Researching Violence in Africa
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Conflict settings: changing wars, changing analyses

Conflict in Africa has changed since the end of the Cold War.¹ Turmoil within sovereign states has made way for turmoil of a more global nature, forcing analysts to use frameworks that capture trans-border movements and extra-legal activity. Extreme forms of violence, while admittedly not entirely new, are now part of the arsenal of tactics in common use in today’s wars. Radicalized too are the responses to conflict-induced humanitarian aid. In the wake of 9/11, leading donors have become preoccupied with international security; they have made ‘security’ central to a reconfigured approach to development. Differently put, humanitarian aid is now an integral part of the politico-military strategies western governments (and donors) pursue “to transform conflicts, decrease violence and set the stage for liberal development” (Duffield, Macrae and Curtis 2001, 269). While this converging of humanitarian action and politics can be seen most clearly in places like Afghanistan and Iraq, in Africa too states perceived as ‘failed states’ are increasingly targeted for a radical overhaul of attitudes and beliefs. The new development agenda aims at “getting inside the head to govern the hand” (Duffield 2002, 1067). The aid emphasis now focuses on the will to govern; a priority which appears logical to donors since they portray ‘borderlands’ as chaotic and devoid of morality. Arguing against this perception of Africa’s so-called New Wars, Duffield (2002, 1059) posits that the ‘shadow economies’ of recent war zones – economies that have a transnational, extra-legal character – are governed by social principles and may provide opportunities for reflexive development. Similarly, Richards (2004) argues that war must be regarded as a social system without the sharp distinction from peace or the pure imagery of social breakdown conveyed by most mainstream analysis.

¹ While some have adopted the label ‘new wars’ for post-Cold War conflicts, this remains a contentious category: one critique is Kalyvas’s (2001).
Recent African conflicts have spawned a diverging set of analytical explanations: their *raison d’être* is commonly sought in global and local motivations of power and profit (from oil or mineral deposits) or in the continent’s presumed ‘barbarism’ or, much more credibly, in the collapse of neo-patrimonial states and the impact on young people’s lives (for a review, see Peters and Richards 2007). Whatever reasons are put forward, a comprehensive analysis of post-Cold War conflicts requires a broad spectrum of social science perspectives, along with hard questions about research agendas, strategies and ethics. Heated debates on the causes and characteristics of armed conflicts or non-war violence have often been sustained in recent years without adequate attention to the robustness of evidence or the care of methods in research. Questions of methodology and ethics must be asked and answered with some urgency, especially given the renewed threat of a ‘militarized’ approach to social science research on conflict and security. The threat has already resulted in intense academic debate on the use of ‘cultural knowledge’ in the so-called ‘war on terror’ (Gonzales 2007; Keenan 2008). Not only is the use of ‘cultural knowledge’ for purposes of establishing indirect rule over foreign lands (or people) ethically inadmissible, in breach of anthropological codes of ethics for example, but it also promotes a notion of culture that routinely fails to recognise it as historical process. Moreover, the new policy discourse of ‘risk/fear/security/development’ views Africa’s marginalized populations as collectively prone to committing acts of extreme violence and terrorism, thus posing a danger to the international community (Keenan 2008, 18). With roots in Kaplan’s (1994) infamous ‘loose molecules’ and ‘coming anarchy’, this discourse has taken a more sophisticated and formal political science turn with the suggestion by Fearon and Laitin (2004) that so-called failed states are a source of ‘international public bads’, spillover effects including disease, unwanted migration, and terrorism. Such a discourse presents new and very difficult ethical dilemmas for social scientists involved in the study of conflict.

*Research ethics: general considerations*

Social science research involving fieldwork carries legal and ethical concerns every step of the way, even in situations not characterized by violence and/or conflict. Hard questions must be asked of every activity: from selecting a topic, a location and a time, to deciding who to ask for funding and how to obtain access and consent,
right down to deciding what and where to publish and how to handle post-publication responsibilities. Paramount among the ethical concerns, however, are the need to protect research participants and honour their trust, and the need for foresight regarding the consequences of research, anticipated and non-anticipated. Obtaining consent does not absolve the researcher from the obligation to protect informants against potentially harmful repercussions (ASA 1987, 2). The basic ‘avoid harm or wrong’ stance is also pivotal in the Code of Ethics drafted by the American Anthropological Association (AAA 1998, section III), which is currently being revised to ensure greater transparency and openness in the hope of stemming the danger that military and intelligence institutions use findings “to harm those we study” (AAA Communication, 24 September 2008). Transparency about the nature and objectives of research, with all stakeholders, has become a fundamental preoccupation.

