

# Relationships, complicity and representation

## Conducting research in Nepal during the Maoist insurgency

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Fig. 1. Newsweek magazine covers the Maoist insurgency.

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1. Sluka (1995) and Lee (1995) offer notable exceptions.

2. A number of recent works in English directly address these complex issues. See Dixit and Ramachandran (2002), Gellner (2003), Karki and Seddon (2003), Thapa (2003), Thapa with Sijapati (2003) and Hutt (2004).

3. A lawless subcontinent. *Himal South Asian*, December 2003, 16(12): 41.

4. The disappeared. *Nepali Times*, 176, 26 December 2003-1 January 2004.

5. 'Question of enforced or involuntary disappearances. Report of the UN Working Group on Enforced or Involuntary Disappearances' (document number E/CN.4/2003/70): see [http://www.unhcr.ch/Huridoca/Huridoca.nsf/\(Symbol\)/E.CN.4.2003.70.and+Corr.1+a.nd+2.En?Opendocument](http://www.unhcr.ch/Huridoca/Huridoca.nsf/(Symbol)/E.CN.4.2003.70.and+Corr.1+a.nd+2.En?Opendocument).

### Introduction

Conducting fieldwork in conflict situations raises specific methodological and ethical questions. While Taussig (1984, 1987, 1992) and Scheper-Hughes (1995) have called for anthropologists to speak out against terror, they have not addressed in detail how researchers working in war zones should negotiate the pragmatics of representation in dangerous field situations. As Kovats-Bernat (2002) notes, although there is a growing literature that aims to develop theoretical approaches to the study of violence, relatively little attention has been given to the practical concerns surrounding fieldwork in conflict situations.<sup>1</sup> What is needed, he argues, is 'the adoption of new tactics for ethnographic research and survival in dangerous field sites – strategies that challenge the conventional ethics of the discipline, reconfigure the relationships between anthropologist and informant, and compel innovation in negotiating the exchange of data under hazardous circumstances' (2002: 208). Danny Hoffman (2003) similarly addresses this issue with his call for a reconsideration of 'frontline anthropology' techniques. We contribute to this discussion by examining these questions within the rapidly changing context of Nepal, where a bitter internal conflict has developed over the last eight years.

Until recently Nepal was considered a relatively unproblematic place to do anthropological fieldwork, perhaps even an 'ideal' place, given the apparent isolation of some of its people from the effects of modernity. The early 'regional ethnography traditions' (Fardon 1990) developed in Nepal tended to marginalize the state in their analysis. In the last few years, however, the situation has changed and it has become impossible to ignore issues of politics and the state.<sup>2</sup>

Since the far-left Communist Party of Nepal (Maoist) proclaimed a 'people's war' in 1996 a brutal conflict has ensued, in response to which the government declared a state of emergency which lasted from November 2001 until August 2002. A seven-month ceasefire broke down in August 2003, and there has since been a significant increase in militarization. At the time of writing, it is estimated that over 8000 people have been killed in the conflict, with approximately 1000 people killed and 600

arrested and held incommunicado just in the four months since the last ceasefire collapsed.<sup>3</sup> The National Human Rights Commission of Nepal lists 808 people as having disappeared since 2000, with 663 of those abducted by the state.<sup>4</sup> This figure gives Nepal one of the highest numbers of disappearances for this time period anywhere in the world, causing the UN Commission on Human Rights to express grave concern.<sup>5</sup>

Clearly the most serious internal crisis in Nepal's modern history (Thapa 2002), this *de facto* civil war has radically altered the context in which anthropologists work. The state response to the violent Maoist movement under the terms of the emergency included the suspension of human rights, repression of free speech, and military deployment to squash the insurgency.<sup>6</sup> Such developments have placed researchers in increasingly complex relationships with regard to informants and their safety, the state machinery in Nepal and the governments of the countries in which we live.

