional Review Boards (IRBs) which were set up to monitor and regulate university-based research by the Association. In particular it attempted to differentiate ethnography from other forms of research by emphasizing: (1) that the professional code of ethics imposes clear obligations on anthropologists in relation to 'the people, species, and materials they study and to the people with whom they work'; (2) that ethnographic research requires researchers to cultivate 'trust among all those involved'; and (3) 'the IRBs should view informed consent'¹² in this context as an ongoing and dynamic process' (AAA 2004). However the AAAs special pleading fell on deaf ears (Bosk and De Vries 2004).

During the last forty years *professional anthropology associations* have argued that they lack the power to regulate or sanction members who infringe professional ethical codes. Instead they argue that the code of ethics serves an educative function by alerting members to potential 'ethical hazards' and that *responsibility for unethical action falls on individual members not the Association*. Has anthropological practice during the final decades of the twentieth century conformed to professional ethical standards? It is impossible to say with any certainty. It is certainly the case that the allegations which prompted the AAA to re-articulate its ethical code were made against individuals in university departments (i.e. the controversy over the Yanomami). The situation seems a little odd given the extent to which anthropologists were increasingly working outside the academy.

Chambers (1987) reminds us that in the seventies there were very few jobs for professionals in the American academy and that anthropologists increasingly sought, and were recruited for, work in development. He also tellingly notes that tenured academics have historically viewed 'applied' researchers and/or anthropologists employed outside the academy as 'lacking in intellectual rigor, ethically suspect, unimaginative, bereft of theoretical sophistication, and somehow essential to our future' (p. 309).¹³ The extent to which anthropologists were/are working for the private sector is attested to by growth of the National Association of Practicing Anthropologists.¹⁴ The situation has many parallels elsewhere. For example during the 1980s in Britain there was an absolute decline in the availability of academic jobs for new graduates; this period saw the rise of 'anthropology at home' and a movement of professionally trained anthropologists into employment outside the academy (Grillo and Rew 1984; Spencer 2000:11).¹⁵

Many British anthropologists who graduated in the 1980s 'cut' their professional teeth by working for aid agencies, NGOs etc as project managers/staff and/or as consultants. In the early 1990s British universities saw an expansion in the number of undergraduates studying anthropology which resulted in some of us being recruited into university teaching departments while many anthropologists were recruited into development studies centers at UK universities where they were required to undertake commissioned research/consultancies to underwrite their quasi-academic post.

ii. Anthropology in the 1990s: the implications of 'studying-up'?

The 1990s provides ample evidence that anthropologists accepted Nader's (1973) injunction to 'study-up' seriously.¹⁶ In this section I draw on the work of two individuals whose research on powerful development institutions illustrates how the 'dyadic' relation between ethnographers and their subjects has radically changed.

David Mosse worked briefly for the British Overseas Development Agency – the precursor of the UK Department for International Development – in the early 1980s before returning to the site of his doctoral research in southern India to work for an International NGO. Between 1990 and 2001 he worked as an 'anthropologist-consultant'¹⁷ on a development project in southern India funded by British aid. As he put it, '[B]ecause of this rare continuity, and the particular importance of this project as a 'flagship' within the 1990s British aid programme ... the DFID agreed to support a study

of the project experience from my particular anthropological perspective' (2006:938). In short, Mosse sought to draw upon his experience with, and knowledge about, the project to write an 'insider ethnography' (Mosse 2005).

He circulated chapter drafts for comment to all those who worked on the project – other consultants, project managers and field staff – and he conducted further interviews with DFID officials and consultants 'to verify my understanding of project processes, to decentre my own view, and to extend the analysis to the wider context of British aid in India' (2005:ix). The intention of circulating the manuscript to others who exercised responsibility for the project was also to learn from them by reflecting on their objections to his interpretation (2006:939).