Honesty and transparency form the grounds also for obtaining consent. The ethical guidelines of the Association of Social Anthropologists of Great Britain and the Commonwealth (ASA) state:

> Negotiating consent entails communicating information likely to be material to a person’s willingness to participate, such as: the purpose(s) of study, and anticipated consequences of the research; the identity of funders and sponsors; the anticipated uses of data; possible benefits of the study and possible harm or discomfort that might affect participants; issues related to data storage and security; and the degree of anonymity and confidentiality which may be afforded to informants and subjects (ASA 1987, 3).

While social science researchers generally endeavour to avoid undue intrusion, they also know from experience that the fieldwork trajectory is difficult to predict in all its aspects, especially when long-term research is undertaken. Unforeseen events, a chance meeting perhaps or a sudden hint, may offer new, even pressing opportunities for enquiry long after the research was explained and consent obtained. In light of such difficulties, the AAA Code of Ethics presents researchers with “a framework, not an ironclad formula for making decisions”. The onus is upon the “individual anthropologist, [who] must be willing to make carefully considered ethical choices and be prepared to make clear the assumptions, facts and issues on which those choices are based” (AAA Code of Ethics, 1998, section II). Given the dynamic nature of the research process, the implication is that obtaining consent may require re-negotiations over time.
Despite their built-in flexibility, anthropological guidelines have been criticised from within the discipline (see, for example, Miller and Scollon this volume). Invaluable though it is, the advice on consent and harm tends to depoliticize the researched, thus falling short of reflecting on the diversity of actors who participate in research. To take a common experience, the gatekeepers of a given community may have expectations of the researcher quite different from those held by ordinary members. Researchers may therefore face a dilemma. While they “should be wary of inadvertently disturbing the relationship between subjects and gatekeepers since that will continue long after the researcher has left the field” (ASA 1987, 6), researchers will also need to ensure that they move beyond the gatekeepers in order to access a representative range of voices (see Pottier 1997). More broadly, researchers should be mindful that “ethics, like politics, is a series of processes in which power is heavily implicated” (Caplan 2003, 27). Research has to be conducted with an awareness of power relations involving gatekeepers and community members, gatekeepers and researchers, national institutions and research subjects, and indeed families and individuals, as Mupotsa (this volume) shows with respect to discourses of gender roles in Zimbabwe.

When considering issues of power the researcher should also ask: “who am I to do this research?” and “what’s it all for?” (Caplan 2003, 27) as well as “in whose interest is this research, or this access to information?”. Where researchers have built ‘attachments’ and taken on advocacy roles, perhaps to stem the negative impact of opportunistic development programmes, they too must check for traces of hierarchy and paternalism in the way they conduct their research and serve up its findings. Hierarchy and paternalism are stubborn forces much exposed in postmodern discussions on ethics and fieldwork (see Fluehr-Lobban 1991).

**Ethical research in conflict settings**

9/11 and its aftermath have prompted intense debate about how social scientists should engage in conflict-related research. While the debate has focused mainly on intelligence matters, the issues raised do have wider relevance for anyone attempting to understand civil strife. One central concern here is that the apparently simple ‘do no harm’ principle presents several ethical quandaries. For example, the extent to
which a researcher “can avoid or prevent harm may be severely limited or impossible to know or predict” (Fluehr-Lobban 2008, 19). Addressing colleagues across a broad spectrum of disciplines, Fluehr-Lobban proposes that researchers involved in conflict (and post-conflict) situations ask a set of questions to assess the specific situation they are facing. For our purposes, we have modified these questions to provide an ethical basis for shaping the researcher’s actions.

1. How can I reduce or prevent potential harm (e.g. loss of life, disability, psychological harm)?
2. How can I avoid violating ethical/moral norms (e.g. the informed consent and ‘do no harm’ principles)?
3. How can I avoid transgressing values, customs, and desires and how should I portray cultural perspectives on these? (Fluehr-Lobban 2008, 20).

