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Confronting Anthropological Ethics: Ethnographic Lessons from Central America*

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The concern with ethics in North American cultural anthropology discourages political economy research on unequal power relations and other 'dangerous' subjects. US anthropologists define ethics in narrow, largely methodological terms – informed consent, respect for traditional institutions, responsibility to future researchers, legal approval by host nations, and so on. The responsibility of the researcher to uphold 'human rights' or to document political repression and suffering is not merely dismissed by mainstream anthropology as a partisan issue outside the realm of scholarship, but is actually condemned as ethically problematic. The growing postmodernist deconstructivist approach within US anthropology allows ethnographers to obey their discipline's narrow ethical dictates through a reflexive investigation of the hermeneutics of signs and symbols devoid of political economic social context. Drawing on his fieldwork experiences in Central America, the author argues that anthropologists have a historical responsibility to address larger moral issues because their discipline's traditional research subjects – exotic others in remote Third World settings – are violently being incorporated into the world economy in a traumatic manner that often includes starvation, political repression, or even genocide. Meanwhile, in the name of ethics, North American anthropologists continue to ignore or avoid the human tragedies engulfing their 'research subjects'.

1. *North American Cultural Anthropology*

The ethics of anthropological research are too complicated and important to be reduced to unambiguous absolutes or even perhaps to be clearly defined. The human tragedy and political dilemmas I encountered in my ethnographic fieldwork in Central America obliged me to confront the inadequacy and internal contradictions of current anthropological definitions of ethics in research. In this article I will be referring to cultural anth-

ropology primarily as it is practiced by North American scholars researching the Third World. These ethical quandaries arise within the epistemological tension imposed by the US intellectual tradition of allegedly apolitical liberal relativism in opposition to engaged universalism. In Europe, Latin America, and elsewhere this intellectual dichotomy between science and politics is not as clearly drawn. Indeed, throughout the rest of the world, engaged scholarly analysis is not only legitimate but part of the researchers 'social responsibility'. I am not proposing a systematic political economy of North American anthropological knowledge, but I do feel that the framing of ethical issues in cultural anthropology needs to be understood in the context of the history of the development of the discipline in the larger society. The eminently political orientation of a supposed apolitical commitment to empirical research must be appreciated for its internal inconsistencies and ultimate ethical poverty. Finally, the emergence of the contemporary 'postmodernist deconstructivist' focus on 'culture as text' within symbolic anthropology as the dominant theoretical tendency among US anthropologists needs

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to be placed in the problematic context of anthropological ethics in a politically polarized world.

In the late 1960s and early 1970s North American social scientists began discussing the ethical dilemmas faced by fieldworkers studying and living in a world rife with political turmoil. Several important edited volumes in anthropology were produced on the subject (cf. Weaver, 1973; Hymes, 1972) and major journals devoted considerable space to earnest – and at times polemical – debates by important figures in the discipline (cf. *Current Anthropology* (1968), vol. 9 (5), pp. 391–435; (1971), vol. 12 (3), pp. 321–356). In an important early volume a dozen anthropologists from around the world questioned the historical relationship between the development of the discipline in a functionalist theoretical framework in Great Britain and the political and economic realities of British Colonial domination and indirect rule in Africa and elsewhere (Asad, 1973). This critical reappraisal of the roots of the discipline was even prominently incorporated in a major cultural anthropology textbook in 1981 (Keesing, 1981, pp. 481–499). Respected anthropologists in North America have denounced the conscious and unconscious collaboration of anthropology with the counterinsurgency agencies of the US government – specifically Project Camelot in Latin America (Horowitz, 1967), Project Agile in Thailand (Gough, 1973; Jones, 1971; Wolf & Jorgensen, 1970), and the Himalayan Border Countries Project in India (Berreman, 1973). By this time, however, the US Defense Department had already successfully tapped anthropological expertise to refine counterinsurgency strategies in Indochina. The US military even started an ‘Ethnographic Study Series’ and published a volume *Minority Groups in North Vietnam* which was ‘... designed to be useful to military and other personnel who need a convenient compilation of basic facts about the social, economic, and political institutions and practices of minority groups in North Vietnam’ (Kensington Office of the American Institutes for Research, 1972).¹

The at times polemical debates of the late 1960s and early 1970s have injected an im-

portant self-consciousness among US anthropologists researching far from home. We have come a long way from our European forebears (especially the British) who flew into colonial war zones under the auspices of colonial offices to interview ‘natives’ and write ‘how-to-administer-more-humanely’ reports for government bureaucracies intent on increasing ‘administrative’ efficiency and lowering costs. Today, few self-respecting anthropologists would condone the exercise of anthropology at the service of a world superpower or as a complement to espionage. Most ethnographers now include a discussion of the methodological and personal ethical dilemmas they faced during their fieldwork.