While project and social development staff endorsed his views, a small number of UK consultants and DFID project advisers "took strong exception to my 'too negative and unbalanced' account, which was 'unfair and disrespectful', 'out of date', and even 'damming of all our work' (941). The specific interpretations offered by Mosse and the allegations made by these consultants can be found elsewhere (Mosse 2005, 2006). These consultants raised objections concerning who was qualified to construct knowledge about a project, and how that interpretation should be arrived at. They also refused to exchange written comments, and proposed instead a socially mediated discussion in which an agreed consensus would prevail. After several months Mosse wrote to these individuals to say that he was proceeding with publication. They reacted by writing to his publisher (asking that publication of the book be blocked) and they wrote, e-mailed and phoned his head of department, the Pro Director in charge of research at his university, and the Association of Social Anthropology (UK) to demand an adjudicated resolution of the dispute. Their demands now included the allegation that he had breached the ethical guidelines of the ASA because of the way he had negotiated research consent and because publication would have 'harmful effects'.

The ASA 'concluded that they had no remit to adjudicate, or act as a court, for what were only ethical *guidelines*'.¹⁸ It was said that officers of the ASA privately believed that neither the research process used by Mosse nor the outcome of his research contravened the guidelines. Consent to research had been given and, as a senior colleague put it, 'the absence of flattery is not harm' (Mosse 2006:947). However the university agreed to convene a one-day meeting in London at which it, the ASA and the two parties to the dispute would meet. The meeting was not intended as a 'court of arbitration' nor, it was emphasized, could it impose any obligations on him to make specific changes to the book.

His erstwhile research collaborators submitted 56 pages of material substantiating their claims. As Mosse commented:

While the language of defamation was sufficiently serious for me to take legal advice, and while the objections had been helpful in indicating alternative points of view or correcting certain factual errors, the more I examined these comments, the less they seemed to be matters of substance (evidence or argument) and the more they invoked the moral community. That is, their concerns were matters not

of ontology but of relational epistemology ... They were about a person not a text' (2006:948).

The day-long 'ritual of objection', which was chaired by a former DFID chief adviser, allowed both parties to have their say but ended without Mosse making any undertaking to revise the text of his book: 'In the end, I did not change my analysis, although I clarified its purpose, and modified phrasings that offended, where *I* judged this appropriate' (948, emphasis in original).

The 'improvised procedure' gave a green light for the publication of his ethnography to proceed by a 're-affirmation of the Malinowskian boundary between desk and field' (p. 948). In other words the meeting reaffirmed academic rules by recognizing the anthropologist's right to write about social life even though the issue of 'factual accuracy' or ethnographic truth was not resolved. As Mosse saw it, three issues were at stake. First

The challenge for anthropology today is not how to rearrange 'fieldwork' ... or how to re-frame writing, but how to get to grips with the changing relationship between the two: *change*, first, *in how fieldwork relations shape writing*, and, *second*, *in how writing now alters relationships of 'the field'* (936, my emphasis)

The final issue concerned whether the factual accuracy of ethnographic accounts was dependent on a careful separation of 'facts' from interpretations (aka Malinowski)? Or was it that a case study and 'discursive material' could, at best, only illustrate an argument? Following Hastrup (2004:456-f) Mosse phrased the issue in the following way:

... anthropologists can never prove the rightness of their generalizations with reference to evidence or experience ... since these are neither separate from, nor prior to, the anthropologists own frame of interpretation ... At the very least anthropologists need to examine the social basis of their own 'evidence making.' They need to examine their own 'point of view' – their personal and academic predilections, judgements, and aesthetics – as the product of social conditions ...' (949).

While it is possible to say much more about this case, it is instructive to turn to the work of an American anthropologist who was undertaking fieldwork at roughly the same time. Janine Wedel examined neo-liberal economic reforms in the 1990s and in particular at the way that the US government had delegated its authority to unaccountable non-state actors – specifically 'consultants', non-governmental organizations, private enterprises etc – to make policy, allocate funding and implement programs. She describes these organizations as 'chameleon-like entities' which perform a variety of official and semi-official functions that primarily benefited the individuals who participated in the policy process (Wedel 2001).

Wedel writes about the 'agendas' of powerful individuals involved in foreign-aid/development policy and has argued that

[A]id policies, like any policies ... involves people and institutions: people, with their own interests and cultural frameworks; institutions, grounded in culture and politics. The lack of attention paid to the agendas of real people involved in both sides of foreign aid ... has played a major role in its shortcomings' (2005:36).