While the section on informed consent in the AAA Code of Ethics requires updating in view of possible military-intelligence engagements, it is nonetheless sufficiently fine-grained to guide conflict analysts on the risks and benefits of their performances (Fluehr-Lobban 2008, 21). Concretely, and because researching ‘dangerous fields’ carries multiple risks, researchers should consider \textit{calculated strategies} whereby the gain of (relatively) credible information outweighs the risk of personal harm to the researcher and her informants. But what is calculated risk? To answer the question is to confront some difficult-to-pin-down emotions and awkward-to-acknowledge feelings. Indeed, after defining dangerous fields as “sites where social relationships and cultural realities are critically modified by the pervasion of fear, threat of force, or (ir)regular application of violence”, Kovats-Bernat speaks of researchers falling for the seductive calculation of weighing their own lives against the raw material of their livelihoods (Kovats-Bernat 2002, 208). Fieldworking anthropologists frequently ponder “how much must be laid down as collateral (in the form of risks to the personal safety and well being of self and informant) in order to acquire certain information” (2002, 211).

\textsuperscript{2} Fluehr-Lobban defines ethics as ‘a system of moral principles, the rules of conduct, associated with human actions described as right or wrong, good or bad’ (Fluehr-Lobban 2008, 19).
This portrayal of how researchers work and calculate ‘under fire’ may be somewhat caricatured, but it is not far off the mark. When they feel the adrenalin rush, do researchers really understand (and control) what is going on inside their own bodies and minds? When they are fully immersed in research with a community they know intimately, are they able to detect increasing risks to themselves and their collaborators in time to be able to do anything about them? Elisabeth Wood (2006, 384) emphasises that those “carrying out extended field research in conflict zones are likely to experience additional and intense emotions in the course of their work, including fear, anger, outrage, grief, and pity, often through observing, suffering, or fearing the effects of violence. Indeed, field researchers in extreme cases may suffer from ‘secondary trauma’, the sustained effects on witnesses of observing gross human rights violations. And field researchers often feel tremendous stress in their efforts to keep their data secure (as when carrying their data through check points.) In sharply polarized settings, researchers may also feel it stressful to “manage” information from both sides and to engage in interviews with all parties.” Her argument is that failure to acknowledge these emotions can lead researchers to make errors of judgement with potentially damaging consequences for the researcher herself and/or for informants. In such contexts, “most people are susceptible to flattering invitations to share their experiences (and inevitably their data), to entertain new friends with stories (and data) from their field site, to embark on friendships or relationships that may be perceived as compromising the project, or to “make a difference” by passing on field data “confidentially” to some (supposedly responsible) person”.

In answer to the question ‘what’s it all for?’ (Caplan above) or, for our purposes, ‘why and how should social scientists work in conflict zones?’, Danny Hoffman suggests that the honourable aim is to seek “to broaden our understanding of what constitutes war” through the exploration of “research spaces that have previously been officially or unofficially restricted, and to disseminate that scholarship in underutilized venues” (Hoffman 2003, 9, 10). “Research carried out in these

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3 For a neuro-scientific assessment of the inseparability (for good and ill) of emotions and rational judgement see Damasio (1995).
4 Other efforts to broaden the understanding of what constitutes war or to promote the use of ideas of a ‘continuum of violence’ include McIlwaine and Moser (2006), Bourgois (2005), and Scheper-Hughes (1993).
difficult situations provides alternate ways of understanding particular conflicts, and can greatly facilitate policy responses”, argues Susan Thomson (2009, 3). Broadening the concept of war can mean researching informal war-time trade, as Carolyn Nordstrom has done in Shadows of War (2004). Commonly perceived as ‘peripheral’, such trade is not, Nordstrom showed, simply a matter of money, but it is also part and parcel of the ‘socio-political power houses’ linked to ‘the centre’, i.e. linked more closely than commonly thought to several less-than-legal economic operations run by global corporations and economic institutions (2004, 233). The potential such research has in raising awareness about crucial—but-hidden linkages is what gives empirical research on conflict and war a legitimate place in the 21st century. Empirical research may also challenge other assumptions, e.g. that violence is random, irrational and meaningless (see Richards 1996; Cramer 2006), that war and violence are self-evidently “development in reverse” as the World Bank put it (Cramer 2006), and that collectively delivered testimonies of extreme violence can be taken at face value (see Peters and Richards 2007; Pottier 2007). Further, though, researchers in conflict zones may wonder what the purpose of their research is by contrast, for example, with engaging in humanitarian assistance or technical support or, indeed, political activism. Individual researchers will make their own judgements on this dilemma. One example is Elisabeth Wood’s (2006, 383): “my own belief in the value of what I was doing was sustained by the ongoing endorsement of the project by rural residents willing to spend many hours telling me the history of their families and communities. Their belief kept me clear that my core value was the purpose of the project, to document the history of the war in the case-study areas.”