2. *The Discipline's Narrow Definition of Ethics*

Let us examine the more commonly cited of these ethical dilemmas: we worry about whether or not our research subjects have truly consented in an ‘informed’ manner to our study; we ponder over the honesty of our presentation of self; we condemn the distortion in the local economy caused by the resources we inject into it in the form of ‘informants’ gifts or wages; we are wary of the social disapproval foisted on our primary informants when they become the objects of envy or ridicule from the rest of the community because of the resources, prestige, or shame we heap upon them; we no longer steal ceremonial secrets unapologetically; we examine our emotions introspectively to watch for glints of ethnocentrism; we struggle to uphold cultural relativism and to avoid unconsciously conveying disrespect for traditional institutions and values through our lifestyle; we studiously preserve the anonymity of our research subjects and host communities; we feel guilty for violating the privacy of our informants and their culture; we worry about ‘scientific colonialism’ and our ‘responsibility to the host community’ so we send extra copies of our publications to our research site; we might even read translated versions of our publications out loud to our host families and friends if they are illiterate; we do not take photographs indiscri-

minately and we do not tape record without obtaining prior permission; we discuss the pros and cons of consulting forbidden archives or quoting from personal diaries and letters; we question the ethics of accepting financial support from governments and politically biased institutions; we worry about the potential misuse of our research material once it has been published in the public domain; and finally we take care not to jeopardize the access of future colleagues to our fieldwork site by our actions and publications.

These are indeed all vitally important ethical issues that we must all confront during fieldwork and write-up. But why does the anthropological concern with ethics stop here? What about the larger moral and human dimensions of the political and economic structures which are ravaging most of the peoples that anthropologists have studied historically? With notable exceptions most North American anthropologists do not include the political and even human rights dimension confronting the people they research in their discussion of 'anthropological ethics'. In fact the dominant trend has been to avoid these issues by a theoretical focus on the meaning of signs and symbols outside of social context.

The problem with contemporary anthropological ethics is not merely that the boundaries of what is defined as ethical are too narrowly drawn, but more importantly, that ethics can be subject to rigid, righteous interpretations which place them at loggerheads with overarching human rights concerns. How does one investigate power relations and fulfill the researcher's obligation to obtain informed consent from the powerful? What about the right to privacy of absentee landlords as a social group? It is much more difficult – if not impossible – to satisfy the discipline-bound anthropological/methodological code of ethics if we attempt to research marginalization and oppression, than if we focus on the philosophical aesthetics of cosmology. Can we address the urgent problems faced by our research subjects and still obey our discipline's interpretation of methodological ethics?

3. *A Moral Imperative to Anthropology*

The simple solution so often adopted by anthropologists is to avoid examining unequal power relationships – and to orient their theoretical interests towards safer, more traditionally exotic focuses. In the late 1960s Eric Wolf (1972 [1969], p. 261) admonished anthropology to avoid a '... descent into triviality and irrelevance' by focusing on large-scale '... problems of power.' More recently Roger Keesing (1987) has reiterated the gist of Wolf's 'moral' auto-critique of the discipline by calling on symbolic anthropologists to situate analyses of the hermeneutics of signs and symbols systematically in the political economy of power relations. He goes further, arguing that it is scientifically necessary to place interpretations of 'culture as text' in the context of the real world in order to counter the tendency to impose articulate but intensely subjective and 'exotic' interpretations of religion, myth, and cosmology on the people we study.

A logistical imperative could also be advanced for why cultural anthropologists might want to assign priority to an analysis of power inequalities in their research. Unlike philosophers, literary critics, or art historians we usually study living human beings. Furthermore, we differentiate ourselves methodologically from other social science and humanities disciplines which also study humans through our technique of participant/observation fieldwork. We are not allowed to remain at our desks to pore over census tracts; we have to venture into the 'real world' not just to 'interview' people but to actually participate in their daily life and to partake of their social and cultural reality. In the Third World, therefore, fieldwork offers a privileged arena for intensive contact with politically imposed human tragedy. Perhaps this methodological obligation to be participant/observers could inject a humanistic praxis into our research? Does social responsibility have to contradict our discipline's commitment to cultural relativism?

A moral argument for theoretical compassion does not stop at methodological praxis. We also have a historical responsibility to the particular types of research subjects selected by our forbears. Historically our discipline

differentiated itself from sociology and other social sciences by focusing on the 'distinctive other' (Hymes, 1972, p. 31). Since our inception we have had what Keesing (1987, p. 161) calls a 'predilection for the exotic' and what Sidney Mintz (1970, p. 14) criticized as a 'preoccupation with purity'. We are most famous for having trekked deepest into the remotest corners of colonial territories to try to find people outside the reach of 'civilization'. We have unabashedly worshipped 'the traditional' – so long as it is in a pristine vacuum. Over the past two decades we have begun to remedy our ahistorical, disarticulated focus on the particular. Ethnographies are increasingly situating our research in regional contexts. In fact, as Carol Smith (1984) notes in an article on the Maya in the Western Highlands of Guatemala, it has almost become fashionable for anthropologists to bemoan the myopic-community-study-in-a-vacuum focus of traditional anthropology.