She conducted research into the aid-mediated relationships established by USAID, specifically the relationship between economists at Harvard University's Institute for International Development and the 'Chubais' Clan, a close-knit group of individuals with links to the Russian state. This clique, which managed 'technical assistance' worth US\$400 million, failed to realize stated policy objectives (indeed the process caused a major abreaction to western policy in Russia) and individuals misused their positions 'to advance their own and their spouses private financial interests' to the tune of US\$31 million.¹⁹

Wedel has also written about the role of Washington 'neoconservatives' who were active in formulating and implementing the Bush Administrations policy vis-à-vis Iraq and Iran (2005, 2009a). Both cases illustrate the manner in which the US 'outsourced' responsibility for key areas of foreign policy by relying upon a small, tight-knit group of individuals who were/are positioned within and between key institutions in the US and the Middle East. These cliques operate as privileged insiders to western policy-making in respect of countries which the US sought to reform and/or re-build. With respect to Iraq, the key actors were Richard Perle and Ahmed Chalabi who created shadowy and unaccountable organizations to influence policy in Washington and Iraq after the US-led invasion in 2001. While Wedel's 'informants' do not appear to have publicly 'objected' to her conclusions, it is probably because they have exercised one of their defining characteristics, namely institutional deniability to obfuscate their role in the policies she has examined.²⁰

Both Mosse and Wedel sought to 'study through' – a term coined by Shore and Wright (2000) – to establish the policy connections between key institutions, organizations and actors.²¹ Whereas Mosse knew all the actors/institutions and had permission to write an ethnography, Wedel had to establish her credentials at each stage of her research in order to persuade individuals to speak to her. The contrast is between two very different approaches to policy research, namely ethnography produced by an 'insider' and hard-nosed research conducted by an outsider.

While in certain respects Mosse and Wedel share a similar focus, namely 'studying through' development policy, their research differs markedly, thus: (1) research objectives differ (an examination of the policy process v a close scrutiny of policy makers); (2) their approach to 'evidence making' is very different²²; finally (3) their epistemological approach differs radically. It is worth noting that unlike previous generations of anthropologists who enjoyed a relatively powerful position vis-à-vis their informants, in studying-up Mosse and Wedel's subjects voluntarily participated in their work fully knowledgeable about the research process (even if they later disagree with the interpretations/arguments of the researcher).

iii. Fieldwork in the new millennium: dealing with threats from the field?

Today anthropologists undertake fieldwork in settings marred by high levels of violence. Whether this situation is characteristic of the times or whether it reflects a decision to study a potentially dangerous subject is difficult to say. In any event violence changes the experience of fieldwork and raises new concerns about ethics, research methods and the anthropologist's relationship to their informants.

A particularly good example comes from the work of a Jamaican student I had the privilege to supervise. As with many overseas students studying in the UK, Herbert Gayle was a junior member of staff at his national university where he taught and became involved in a fairly high profile university initiative that sought to understand inner-city violence and to mediate between gangs of youth and the police. As a result of his involvement in these initiatives he built up a certain level of social capital in inner city areas which enabled him to undertake a comparative study of violence in two areas of Kingston, Jamaica, and two inner-city areas of London where Jamaican gangs operate. As he noted:

The use of violence can be made to seem necessary, even 'Justifiable', especially when it is claimed to be part of a process to achieve public order. With this knowledge, I have chosen to act upon Riches' (1986) suggestion to assess the complete performance of violence, including that carried out by the state against citizens. As a result violence is defined in this thesis as all intentional rendering of physical hurt to another person or group. The central concern of the thesis is homicide, the most severe form of violence. (2007:17-18).

Leaving aside the difficult methodological issues involved in a cross-national study of this type, I want to focus on issues of research access and the personal safety of the researcher which is exemplified by Gayle's fieldwork. In attempting to understand violence, he had to negotiate access to key actors and to locales where violence was 'performed' on a daily basis (successful entrée into communities also meant that he could easily have been subjected to the violence he sought to understand). At each fieldwork site he negotiated access with key gate-keepers – elders, gang leaders ('dons') and the police – before entering a community. While participant observation and interviewing were important research tools, he deliberately relied upon a variety of 'participatory research methods'²³ which allowed him to purposefully and publicly draw a wide cross-section of the community – including youth in gangs, young women, parents, gang leaders etc – into his research. This had the advantage of creating a group of local 'research assistants' who were paid and trained to assist him and it ensured that the wider community knew who he was and what he was doing.