In this article we consider some of the practical, theoretical and political implications of this changed context for fieldwork and ethnographic writing from our perspective as foreign researchers. In order to create a broad framework for discussion we have each contributed a section. Shneiderman draws on a specific research experience in rural Nepal as a case study of the ways in which 'complicity' with informants in the field and colleagues in Kathmandu took on new dimensions as the Maoist situation evolved. Pettigrew focuses on how a changed political context radically alters the impact of our scholarly representations of informants, and considers practically how we may most effectively protect those with whom we work. Harper questions how we may engage critically with our own nations' changing policies towards Nepal in particular, and towards the host countries in which we work in general. He examines the ASA (Association of Social Anthropologists of the UK and Commonwealth) ethical guidelines in relation to broader political complexities to pose the question: to whom are we responsible, as scholars and individuals, when we represent Nepal? We conclude by reflecting upon several shared themes emerging from our accounts which compel us to rethink

Fig. 2. Police inspecting an ambulance destroyed by a Maoist petrol bomb attack, in which several civilians suffered severe burn injuries, April 1999.



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6. The repressive state response to the Maoist movement dates back to the armed police operations of Operation Romeo (1995) and Operation Kilo Sierra 2 (1998), both of which resulted in mass deaths, disappearances and unwarranted arrests (Thapa with Sijapati 2003).

7. I do not intend to replicate the Orientalist narrative of Nepal as a peaceful Shangri-la. Rather, I emphasize how the focal point of daily life among the villagers with whom I work shifted rapidly from concerns about subsistence and food shortage to fear of political violence.

8. Amnesty International reports that, 'The definition of what constitutes a "Maoist", according to army commanders interviewed by Amnesty International, includes civilians who give shelter, food or money to the armed Maoists' (2002: 8).

Fig. 3 (right). Cartoon showing a Nepali family shocked to realize that a ceasefire between the Maoists and the state security forces has been used by both sides to rearm, increasing the country's overall militarization with new shipments of weapons from international sources.

Fig. 4 (left). Maoist guerrilla with muzzle-loader and 'pressure cooker bomb' slung around his neck. Surkhet district, June 2003.



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existing ethical standards for fieldwork, as well as our own positions in relation to such standards.

### Sara Shneiderman

In September 1999 I began my period of tenure as a Fulbright Scholar in Nepal, with a proposal to conduct basic ethnographic research on ethnic and religious identity among a politically under-represented ethnic group in two eastern districts. Having lived in Nepal since 1997, I was well aware of the 'people's war' that had been under way for three years at the time I began my research, but I had never considered the possibility that it would affect my work. Although both of the districts in which I intended to work were listed as 'affected' by the US Embassy in Kathmandu, I had seen no evidence of Maoist activities on my previous visits there. Like most urban residents at the time, I believed that the insurgency was limited to the mid-western areas of Rolpa, Rukum and Jajarkot where it had begun.

By November 1999, when I received my first hand-delivered message from the Maoists, it became clear that my earlier assumptions were mistaken. I was warned that I was under surveillance and should leave the village. However, it took several months for the full implications of this for my work to sink in. Only now, with a few years' distance, am I able to reflect productively on the broader issues surrounding the practice of ethnography in conflict situations that I wish I had considered before beginning my work. This is the crux of the problem I seek to discuss here: how do we adjust our practice when the conditions of our anthropological work change suddenly from those of relative peace to the tension of violent political conflict? In particular, when the foresight with which anthropologists who consciously craft research projects in known conflict zones is unavailable, how must our relationships with local colleagues adapt in order to accommodate the changing situation?

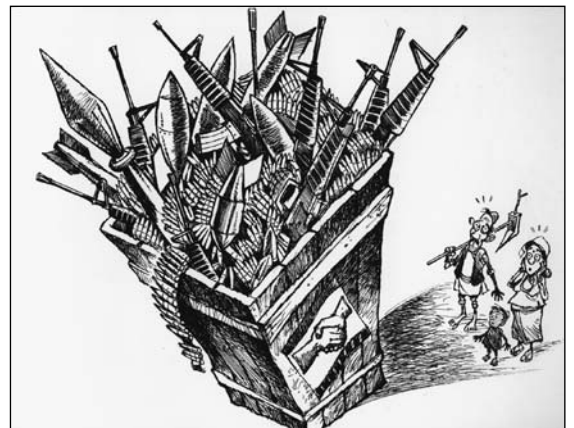
In addressing these questions, I draw upon George Marcus' notion of 'complicity' (1999) to consider the full complexity of relationships between informants and social scientists in emerging conflict situations such as the one I faced in Nepal. Marcus suggests a shift from the traditional relationship of 'rapport', presumed between indigenous informants and anthropologists, to one of 'complicity'. Neither anthropologist nor subject can limit their project to the local alone; instead, both must work towards situating themselves within the bigger picture by acknowledging the complicity of their goals and their constant engagement with an external 'third' (Marcus 1999: 101). In the situation I describe, the 'third' might be considered the insurgency itself – a powerful ideological framework that articulated long-standing indigenous frustrations and gave rise to new forms of political consciousness, but also carried with it threats of violence emanating