Even when we succeed in finding a particularly remote cultural cranny where a 'traditional' people has had only minimal contact with the outside world we can safely predict that these noble folk will sooner or later be sucked into the world economy in a traumatized manner. There is a good chance that their land and subsistence base will be stolen; their efforts at resistance will be met with violence, sometimes genocide; their entrance into the labor market will be in the most vulnerable niche; if they are hired by a multinational agro-export company – as they so often are – they will be systematically assigned to the labor gangs that spray venomous pesticides; if they work for a transnational subsidiary exploiting mineral resources – as they so often do – they will be sent to the bottom of the shafts to contract black lung – or worse. If they manage to maintain their ancestral lands, when they finally start to bring their produce to markets they will be obliged to sell at below subsistence prices; when they come into contact with the dominant ethnic groups and classes of their nation they will be ridiculed. In other words, with few exceptions, the traditional, noble, and 'exotic' subjects of anthropology have today emerged as the most malnour-

ished, politically repressed, economically exploited humans on earth. As a rule of thumb, the deeper, more traditional, and more 'isolated' the people our forebears studied, the more traumatized their lifeways have become today.

Given that there is virtually no such thing as a traditional people disconnected from the outside world, then our 'traditional' fieldwork sites should grant us privileged access to the massive sector of humanity pinned into the world economy's most vulnerable nexus. We have chosen to study the wretched of the earth. These are the individuals too often condemned to periodic famines, to below subsistence-level incorporation in flooded labor markets, to relocation, dislocation, or more simply extermination. Many of our discipline's former research subjects are fighting back in organized political movements; but as the Central American experience demonstrates, their struggles are prolonged, bloody, and often unsuccessful. Although as uninvited outsiders it might be naive and arrogant for us to think we have anything definitive to offer, we can still recognize the ethical challenge. Why do we avoid it?

In the early 1980s dissertations were written on the hermeneutics of shame among the Maya. But how can we understand the meaning of that important cultural construct if we ignore the tens of thousands of Maya massacred by the military at the same time, or the hundreds of thousands who migrate each year to harvest cotton, sugar cane, and coffee. Even if there were no urgent human rights imperatives as in the case of the Maya; even if there was no extreme economic exploitation and subsistence dislocation; there is at least a scientific imperative to situate their 'webs of significance' in the context of what they are really doing every day.

4. *Compassion for the Fourth World – Only*
The journals and books that regularly denounce ethnocide and genocide published by indigenous rights organizations – such as the IWGIA in Copenhagen, Survival International in England and France, or Cultural Survival in Cambridge, Massachusetts – are

a welcome exception to the tendency for anthropologists to escape a human rights mandate. Significantly, however, often these organizations tend to legitimize their militance by purposefully narrowing their focus in the classically anthropological manner – in pursuit of the ‘noble savage’. They prefer to denounce genocide when it also entails ethnocide.

In Central America this theoretical orientation is referred to as *indigenista* or ‘fourth worldist’. Amerindian culture is seen in a manichean manner as the human ideal while Hispanic culture is treated as irrelevant at best. This *indigenista* tendency is most prevalent among North American anthropologists, and one can recognize its intellectual roots in the discipline’s traditional focus on exotic, isolated community studies.

Although guided by a moral vision to denounce human rights abuses, fourth worldists tend to ignore international geopolitical contexts because of their geographically and culturally reductionist theoretical focus. This leads to arbitrary compassion; for example, I published a brief account of the poisoning of Guaymi Indian banana workers who spray pesticides for the United Fruit Company in a special issue of a French fourth worldist journal documenting the human rights violations of indigenous peoples in Central America (Bourgois, 1986). The editors would not have been interested in the article had the poisoned sprayers been Hispanic mestizoes rather than Amerindians. In fact they decided not to publish anything on massacres in the Salvadoran countryside because the peasants being killed were not Amerindians. Ironically, only two generations ago most of the grandparents of these ‘Hispanic’ Salvadoran rural dwellers currently being massacred were Pipils. They were forced to abandon their traditional language, dress, and indigenous culture when the government began systematically killing all indigenous peoples – between 18,000 and 30,000 individuals were massacred – following an Amerindian rebellion in 1932.

Fourth worldists provide vitally needed documentation of tragic human rights violations but they often fail to make common cause with human beings. They discriminate

according to ethnicity, reproducing the traditional anthropological focus on the ‘exotic other’ in a vacuum. This obscures their theoretical understanding of the structural roots of repression and exploitation by framing it exclusively in manichean culturalist terms.