As he noted, fieldwork in the context of extensive violence called for a very transparent research process. He had to ensure the anonymity of his subjects and his research site in part because it was not possible to predict the agenda or the action of 'policy makers' – or indeed of the police and politicians, as events were to show – but also because

[E]xperience has taught me that even with a high degree of transparency, trust is difficult to establish and easily broken in environments such as the inner city where people have learnt as a rule of survival not to trust outsiders. Covert research in this setting was therefore not only immoral but also illogical (81).

This meant that 'research was overt and totally negotiated' *in public*.²⁴ This approach yielded rich dividends in terms of the quality of his fieldwork material even if, at times, it generated a degree of closeness with certain informants that caused intense personal discomfort:

Yet it is equally dangerous to get too close. I was particularly uncomfortable in meeting, travelling and partying with drug-smugglers. There were times when I felt that I was allowed to learn too much. There were also times when I got too emotional. On a few occasions I found myself crying with the respondents. (ibid).

Lest his remarks be taken as a reflection of the injunction that anthropologists should build up a degree empathy with ones' subjects, it should be pointed out that on numerous occasions he was directly confronted by armed gangs who questioned his presence; that he was a witness to a gangland confrontation involving the use of automatic weapons; that he had to deal with the consequences of murder and rape which were the stock in trade of gang violence; and that in Britain he was picked up by the Metropolitan Police whilst hanging out with informants in South London.

In addition to the 'threats' that arose during fieldwork, life did not get easier after he completed his thesis and returned home. For shortly after arriving home he was interviewed on Jamaican radio and television about his research, and the two part television program was rebroadcast several times during national elections. Following these interviews he was 'asked' by a 'messenger' from one of the two main political parties to stop speaking publicly about his work (the request was accompanied by the threat that 'life would not be the same if I insisted that the country needs to change the way in which votes are sought'). His life was later threatened, and his university office was broken into and his research material including his computer, back up files and notes were taken. Ominously the police refused to investigate the incident (fortunately the material was coded and the identity of his informants was not compromised). Badly shaken by these experiences he left Jamaica to return after the elections were over.

The nature of contemporary fieldwork exposes ethnographers to a range of threats from their interlocutors and from various agents of the state who now take an interest in what we have to say and how and when we talk about our research. These threats range from verbal assaults, to pressure to self-censor and/or withdraw from public fora, to attacks on our integrity as anthropologists (from individuals/organizations who seek to repudiate our arguments and/or prevent us from speaking out; e.g. Scheper-Hughes 2010), to direct intimidation²⁵ (e.g. the threat of arrest/interrogation) and to death threats.

Conclusion

The point of acknowledging the checkered nature of unethical practice in anthropology and the changing relationship between anthropologists and their informants is to argue

that we must take a long, hard look at our profession *and* at our own practices. It will simply not do to ignore past unethical practice, to claim that our research does not raise ethical issues (perhaps our inability to see the wider picture leaves us blind or indifferent to important moral issues), nor to assert the high moral ground and condemn those who do not accept our position and arguments (*pace* Fassin 2008).

As I see it a key task is to critically examine our research with regard to three sets of ethical concerns. First, we should expect to be challenged by our subjects and by those who fund our research regarding the way we conduct fieldwork and write up our material. A central element of these challenges will be our 'research ethics', broadly construed to mean how we conducted fieldwork, as well as whether we obtained 'informed consent' from our subjects. Failure to our ethical responsibilities seriously increases the risk that our work will be seen as biased, unbalanced and unethical (it also makes it easier for our work to be vilified by powerful people). To answer such criticisms it is not enough to pay lip service to ethics as a 'code of conduct'; rather it is essential to critically assess how our research may put our informants at risk of harm. This requires careful thought, repeated explanations to informants in ways that they understand, and where relevant *public* negotiations to explain our research and obtain access. Ethnographic research requires the consent and/or trust of those we seek to understand.