from previously unknown sources whose intentions were difficult to ascertain (both Maoist activists and state security forces). My relationships with local colleagues were reconfigured by the rapid establishment of Maoist 'base areas' in the locality and the state response to them. My local colleagues and I entered into new forms of complicity as the primary goals for all of us shifted towards maintaining safety and understanding the evolving situation.

During the early phase of the Maoist presence in my research area in late 1998-early 1999, the Maoists were an unknown quantity, and most villagers vacillated between fear of the unknown and a bravado that trivialized such fears. I did not know whether the Maoists would target me on account of my 'foreignness' – they had made clear anti-foreign statements and were forcing foreign development workers to leave the area – or whether my local friends and co-workers would be targeted because of their engagement with me. It was difficult to discuss the validity of the numerous rumours of Maoist activities – both destructive and constructive – with my close local friends, since during this early phase many people were still in denial about the very real effects of the insurgency on their own lives. The insinuation that I might be in danger was an implicit challenge to my local hosts' ability to protect me, and thereby an insult. At the same time, the suggestion that they might be in danger because of their relationship with me was hard for many to accept. Until that point, relationships with foreigners had been considered as positive symbolic capital within local networks of power and status. This was particularly so for the poor and disenfranchised community with whom I worked: the presence of foreign researchers investigating their culture and history was a major asset in their campaign to gain recognition as a distinct ethnic group within the Nepali nation-state. Initially the fear that I would leave without completing my work outweighed the fear of Maoist repercussions. These issues combined to make it nearly impossible to discuss openly the potential dangers to either me or my informants.

I therefore decided to stay away from the village for three months, from December 1999 to February 2000. Instead I lived in Kathmandu, where I was protected by urban anonymity, and my absence from the village removed any danger that I might precipitate for my friends there. Although my closest research assistant understood and supported my decision, it was largely against the advice and wishes of the larger village community, who repeatedly asked me to return.

The multi-layered nature of complicity in action is evident here. Had I sought to maintain my original complicity with my local informants' agenda of gaining power and status within the identity politics framework, I might have listened to the narrative of bravado, which claimed that the Maoists posed little real danger. Yet from my perspective, the situation required an acknowledgement of the real



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Fig. 5. Water tap in a rural area of Nepal inscribed with graffiti reading 'Communist Party of Nepal (Maoist)'.



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9. As van der Geest (2003) has recently shown, it is possible to maintain confidentiality if informants, the location and the anthropologist are all given pseudonyms. This is not possible in my case as I have always previously written under my own name.

10. Shneiderman is an American citizen and Harper a British citizen. Pettigrew, an Irish citizen, is resident in the UK.

11. These issues were addressed in a recent article in *The Guardian* that critiqued the current British policy towards Nepal. The author was identified as someone working within the development apparatus inside Nepal, but felt compelled to use a pseudonym. (Porter, I. 2003. Britain must act to stop the slide to all-out war in Nepal. *The Guardian*, 18 October).

12. See Roka (2003) for a good overview of the broader social problems militarization has generated.

13. [www.angelfire.com/empire2/nepal/patra.html](http://www.angelfire.com/empire2/nepal/patra.html). The US ultimately allocated more than \$14 million in military aid to Nepal.

14. [www.theasa.org/ethics](http://www.theasa.org/ethics)

changes and a consequent shift towards emphasizing safety rather than pre-existing local goals. In making the decision to stay away, I entered a complex and ongoing set of negotiations between complicities – with the villagers with whom I had originally worked, the Maoists, the state, and the urban intelligentsia, whose attitudes towards the Maoist movement differed substantially from those I had encountered in the village.