5. Fieldwork in Central America

5.1 *At War in Nicaragua's Moskitia*

Let me document this critique in a classically anthropological manner – by drawing on my own fieldwork experience. My first stop in pursuit of a dissertation topic was Nicaragua in 1979, just after the overthrow of Somoza by the Sandinistas (Bourgois, 1981). Like a good anthropologist, I went as far away from the capital city as possible into the Moskitia, the most remote corner of the only province where an indigenous population – the Miskitu – were said to have maintained organically their non-Hispanic culture. Their language, religious system, cultural identity, structure of land tenure, etc., were indeed distinct from the national Hispanic mainstream. Of course the historical record reveals that there is nothing ‘traditional’ or isolated about Amerindian culture in the Moskitia. The Miskitu emerged as a people distinct from their Sumu Amerindian neighbors in the 1600s through the colonial confrontation of the two great superpowers of the time – Spain and England. They allied themselves with British pirates – and later with Her Majesty herself – to fight off the Spanish conquerors. In the process they became the first indigenous people to obtain firearms. This enabled them to conquer all their aboriginal neighbors. They became warriors and economic middlemen selling Amerindian slaves and smuggled trade goods from the Central American mainland to British settlers in the Caribbean. Some of this historical legacy has been frozen linguistically – one-third of the words in their ‘traditional’ language bear a relationship to English (Holm, 1978).

Soon after I arrived in the heartland of Miskitu territory, the indigenous population began mobilizing to defend their rights to

land and autonomy in a tragic alliance with the Central Intelligence Agency. 'My' fieldwork village, accessible only by a full day's journey upstream in a dug-out canoe, became the central arena of a bloody conflict against the central government. Although the underlying causes for this indigenous war were the historical structures of racism and marginalization of the region dating back to the colonial period, the fighting itself was sponsored economically and was escalated militarily by the US government.

My theoretical training in anthropological approaches to political economy and history prepared me to deal with understanding who the Miskitu were – why there was nothing 'traditional' about them – and why they might rise up in arms against their central government. I was completely unprepared, however, for what to do on the more important practical human level. Should I publish my material or would CIA analysts perusing academic journals seize upon my information to refine counterinsurgency operations the way monographs by unsuspecting – and not so unsuspecting – anthropologists working in Indochina were abused in Southeast Asia during the Vietnam War? (cf. discussion by Jones (1971) and the preface to *Condominas* (1977)). When I discussed these issues at professional societies in the US context I was 'being political' or I was 'outside the realm of anthropology'.² If I went to the media I was by definition no longer an academic researcher – or worse yet – I was a political activist posing as an anthropologist. If we are to be logically consistent to our discipline's position on honesty of self-presentation, should we punish the closet human rights activist as firmly as we condemn the counterrevolutionary spy?

5.2 *Peasant Massacre in El Salvador*

My next aborted fieldwork experience proved to be even more painful and even more 'political'. Having been a participant/observer among a people who went through an extraordinarily rapid political mobilization, I decided to leave ethnicity aside and to investigate the broader theoretical relationship (potentially encompassing ethnicity) between ideology and material reality in the

context of radical political mobilization. A rigorous debate on peasant revolutions exists in the literature, but relatively few researchers talk to revolutionary peasants let alone live and work with them. Anthropology's tradition of participant/observation fieldwork encouraged me to try to live among radicalized peasants rather than merely to examine their vital statistics from the vantage point of historical archives or census tract statistics.

With this in mind I went to explore the possibility of fieldwork in a Salvadoran refugee camp in Honduras (Bourgois, 1982). My central ethical concern was that counterinsurgency experts would have access to my eventual publications and that I might unwittingly contribute to more efficient repression in the long run. I was also concerned lest I attract attention to refugee political leaders merely by being seen talking to them regularly. Because of these problems I initially cancelled my fieldwork plans, but on second thoughts I decided it was too important a human issue to abandon without at least a preliminary feasibility investigation.

In my exploratory visits to the refugee camps I was surprised to learn that the refugees desperately wanted foreigners to reside in the camps with them. They sought out my company because a foreign witness deters local military officials from engaging in random abuses. They assured me that far from placing them in danger, my physical presence granted them a measure of security. The church and United Nations organizations operating the camps were also interested in having an anthropologist present on a long-term basis. They pointed out that, as a full-time researcher, I would also be in an ideal position to document human rights abuses and to help receive civilians continuing to flee government search and destroy operations just across the border. In fact, all the human rights workers I spoke with urged me to stay and undertake my study in the camps.

This did not remedy the problem of the potential misuse of my published research. Several refugees suggested I cross the border into El Salvador and discuss this complicated issue with the fighters and sympathizers who

remained in their home communities. (I think the refugees also hoped that a brief visit on my part would end my repeated uninformed questions on such obvious facts as the distance between their houses or the fertility of their fields.) Although CIA analysts probably collect most theoretical studies on peasant politicization in Central America, I thought one manner of reducing the practical counterinsurgency value of such research would be to delay publication – aside from periodic human rights reports – until the political and military situation had changed sufficiently to limit the applicability of my data. The theoretical questions I would be exploring were already a part of the rigorous scholarly debate on revolutionary peasants in the social sciences. Was that entire debate to be eliminated from social science research for fear of raising the analytic capabilities of the CIA?

To abbreviate a long story, a few days after my arrival, while I was still debating this issue, a group of peasants planning to cross into El Salvador a few hours later offered to let me accompany them. I impetuously – in retrospect unwisely – jumped at the opportunity. My intent was to stay in El Salvador for only 48 hours. I thought conversations with peasants and fighters in the war zone would help me come to a final decision as to whether or not extended fieldwork in the refugee camps in Honduras was feasible and – more importantly – ethically defensible.