Second, research may confront certain problems where the best strategy is to rethink one's approach and/or redefine one's research project. For example research on or about particularly vulnerable categories of persons – children, the elderly, the chronically ill etc – from whom informed consent cannot realistically or legally be obtained needs to be approached carefully. Access to key 'gatekeepers' and to social domains where illegal/quasi-legal activities occur also present special problems: the benefits of access, including justification for covert research, need to be carefully weighed against the potential 'costs' (which may include being forced to terminate your research and/or being hurt). It is advisable to rethink and re-design a project rather than push ahead at all costs.

Finally there are lessons which the discipline should learn from the issues raised by contemporary research. First we cannot ignore the threats that confront us today. Whether these threats arise from a rebalancing of the dyadic relation between ethnographers and our subjects or from 'globalization', it is clear that the people and institutions we seek to understand exercise considerable power. To meet this challenge we need to better prepare students to understand the place of ethics in fieldwork, which means we must rethink the way we teach ethics – this requires that we squarely address the problems inherent in undertaking fieldwork, in ensuring that no harm comes to our informants, and in the production of anthropological knowledge.

One final issue remains: does the discipline need 'an alternative code of ethics that takes power into account' (Sluka 1999:126), one which enables anthropologists to undertake research into the world of powerful individuals and organizations? Do we owe the same professional responsibilities and duties of care to *all* research participants, the poor, sponsors, funding agencies, employers, our own and host governments, and the powerful? Several anthropologists have already suggested proposals for how to take this issue forward. Wedel (2009b) has advocated a pragmatic approach by suggesting that anthropologists should borrow from the ethics of journalism when interviewing powerful

individuals by agreeing at the start of each interview whether the discussion is to be “off the record” (used to advance the researchers understanding or acquire other sources’), ‘on background’ (used without attribution) or ‘on the record’ (used with attribution)’. She argues that only in this way will it prove possible to interview powerful informants and that ‘our code of ethics must be tailored to fit the world that its practitioners encounter.’ Scheper-Hughes (2009) has taken this argument to its logical conclusion by arguing that ‘militant’ anthropologists have a duty not just to respond to public issues but to bring issues into the public domain that need to be heard. She sees this as a professional obligation. Fassin (2008) takes a more philosophical position. He notes that ‘moral discourse evaluates, judges, sanctions. Critical analysis proposes a possible intelligibility by considering the sense that words and acts have for social agents but also by inscribing them in their broad historical and political context. Moral discourse simplifies for the purpose of its cause ... whereas critical analysis renders the complexity of issues and positions’ (339). The resolution of this issue will no doubt be decided on the basis of a careful consideration of individual cases and as a result of an intense debate.

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About the Author

John Campbell is a Senior Lecturer in the Anthropology of Development, Department of Anthropology, at the School of Oriental and African Studies. He can be contacted at jc58(AT)soas.ac.uk.

Endnotes

¹ I would like to thank Robert McKenzie, Herbert Gayle and Lisa Wynn for their comments and suggestions on this paper. All omissions and mistakes are my own. In late March 2009 after I had completed this paper the ASA brought on-line a valuable new resource which looks at ethics and research, see: Social Science Research Ethics available at: <http://www.lancaster.ac.uk/researchethics/index.html>.

² Source: 'A statement on ethics from the Chairman' of the Association of Social Anthropology UK.' This can be found at: <http://www.theasa.org/ethics.htm>.

³ In the early nineties doctoral students were required to complete their degree within five years of their initial registration; recently this has been reduced to four years.

⁴ In 2001 the funding councils dictated the specific topics that were to be taught to research students to which were added a range of 'generic' skills.

⁵ This document was issued by the UK's Economic and Social Research Council and can be found on their website.

⁶ General or 'generic' research skills, which all doctoral students are expected to complete, is provided by training consortia in the form of specific lectures or short courses which run in parallel with formal training provided by departments.

⁷ Interestingly, The Society for Applied Anthropology, formed in 1941, established its ethical code in 1949. See: 'SFAA Presidents letter', in *Newsletter for the Society for Applied Anthropology* of November 2000 (11, 4: 2) available at: <http://www.sfaa.net/newsletter/nov00nl.pdf>. It is notable that SFAA included the need for informed consent in its code in 1983, where as the AAA did not add this to its code until 1998.