The initial tension between my local friends' desire to have me stay and my own concerns about their safety was temporarily resolved by a Maoist visit to my village 'home' in late February. My host family was questioned intensively, although it was clear that the Maoist visitors already had most of the essential information about my activities. While the encounter began in an atmosphere of fear, by the end the Maoists reassured my hosts that neither they nor I were in any real danger. They asked the family to communicate to me that I was given explicit permission to stay because I was not engaged in any development-related activities and my work was aimed at helping the poorest community in the area gain recognition. The unknown had made itself known, and some of my fears were allayed. I returned to the village soon afterwards to continue with my work. It only occurred to me much later that with that move I entered into a new relationship of complicity with the Maoists and their supporters – while almost all other foreigners had been expelled from the area, I was invited back specifically because my work was perceived as implicitly supporting the Maoist agenda of raising class consciousness among disenfranchised rural populations.

This second phase of fieldwork in the now Maoist-occupied area proceeded relatively smoothly, and I spent the period between February and October 2000 living almost entirely in the village. During this time, some villagers made the transition from fearing the Maoists to tac-

itly supporting their activities. Only a very small number of individuals left to join the armed Maoist militia, but many more saw them as a potentially plausible alternative to the corrupt and ineffectual state, which was almost entirely absent in their lives except through relations of exploitation. A new sort of complicity arose through my attempts to make sense of the apparent sympathy among people I greatly respected for a movement which I knew to be unacceptably violent. Both my own continued well-being and that of my informants, not to mention my ability to stay in the area, became in part dependent upon my willingness to acknowledge the pragmatic Maoist agenda as a potentially worthwhile one, however problematic in its implementation.

This approach was in stark opposition to that taken by most government officials, urban Nepali intellectuals, foreign diplomats and aid workers back in Kathmandu at the time. For the most part they continued to view the insurgency as a law and order problem rather than a political battle which required intensive attention at the local level. When I tried to explain what I had witnessed at the village level during brief trips back to the city, I was repeatedly chastised for my naiveté in granting the Maoists any credibility. I was even accused of lying when I described the extent to which the Maoists had established themselves as a powerful force at the local level. At this juncture, I found myself caught between competing complicities, a situation Marcus describes as inherent in multi-sited fieldwork (1999). While I needed to maintain good relationships with people in influential positions in the government as well as in urban-based professional networks, I could not renege on my commitment to represent the village-level reality as my friends and informants there saw it.

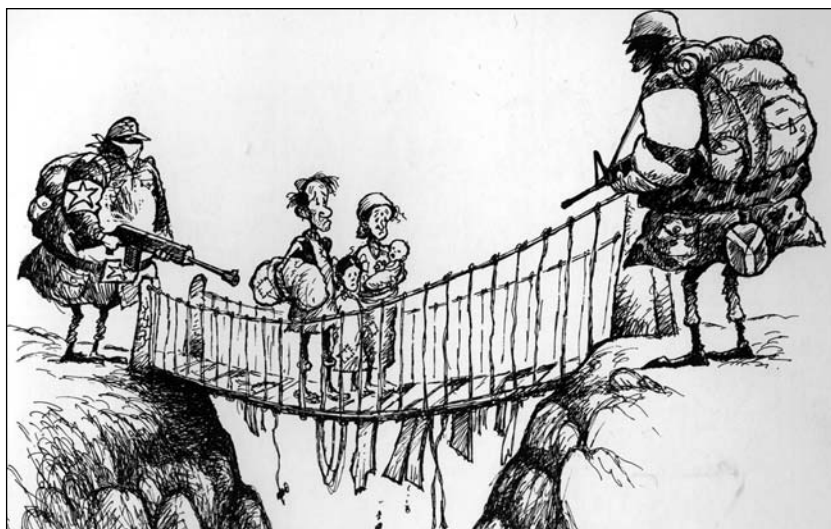
I was therefore caught in the bind of 'accidental anthropology' so well described by Frank Pieke (1995). Having stumbled into a situation I had never envisaged being in, I felt compelled to tell the story I knew, both in my academic writing and in other public forums, yet without the benefit of consciously constructed research techniques or approval from the necessary governmental or academic entities. I would have benefited from a careful consideration of the questions of representation that Judith Pettigrew and Ian Harper address in the following sections of this article.