My 48-hour visit to El Salvador was prolonged into a fourteen-day nightmare when the Salvadoran military launched a search and destroy operation against the region. The government forces surrounded a 40 square kilometer region (approximately a dozen hamlets) and began systematically bombarding, mortaring, and strafing the entire zone with airplanes, Huey helicopters, and artillery. There were approximately a thousand peasants living in the area and only one or two hundred of these had guns and probably less than a dozen were formal members of the FMLN. The population was composed of a typical cross-section of peasants – the kind of people you would find anywhere in rural Latin America if you circled off 40 square kilometers: grandmothers,

grandfathers, young and middle-aged men and women, pregnant mothers, suckling infants, children, and so on . . . We were all the target of the Salvadoran air force and army. I gave the following oral account to a journalist shortly after my return to the US:

When the bombardments and strafings began we would take cover anywhere we could. I was told to crouch beside a tree trunk and, whatever I did, not to move. They'd shoot at anything that moved. I remember inching around a tree trunk to keep something solid between me and the machine-gun fire of the helicopters.

Sometimes the mortar shots came 10 times in a row, and there's a tremendous sense of panic when you hear them getting closer and closer. I was told that when I heard a mortar fired I should grit my teeth and keep my mouth open to prevent my ear drums from rupturing. . . . On the first four days, . . . about 15 men, women and children . . . were wounded. Shrapnel was removed, and amputations were performed with absolutely no pain medicine (*Washington Post*, 14 February 1982, pp. C1).

On the fourth night of the invasion we tried to break through the government troops encircling us. The plan was for the FMLN fighters (i.e. younger peasants with guns and minimal military training) to draw fire from a machine gun nest set up by the government soldiers on a knoll, while the rest of us civilians tried to run by unseen in the darkness of the night. Once again there were about a thousand of us of all ages, several pregnant, others sick, one blind, and many under three years of age:

We were on a rocky path with a Salvadoran gun-post off to our left. FMLN guerrillas, also on our left and to the rear, drew fire while we made a break for it. The babies the women were carrying were shrieking at the noise and, as soon as we got within earshot, the Salvadoran forces turned their fire on us.

At this point, it was pandemonium. Grenades were landing around us; machine guns were firing; we were running. A little boy about 20 yards ahead of me was blown in half when a grenade landed on him. His body lay in the middle of the path, so I had to run over it to escape (*Washington Post*, 14 February 1982, pp. C1).

I remember at one point being crouched near a woman under cover of some bushes when her baby began to cry. She waved at me with her hand and whispered to me to run away as fast as possible before the government soldiers heard the noise. I obeyed, and sprinting

forward I heard machine gun bullets and shrieks all around me. Mothers and infants made up the bulk of the casualties that night. Only a mother can carry her baby under fire because only a mother has a chance of preventing her suckling infant from crying. The Salvadoran military was shooting in the darkness into the sound of crying babies.

Six to seven hundred of us managed to sprint past the machine gun nest. For the next fourteen days, we stayed together, running at night and hiding during the day:

One of the major hazards we always faced was the noise of crying babies and the moans of the wounded, making the whole group vulnerable to detection. Rags were stuffed in the mouths of the wounded, so their cries would not be heard. The babies cried a lot because they were hungry; their mothers' milk had dried up.

A young woman gave birth on the second night of our flight. She was up and running for her life the next day, along with the rest of us. Those of us who were young and healthy were lucky. It was the law of survival at its cruelest: the slow runners and the elderly were killed (*Washington Post*, 14 February 1982, pp. C1).

At one point we crossed back through the villages we had fled from:

... we were hit with the overpowering stench of decaying bodies. There were donkeys, pigs, horses, chickens – all dead. The soldiers had burned down as many of the houses as they could, ripped apart the granaries. It even looked as if they had tried to trample the fields (*Washington Post*, 14 February 1982, pp. C1).

... [We] came upon the naked body of a middle-aged woman. Her clothes had been ripped off and apparently acid had been poured on her skin because it was bubbling off. The body had been left in a prominent position along the path, presumably to terrorize any survivors (Bourgois, 1982, p. 21).

5.3 *The Academic Reaction*

That was the end of my fieldwork on ideology and material reality among revolutionary peasants. It was also almost the end of my anthropological career, after I sought out the media and human rights lobbyists on Capitol Hill to present my testimony to the public. I had violated several of the anthropological/methodological ethics discussed earlier along with the specific duties of a graduate student to keep his/her academic advisors informed

of a change in research plans. A strong argument was made to terminate me as a graduate student – and with abundant justification according to the anthropological ethics I had broken: I had crossed a border illegally, thereby violating the laws of my host country government, I had not notified my dissertation committee of my decision to explore a new dangerous research site; I had notified the media and contacted human rights organizations, thereby violating the right to privacy of my research subjects; I had potentially jeopardized the future opportunities of colleagues to research in Honduras and El Salvador by breaking immigration laws and calling attention to government repression in public forums.