⁸ See: 'CA Forum: Anthropology in Public - Perspectives on Tierney's *Darkness in Eldorado*', *Current Anthropology* (42, 2, 265-76) and documents on the AAA website.

⁹ For the AAAs deliberations and recommendations about HTS, see: <http://www.aaanet.org/cmtes/commissions/CEAUSSIC/index.cfm> For a critique of anthropological engagement with HTS, see Price (2009).

¹⁰ Wax's dated paper is still listed on the website of the AAA; also see Price's (2008a) review of the AAA's representation of its history.

¹¹ During the late 1950s and early 1960s the inequality that characterized the relationship between ethnographers and their subjects was beginning to change, a process linked to economic development and growing literacy which allowed our erstwhile subjects to read our accounts and undertake research themselves (Campbell 2006). This process also contributed to the indigenization of local/national anthropologies.

¹² The AAA argued that ‘Informed consent includes three key components: communication of information, comprehension of information, and voluntary participation’. See footnote no.17 on this issue.

¹³ The ASA (UK) had historically been opposed to professionally trained anthropologists working outside the academy. This entrenched position came to a head in the early 1980s when its former President, Evans Pritchard, argued vociferously that applied subjects should not be taught. Ultimately this position was successfully opposed as the discipline slowly embraced new applied subjects and sought to prepare graduates for work outside the academy (Spencer 2000: 14).

¹⁴ See NAPAs website and information on its membership at: <http://practicinganthropology.org/practicing-anthro/>. I have been unable to find evidence indicating the extent to which professional associations have taken steps to enhance the status and protect the rights of non-academic members.

¹⁵ The extent to which anthropologists were working outside the academy was recognized in the 1980s by the establishment of a new ‘network’ which was much later absorbed into the Association of Social Anthropology (UK) as the ‘Anthropology of Britain Network’ (see: <http://www.theasa.org/networks/aob.htm>). Also in 1984 the ASA (UK) finally changed its membership criteria to allow non-academic members to join.

¹⁶ Nader’s paper is a reaction to the charge by anthropology students that the discipline lacked relevance; she argues that students use their ‘indignation’ as a motive for studying the powerful in American society.

¹⁷ By the early 1990s he was also employed as an ‘academic-consultant’ at the The Centre for Development Studies, University of Wales, Swansea; in the late 1990s he moved to the Department of Sociology and Anthropology at the School of Oriental and African Studies, London where he is today.

¹⁸ Interestingly, at about the same time the AAA was accused of ‘contravening its own policy prohibiting ethics adjudication’ in its inquiry into the charges made by Tierney (see: Gregor & Gross 2004; Jaschik 2005; Glen & Bartlett 2009).

¹⁹ See the statement by USAID: ‘Harvard Defendants Pay Over \$31 Million to Settle False Claims Act Allegations’ (3 August 2005) at: http://www.usaid.gov/press/releases/2005/pr050803_1.html.

²⁰ I understand that one individual studied by Wedel sought to use his influence to get her removed from her university post. If this was the case, it represented a direct threat to her integrity as an anthropologist and to her livelihood.

²¹ It is interesting to reflect on their very different takes on key individuals as ‘brokers’, a term with a long pedigree in anthropology.

²² One way of characterizing this difference might be to say that they place differing weight upon the opinions expressed by their interlocutors, and to secondary sources of data including material that might be subject to numeric/statistical analysis and/or ‘off-the record’ briefings where sources cannot be attributed/identified.

²³ These tools were popularized by the Participatory Learning and Action (PLA) Network to facilitate community-based analysis of development needs.

²⁴ The model of verbally agreed informed consent which emerged approximates to the AAA 2004 statement.

²⁵ As Anne de Jong (2010) indicated in a recent seminar, the polarization of politics in Israel is such as to lead to systematic state campaigns against intellectuals and researchers

as well as the use of violence to intimidate individuals at peace rallies. See also: 'The New McCarthyism sweeping Israel', *The Independent*, (Donald Macintyre, 13 February 2010).