### Judith Pettigrew

Since 1990 I have conducted research in Nepal on a range of topics including the politics of cultural preservation, ethno-history, health and religion. My work through the 1990s was clearly positioned as the study of a particular ethnic group in a specific region of the country. While my research also addressed issues of wider national concern in Nepal such as the reinterpretation of ethnic identity in the aftermath of the movement to restore democracy in 1990, it focused on exploring and representing the particular perspectives of the group among whom I worked. An important part of my analysis was situating my work in relation to the existing research conducted on this ethnic group.

In 2000 I returned to the villages where I had done much of my earlier research after a gap of two years and discovered that there were large numbers of Maoists in the area. Interviews with middle-aged and older people threw up two recurring themes: their reluctance to accept that local youth were involved with the Maoists, and their surprise when they discovered women's involvement. The Maoists were also emerging as a catalyst that brought pre-existing unexpressed concerns to the surface. Talking about the guerrillas provided an indirect way of talking about conflict between neighbours and kin, and the fears associated with these conflicts. I decided to undertake a research project on the insurgency shortly after taking part in a con-

Fig. 6. Rural Nepalis trapped between the Maoists (left) and the state security forces (right), with literally nowhere to turn.



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Fig. 7. A Maoist woman fighter in combat fatigues walks up a hillside path.

ference on the Maoist movement in the autumn of 2001, as I felt that inadequate attention was being paid to the impact of the conflict on the people being most affected by it – rural civilians.

I began research in December 2001 just after the declaration of the state of emergency. My current project is multi-sited and includes rural locations in different parts of the country. I chose a multi-sited approach as I felt that single-location research – a strategy which has previously been widely used in academic social science research in Nepal – was not the most appropriate mode in which to study the insurgency. In a single location, while there are the obvious problems of undertaking research in a dangerous and volatile situation and the heightened risks of long-term exposure, there are also additional problems such as drawing unwarranted attention to the research, the researchers and/or the host community, which could have widespread and negative implications for all concerned. Using a multi-sited approach, data can be gathered in relatively short periods of time, which may be essential to avoid attracting attention to the informant or the researcher, or to avoid linking the informant with the researchers, who as outsiders may be under suspicion. By gathering data in a number of conflict-affected areas it is easier to maintain the anonymity of all involved, as research is undertaken in geographically diverse locations with a wide range of informants.

Having decided on the design of my project, I needed to select field sites. As I was concerned about what was happening in the area I knew best, and was aware that I had a comparative peacetime perspective which would be valuable in identifying conflict-related change, I selected my original rural field site for inclusion in the project.

I returned to the village in the summer of 2002 to find that on most nights groups of armed insurgents entered the village demanding food and shelter. The security forces also visited and accused villagers of supporting Maoists.<sup>8</sup> While the security forces were in the village, people feared that the army would learn about their interactions with the Maoists. When the army left, villagers worried that the Maoists would interpret their interactions with the army as treacherous. The Maoists had accused people in neighbouring villages of being spies, although no one in my village had been punished yet. In a nearby village an army officer was killed by Maoists and shortly afterwards the army came to search the village and hit people with rifles. According to my informants, during one search a helicopter circled overhead and fired into the village and the nearby forest. The firing was aimed at houses where the soldiers thought they saw smoke, which might indicate that villagers were preparing food for Maoists. A few days

later somebody told the army that Maoists were eating a meal in the next village. By the time the soldiers arrived the Maoists had left and only the family remained. The soldiers came in with guns firing and killed the newly-married daughter and her husband who was home on leave from his work in Saudi Arabia. When I talked to informants about visiting nearby villages where these incidents had taken place in order to document them, people were horrified and asked if I did not realize how dangerous it was even to talk of such things. When I enquired whether I could report these incidents to a human rights organization, I was told that I must not report them in the nearby town, but could talk about them in the capital city.