Significantly, had I not gone to the media with my testimony of human rights violations, anthropological ethics would not have been violated in as serious a manner. It would have remained a personal story between my committee and myself as an unsuccessful and reckless preliminary fieldwork exploration that had been decided against as too dangerous. By remaining silent I would not have violated anyone's rights to privacy nor have threatened my colleagues' access to the field, nor offended my host country government.

Of course my personal sense of moral responsibility obliged me to provide public testimony and I entered the media/political arena. An anthropologist is presumably not supposed to document human rights violations if this would involve violating a host country government's laws or contravene the informed consent and right to privacy of the parties involved. In other words, anthropology's ethics can be interpreted at loggerheads with humanity's common sense. I could have crossed into FMLN territory as a journalist or as a human rights activist but not as an anthropologist, because access to the information I was seeking was only available by crossing a border illegally: publicizing that information also violated a people's right to privacy and informed consent. Subsequently, lobbying to change US foreign policy exacerbates these transgressions, since political denunciation is not conduct befitting an anthropologist.

To reiterate, the problem is rooted in a specifically North American epistemology of relativism and 'value-free science' which forbids engaged research and – when taken to its logical conclusion – denies absolute assertions including those of universal human rights. This alleged 'apolitical' orientation expresses itself within US academia in a phobic relationship to the media and in a righteous condemnation of 'political activism'. In contrast to Europe – especially France – where political militance and an occasional *op.ed.* in *Le Monde* is a sign of academic prestige (Bourdieu, 1984), in the US, newspaper editorials and magazine articles are often interpreted as an indication of lack of serious commitment to science. While we do have to be cautious of sacrificing analytical rigor by becoming too immersed in media presentations and political polemics are we supposed to keep our human rights denunciations out of the public domain in the name of anthropological ethics and scientific rigor?³

It is important that the discipline enforces the tenets of informed consent and respect for host country governments. Taken out of context, however, these academic requisites obscure the political and economic realities of the regions where we have traditionally been most active. A research project which investigates structures of inequality will have a hard time passing a human subject's review board if the canons of anthropological ethics are rigidly applied. Are we supposed to abandon controversial research? Most political economy studies can be defined as potentially unethical. A fieldworker cannot obtain important information on unequal power relations by strictly obeying the power structure's rules and laws (cf. Nader, 1972, pp. 303ff.). How does one obtain meaningful information on peasant/landlord relations if the landlord is required to provide truly informed consent? What are the limits to 'informed consent' in settings of highly unequal power relations? Do we have to notify absentee landlords prior to interviewing sharecroppers on their estates? Are we allowed to obtain jobs in factories in order to document union repression? Did I have an obligation to obtain informed consent from

the Salvadoran government troops firing at us before photographing the children they wounded? Why not? And where does one draw the line? Does one abandon urgent research simply because a dictatorial host nation government does not want its repressive political system to be documented? How does one decide whether a host country government is sufficiently repressive to warrant breaking its laws? These unresolved questions reveal that there is nothing apolitical about the North American commitment to relativism and to its methodologically defined body of ethics. Most dramatically, the ethic of informed consent as it is interpreted by human subject review boards at North American universities implicitly reinforces the political status quo. Understood in a real world context, the entire logic of anthropology's ethics is premised on a highly political assertion that unequal power relations are not particularly relevant to our research.

5.4 *Informed Consent: United Fruit Company Versus Banana Workers*

For my final dissertation fieldwork project, I purposefully selected a host country which was free of civil-political strife; nevertheless, the same ethical contradictions arose. I chose to study ethnic relations on a United Fruit Company banana plantation in Costa Rica on the Panamanian border (Bourgeois, 1988, 1989). My first problem was that the transnational corporation had redefined the border, and the plantation's operations illegally straddled Panama and Costa Rica. My real host country 'government', therefore, turned out to be the United Fruit Company – not Costa Rica or Panama. High level United Fruit Company officials considered my topic – 'a history of the ethnicity of the population in the plantation region' – innocuously 'anthropological' and ordered local plantation officials to graciously open their confidential files to me. I was even allowed to reside in workers' barracks for some 16 months. Had management's consent been truly informed and had the Company understood what a historical analysis of ethnicity in a plantation context would reveal, I would obviously not have been allowed to document systemati-

cally the transnational's quasi-apartheid labor hierarchy, its ethnic discrimination on occupational safety issues; or its destruction of the union movement by ethnic recruitment, and so on. The head managers would not have toured me through their golf course, drunk whiskey with me, and made racist comments about their workers to me if they had really understood anthropological participant/observation research technique. Although I was never overtly dishonest; and although I always precisely explained my research topic to everyone; they obviously did not understand my research implications or they would have run me out of the area and/or beaten me up.