Such concerns – about human rights documentation, attacks from helicopter gunships and random shooting – introduced an entirely new set of complications to the already sensitive issues of representation that I had struggled with in my earlier research. My previous approach to confidentiality was shaped by the urban Nepali cultural preservationists I had worked among, who found the common anthropological convention of using pseudonyms for informants and field locations deeply suspect. Regarding the writings of other researchers I was often asked, ‘Why are the names changed? Why have people not got credit for their information? What is the researcher trying to hide by changing names? What are the informants trying to hide?’ Surely, my informants reasoned, ‘it is more honest to use the correct names as that way information can be checked’. To these urban activists, disguising informants and research sites was not only questionable, but indicated the first step towards the creation of an unaccountable fantasy world. When it came to my turn to write I named my field sites and my main informants. While this approach was not without problems – one informant in particular was offended by how he had been represented – in the context within which I was working it was the most acceptable one.

But now the situation is radically different. One of the primary concerns I have at present is the protection of my informants. In my written work I no longer identify individuals, the area, or even the district as to do so would place people in danger. Yet the notion that I can fully protect my informants is clearly naïve.<sup>9</sup> By the time I started my work on the Maoist insurgency I already had a history of doing research in this area and among these people. Someone serious about discovering where I had done my fieldwork in the past and where I am doing it now can easily do so. By choosing to address these issues in this article I am providing clues that would enable my work to be located. Despite my intentions it is not possible to provide more than a measure of confidentiality. The alternative is to be silent, but then I, like so many Nepalis who face threats, would have become voiceless – silenced by the conflict. In keeping with writers such as Taussig (1987), Scheper-Hughes (1995) and Sluka (2000), I believe that it is important to write against terror. The challenge is to ensure to the extent possible that such writing does not have unintended consequences, or create new threats, for those who are written about.

In my first publication to arise out of this research on the conflict, I represented my informants as generic ‘rural Nepalis’ and omitted culture-specific reactions to the conflict in an attempt to mute the cultural and ethnic identifiers. Subsequently, I have rethought this position and now feel that it is possible, and in fact important, to use identifiers, as otherwise the work lacks the context and detail that makes for good ethnography. My current project – a book on the impact of the conflict on rural civilians – incorporates culture-specific material with careful disguise of locations, individuals and other identifying factors. As the data has been gathered from multiple sites, and

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conventional patterns of field visits have been avoided, the 'research trail' is deliberately difficult to follow. This contrasts with the notion that rigour in qualitative research can be enhanced by an 'audit trail' whereby the research documents can be scrutinized by external evaluators. The 'audit' process is intended to reveal the chronological steps taken and field methodologies used by the original researcher (Denzin and Lincoln 1994). In conflict situations, such research strategies are not only ineffective, but may even constitute 'bad practice'. Instead, new strategies which evolve in relation to the particular conflict situation are required.

Consequently, my new fieldwork has required me to rethink old data collection techniques. The age-old notion that 'if it isn't written down, it didn't happen' (Kovats-Bernat 2002: 215) is irrelevant. In the field I take heavily disguised rough notes, since keeping detailed research records is impossible as it could place informants, assistants, other support staff or myself in danger. I have stopped my research assistant taking notes, and I carry the brief retrospective notes he writes in the town during our journey to the city. I have developed my ability to remember interviews, scenes or particular events in detail rather than commit them to paper when it might be unsafe to do so. On return to the city we expand on our rough notes on my computer. Pseudonyms are used for locations and informant names, and on one occasion after we had encountered a large number of Maoists in a village, and had detailed observations concerning villager-Maoist interactions, we wrote our field notes as if they were a fictitious report based in a distant land.

Despite these precautions, however, I am in agreement with Peritore (1990) and Kovats-Bernat (2002) when they suggest that the identities of informants might be compromised by the unobserved lapses in attention that regularly occur during the daily grind of fieldwork. In other words, the encryption of notes taken in dangerous field locations must always be recognized as imperfect, and the researcher must work with a constant awareness of possible consequences. Ethnographers working in conflict situations must simultaneously face the challenge of developing new strategies to enhance their safety and those of their informants and that of identifying and explaining the unique social interrelations that occur in the midst of crisis and strife (Kovats-Bernat 2002). While caution, foresight and experience play an important role in managing dangerous field situations, the inherent instability and complexity of such work ensures that there are no easy answers.

**Ian Harper**

Nepal's position within a broader regional and global geopolitical context changed dramatically after 11 September 2001. A politically unstable Nepal is now recognized as a potential threat to both US and regional security. The Communist Party of Nepal (Maoist) was briefly included on the US

roster of international terrorist organizations. Simultaneously, the Nepali state appropriated the term 'terrorist' to label the outlawed Maoist party and its adherents during the state of emergency, and this label remained in place until the ceasefire agreement of January 2003. During the first nationally escalated phase of the conflict from late 2001 until early 2003, both the British and the American government justified direct military support for the Royal Nepalese Army by citing Nepal's 'terrorist problem', coupled with the rhetoric of defending a geopolitically important, yet weak, emerging democratic polity. Yet during this period Nepal entered into a constitutional crisis, with King Gyanendra dissolving parliament on 4 October 2002. Considering that the army remains loyal to the king rather than to the elected prime minister, the current external military support for the Royal Nepalese Army seems to be hastening the demise of democratic process.

How do we react to these changing circumstances as citizens, residents and researchers?<sup>10</sup> Where do our responsibilities lie, or with whom do we become complicit? Most immediately, as my own government defends its support of the military in Nepal in terms of allowing an emergent democracy to defend itself, how should we react to the empirical reality of the collapse of its infrastructure? How do we respond to the acute rise in human rights violations?<sup>11</sup> As Pat Caplan (2003) asks in a recent volume on ethics, particularly given the recent global changes, should we as anthropologists remain 'speechless'?

In answering these questions, I believe that we have a responsibility to publicize the consequences of militarizing Nepal's current socio-political problems.<sup>12</sup> I am prepared to use my research and long-standing relationship with Nepal to do this. Judith Pettigrew and I, for example, have been involved with a collective of concerned academics, development workers, human rights activists and others in posing relevant questions to policy-makers in the UK. Such ongoing discussion between Nepal specialists and UK government officials is one way in which we may put our knowledge of Nepal to strategic political use.

Whether such engagement actually has any immediately quantifiable impact is open to question. The British government gifted two military helicopters to the Nepalese army earlier in the conflict, and recent unconfirmed reports suggest further surveillance equipment is to be donated. Although British government assistance to the Nepali security forces is classified as 'non-lethal', there is a certain amount of ambiguity, and a need for greater accountability to British taxpayers as to how such money is spent.

In late 2002, as the US Congress considered foreign military allocations for the coming year, all three of us signed a petition demanding that the US did not provide the Nepali military with extra support.<sup>13</sup> In signing this, we found ourselves entering into increasingly contested political and ethical waters. Some of our academic colleagues were critical of this stand, reasoning that an ostensibly democratic regime had the right to defend itself against armed insurgents. Some Nepali colleagues felt that this petition



Fig. 8. A poster widely distributed in Nepal in 2002, showing 'Maoist leaders declared terrorists'. The government offered cash rewards for information leading to their capture.



Fig. 9. Maoist fighters as part of the audience at a cultural programme. The banner in the background says, 'American imperialism – quit Nepal'. Surkhet district, June 2003.

overtly interfered with the workings of the Nepali state and overstepped the appropriate boundaries of foreign academic engagement. However, others found the petition a welcome show of support for a position that was difficult to voice openly inside Nepal at the time.

In such a complex and shifting terrain, we must ask ourselves about the relationship between scholarly activity and our own personal political positions, which has rightly long been a focus of ethical attention in anthropology. While I acknowledge that we can nominally separate our research agendas from our personal politics, when I sign a petition or take a public political stand in my capacity as a lecturer at an academic institution, for example, I knowingly appropriate the status of this position. I do not speak as an individual alone, but am backed by the weight of a certain authority granted by my academic standing, regardless of my own area of expertise. In this way distinctions between our personal political positions and our subjectivities as researchers become less clear-cut. Becoming explicitly and intentionally complicit with certain political agendas has the benefit of being overt and thereby creating an arena for debate. Colleagues who do not articulate their position, or who feel uncritically that it is the right of any state to defend itself against 'terrorist' groups, could be perceived as being implicitly complicit with the diverse political interests hindering a reinstatement of the democratic process in Nepal today. Perhaps it is wiser to be explicitly complicit, whenever possible, than to be assigned complicity by default in the wake of silence.