In fact, participant/observation fieldwork by its very definition dangerously stretches the anthropological ethic of informed consent. We obviously have an obligation to let the people we are researching know that they are being studied and that a book and/or articles will eventually be written about them. Furthermore, we have to explain as precisely as possible the focus of our study. At the same time, we are taught in our courses preparatory to fieldwork that the gifted researcher must break the boundaries between outsider and insider. We are supposed to 'build rapport' and develop such a level of trust and acceptance in our host societies that we do not distort social interaction. Anything less leads to the collection of skewed or superficial data. How can we reconcile effective participant/observation with truly 'informed consent'? Is rapport-building not just another way of saying 'encourage people to forget that you are constantly observing them and registering everything they are saying and doing'? Technically, to maintain truly informed consent we should interrupt controversial conversations and activities to re-announce our presence and to make sure everyone is aware of the implications of what they are saying or doing. A fieldworker cannot begin every in-depth conversation with old-time informants who have become friends by reminding them that the issues raised in their discussion may be eventually written up. If we recited to our informants their rights to privacy and informed consent – like police officers arrest-

ing a suspect – every time we spoke with them we would make terrible fieldworkers.

Experienced fieldworkers usually advise novice ethnographers not to take notes in public while undertaking fieldwork. Is that not a false representation of self? Is participation/observation fieldwork inconsistent with anthropological ethics? Where do we draw the line? Once again, these important ethical dilemmas become even more pronounced when we are focusing on conflict and unequal power relations, i.e., dangerous issues. Management's informed consent and right to privacy were not the only anthropological fieldwork ethics I stretched in my research on the banana plantation. I would frequently accompany management-level informants through their daily routine. Consequently, sitting by their side in air-conditioned Company pick-up trucks I illegally crossed the border between Panama and Costa Rica on an almost daily basis.⁴ Should I have not relied upon management employees as primary informants merely because their illegal activities would oblige me to break immigration laws? Are we allowed to research illegal operations? Do we systematically have to avoid frequenting the rich and powerful who regularly bend and break laws?

6. *Theoretical Context*

Having raised these issues in a somewhat moralistic and righteous tone, let me hasten to add that I am not arguing that anthropologists necessarily have to be human rights activists and political cadre for 'worthy' causes in order to remain ethical persons. Although perhaps another – arguably more consistent – way of reformulating anthropological ethics would be to require that our studies among the 'poor and powerless' contribute to their empowerment. That would certainly be different from the current practice of requiring 'ethical researchers' to obtain the informed consent of landlords and military bureaucracies. Nevertheless, this discussion of our human responsibility to our research subjects does not imply that we automatically have something concrete to offer in their struggles for survival or for political

rights. We are outsiders: and we have a formidable capacity unwittingly by our mere presence to cause trouble or to seriously complicate matters. For these reasons, anthropologists are finding it much more comfortable to pursue studies which do not situate signs and symbols in their invariably dangerous political and economic contexts. By focusing exclusively on celestialized meaning, we can rest assured that absentee landlords will not unleash the secret police on our informants; future colleagues will not be prevented by host governments from entering the country because of our controversial publications; and we will not politically embarrass our home institutions.

I also want to state specifically that I am not sarcastically implying that interpretive studies within symbolic anthropology are an 'immoral political cop-out' or that the post-modernist deconstructivist movement within anthropology does not have important and potentially emancipating insights to offer us. On the contrary, understanding the dialectic of power relations – even if we understand power to be rooted in a labor process and a history of class confrontations – requires a 'symbolic approach' as much as a 'materialist one'. There should not be anything incompatible between symbolic anthropology and political economy. There have been many important exploratory articles dealing from a symbolic perspective with the most 'dangerous' of all subjects – the meaning of violence and political repression from a symbolic perspective (cf. Falla, 1983; Taussig, 1984). Symbolic studies of all kinds are important for the vitality of anthropology just as are literary and artistic criticism, folklore, and philosophy for understanding the most important dimensions of humanity. At the same time, in our pursuit of science let us not forget that we are usually studying the starving and the persecuted. Moreover, let us not flee from human concerns in the name of anthropological ethics. Let us not be political when we claim to be apolitical or apolitical in the name of ethics. Let us not support repressive status quo's in pursuit of vigorously introspective deconstructions of meaning. We have to confront the serious ethical dilemmas without retreating from the theor-

etical approaches which make dangerous subjects the central thrust of our investigations. Most importantly, we should not systematically forbid fieldwork on exploitation, oppression, and power in the name of anthropological ethics.

It would be dangerous and arrogant to think that there are definite answers to any of these ethical/moral questions. We need to discuss them and think about them in both practical and theoretical terms. Meanwhile, however, as all of us (without exception) wallow in a phenomenological swamp of signs and symbols we should not forget that our 'informants' continue to be crucified.