Since the Nepali state is in a downward spiral in terms of democratic process, I shall finish by reconsidering the ASA guidelines on research ethics, particularly the section on 'relations with own and host governments'. A critical reading of this section of the guidelines leads me to question their utility as a guide for best practice in the light of our multiply complex range of subject positions. Given the state of Nepal's current lack of democratic legitimacy, the uncertain relationships between it and our own countries of citizenship and residence, and the potential and real dangers of these engagements for our informants, it is vital to reconsider the classical ethical question: *to whom are we responsible?* But the answers are increasingly less clear-cut. The ASA guidelines frame the question of ethics in terms of our 'relations with and responsibilities towards' a number of actors in the research experience – research participants; sponsors, funders and employers; colleagues and the discipline; own and host governments; wider society – and provide quite a broad base for considering what to do, or what not to do.<sup>14</sup> The section on relations with own and host states does suggest that, as researchers, we ought to examine the issues of cross-national research, bearing in

mind that there are disparities in wealth, power, legal status and political interests between national systems. But this paints an overly static picture of relationships between researchers and governments, without acknowledging how they may change dramatically in rapid reaction to international, national and local events. Such shifts impinge critically on our research at all levels, including the consequent political positions we choose to adopt, or are pushed into adopting for strategic reasons. As Mills suggests, now is not the time to congratulate ourselves on our discipline's political and ethical reflexivity (2003). Rather, in the glaring light of the harsh political realities of our research areas, perhaps we ought to expand further the dialogue on the ethics and politics of our discipline.

### Concluding remarks

We believe that we must reconsider our research positions more carefully within a range of varied, rapidly shifting and often competing discursive positions. The notion of 'complicity' is useful, also in its everyday meaning – 'the act of taking part with another person in a crime' (Wehmeier 2002) – as it highlights the contextual, yet relational nature of our enterprise, as one where any chance of appropriating the moral high ground is lost. As Shneiderman's narrative suggests, emerging conflict situations can add unexpected complexities to already challenging relationships in the field. But rather than shying away from such difficulties, or ignoring them as conflict-related problems irrelevant to the 'real' fieldwork at hand, these issues must be engaged with fully on anthropological and personal levels. Anthropologists must evaluate the real dangers to both themselves and informants as far as possible, yet at the same time be ready to take a political stand which may involve making choices between competing complicities. As Harper also points out, these complicities and responsibilities play out in broader political fields that extend beyond the boundaries of Nepal, into the transnational realm in which our political views as citizens at home are shaped.

Furthermore, as Pettigrew shows, we must recognize the impossibility of fully anonymizing our informants, although we must minimize the potential for identification whenever possible. The very need to do this stands in stark contrast to previously appropriate techniques, such as the imperative to name informants within the context of cultural revivalism. These opposing strategies demonstrate the constant need for flexibility and insight on our part as anthropologists, so as to avoid reifying any one research strategy as the 'right' one for a particular place or time. In conditions of war any representation, regardless of authorial intent, can become dangerous if it is appropriated as intelligence by actors on either side of the conflict (Lee 1995). It is our responsibility to safeguard our information from being used in that manner to the extent possible, and doing so requires a nuanced understanding of the relationships that inform our representational strategies.

As Wax and Gusterson point out in the June 2003 edition of *ANTHROPOLOGY TODAY*, and David Price (2001) has highlighted in earlier issues, the question of anthropologists' wartime allegiances is not new. Perhaps, however, we should follow Danny Hoffman in recognizing that the definition of 'wartime' itself needs rethinking. Reflecting upon our experiences in the rapidly shifting conflict environment of Nepal, it is clear that anthropologists of their own ethical positions and complicities, and their effects on others at local, national and international levels. The 'frontline' may no longer be somewhere you go intentionally, but rather something that can emerge around you at any time, encompassing past work, relationships and ethical certainties with its new requirements. ●

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