NOTES

1. The chapter on the 'Meo' in the military's ethnographic study series on North Vietnamese minority groups specifically notes that they 'make excellent guides' and 'reliable porters, who can carry heavy loads (up to 50 kilograms) while maintaining a rapid gait' (Kensington Office of the American Institutes for Research, 1972, p. 239).
2. A politically conservative non-anthropologist outside member of my qualifying examination protested my academic training to my professors saying something to the effect that 'anthropology is supposed to be about ritual and religion – not politics'.
3. A church-based director of a human rights organization rebuked me when I explained to him the anthropological ethics which prevented me from showing a member of the US Congress photographs of peasant victims during my testimony on the military invasion: 'For God's sake what are you talking about! Testify as a human being then – not as an anthropologist.'
4. In my anthropological obsession to satisfy my legal obligations to my host country government I tried to obtain a special permit from the local immigration official at the border crossing on the plantation. The public official laughed at me in disbelief telling me that so long as it was OK with the Company he had no objection to my repeated frontier trespasses.

REFERENCES

- Asad, Talal, ed., 1973. *Anthropology and the Colonial Encounter*. New York: Humanities Press.
- Berremen, Gerald, 1973. 'Academic Colonialism: Not so Innocent Abroad', pp. 152–156 in Weaver, 1973.
- Bourdieu, Pierre, 1984. *Homo Academicus*, Paris: Minuit.
- Bourgeois, Philippe, 1981. 'Class, Ethnicity and the State Among the Miskitu Amerindians of Northeastern Nicaragua', *Latin American Perspectives*, vol. 8, no. 2, Spring, pp. 22–39.
- Bourgeois, Philippe, 1982. 'What U.S. Foreign Policy

- Faces in Rural El Salvador: An Eyewitness Account', *Monthly Review*, vol. 37, no. 1, May, pp. 14–30.
- Bourgois, Philippe, 1986. 'Guaymí: les damnés de la plantation', *Ethnies*, nos. 4/5, Summer/Fall, pp. 43–45.
- Bourgois, Philippe, 1988. 'Conjugated Oppression: Class and Ethnicity Among Kuna and Guami Banana Workers on a Corporate Plantation', *American Ethnologist*, vol. 15, no. 2, pp. 328–348.
- Bourgois, Philippe, 1989. *Ethnicity at Work: Divided Labor on a Central American Banana Plantation*. Baltimore: Johns Hopkins University Press.
- Condominas, Georges, 1977 (1957). *We have Eaten the Forest: The Story of a Montagnard Village in the Central Highlands of Vietnam*. Translated by Adrienne Foulke. New York: Hill & Wang.
- Falla, Ricardo, 1983. *Voices of the Survivors: The Massacre at Finca San Francisco, Guatemala*. Cambridge MA: Cultural Survival Occasional Publication, no. 10.
- Gough, Kathleen, 1973. 'World Revolution and the Science of Man', pp. 156–165 in Weaver, 1973.
- Holm, John, 1978. *The Creole English of Nicaragua's Miskito Coast: Its Socio-Linguistic History and a Comparative Study of Its Lexicon and Syntax*. Unpublished PhD dissertation. University of London: Department of Linguistics.
- Horowitz, Irving, ed., 1967. *The Rise and Fall of Project Camelot. Studies in the Relationship between Social Science and Practical Politics*. Cambridge MA: MIT Press.
- Hymes, Dell, 1972. 'Introduction; the Use of Anthropology: Critical, Political, Personal', pp. 3–79 in Dell Hymes, ed., *Reinventing Anthropology*. New York: Pantheon.
- Jones, Delmos, 1971. 'Social Responsibility and the Belief in Basic Research: An Example from Thailand', *Current Anthropology*, vol. 12, no. 3, pp. 347–350.
- Keesing, Roger, 1981. *Cultural Anthropology: A Contemporary Perspective*. New York: Holt, Rinehart & Winston (second edition).
- Keesing, Roger, 1987. 'Anthropology as Interpretive Quest', *Current Anthropology*, vol. 28, no. 2, pp. 161–176.
- Kensington Office of the American Institutes for Research, 1972. *Minority Groups in North Vietnam, Ethnographic Study Series*. Washington DC: US Government Printing Office.
- Mintz, Sidney, 1970. 'Foreword', pp. 1–16 in Norman Whitten & John Szwed, eds., *Afro-American Anthropology: Contemporary Perspectives*. New York: Free Press.
- Nader, Laura, 1972. 'Up the Anthropologist – Perspectives Gained from Studying Up', pp. 284–311 in Hymes, 1972.
- Smith, Carol, 1984. 'Local History in Global Context: Social and Economic Transitions in Western Guatemala', *Comparative Studies in Society and History*, vol. 26, no. 1, pp. 193–228.
- Taussig, Michael, 1984. 'Culture of Terror – Space of Death. Roger Casement's Putumayo Report and the Explanation of Torture', *Comparative Studies in Society and History*, vol. 26, no. 3, pp. 467–497.
- Weaver, Thomas, ed., 1973. *To See Ourselves: Anthropology and Modern Social Issues*. Glenview IL: Scott, Foresman.
- Wolf, Eric, 1972. 'American Anthropologists and American Society', pp. 251–263 in Hymes, 1972.
- Wolf, Eric & Joseph Jorgensen, 1970. 'Anthropology on the Warpath in Thailand', *New York Review of Books*, 19 November, pp. 26–35.

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