But how do researchers decide on the best tactic for reducing harm to self or informants? The chief dilemma here, and a source of moral confusion, is that the AAA Code of Ethics conceptualizes power relations in a way that does not apply to conflict situations. For example, the principle that ‘anthropologists must do everything in their power to protect the physical, social, and psychological welfare and to honour the dignity and privacy of those studied’ fails to take account of post-colonial conditions or other situations in which they may not be able to control such an outcome. According to Kovats-Bernat, the principle reflects “the colonial legacy of anthropology – one in which the anthropologist is assumed to be able to control or at least mediate or negotiate danger away from those with whom she or he is working”
(Kovats-Bernat 2002, 214). Researchers of conflict do not control the settings they work in, but rather rely on local intelligence, know-how and protection. Moreover, in contemporary conflicts, researchers can no longer predict the full array of likely consequences for those who participate in the fieldwork encounter. Such intractable problems force researchers to rethink their relationship with informants. As understood by Kovats-Bernat, the relationship “should be one of mutual responsibility – and not just for the validity of the data reported; all participants in the research must also be willing to accept the possibility that any involvement in the study could result in intimidation, arrest, torture, disappearance, assassination, or a range of other unforeseeable dangers” (2002, 214; emphasis in text). Kovats-Bernat advocates the adoption of a localized ethic grounded in extensive dialogue with informants. Even here, estimating the trade-off between information and political, physical consequences and attaching values to the estimation is not solely the preserve of the researcher but depends also on the agency, emotions, and judgement of informants. Pearce (2008) shows how difficult this is in the intensely politicised context of post-war Angola, a context of partidarização or the pervasive influence in social life of the organisation of the ruling MPLA. Those researchers working with local partners or with research assistants face additional complexities: these individuals and agencies themselves “can influence the neutrality/objectivity of the research site and of its informants” (cited in Thomson 2009, 8).

From the above review one overarching message emerges. In research on or during conflict, ethical issues do not dramatically change but they get sharpened and become more difficult to resolve. Thus Richard Black (2003, 104) has argued that what really counts is the ‘basic ethics’ of normal research. In addition, he suggests, researchers should be alert to new, context-specific guidelines set for humanitarians. For example, during Liberia’s civil war (1989–1996) ethical guidelines for humanitarians were drafted to counter two common criticisms: first, that aid may prevent powerful nations from addressing the need for “political or military action in favour of justice”, and, second, that aid may help to sustain conflict (Black 2003, 96). With the new guidelines in mind, Black urges that researchers, whether ‘independent’ or ‘commissioned’, must be clear about their priorities; must take care that their data are not used in military or strategic intelligence; and must ensure that the information gained, which is likely to be of a ‘snapshot’ nature, is as rigorous and reliable as can be (2003, 102).
The unintended use of data, mostly abuse, features among the larger moral issues that need addressing. It is a risk (as Black also indicates) linked to the appeal that ethnographic data appears to have within counterinsurgency circles, which has quite a history (Bourgois 1990, 44). Bourgois reflects that while his anthropological training had equipped him to research why an ethnic minority might rise up in arms against its government, he was completely unprepared...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 counterrinsurgency operations the way monographs by unsuspecting – and not so unsuspecting – anthropologists working in Indochina were abused in South-east Asia during the Vietnam war? (Bourgois 1990, 48)

The bottom line for Bourgois was that anthropological research ethics as formulated in the guidelines/codes available in the late 1980s stopped short of addressing moral concerns about injustice (including global injustice) and the violation of human rights. The standard ethical concerns about informed consent, representation, anonymity, privacy, funding, and so on, did not address “the political and even human rights dimension confronting the people [whom anthropologists] research” (Bourgois 1990, 45). Thus, the anthropologist found himself in the position of having to decide whether to censor or silence his own voice in order to prevent further injustice from being meted out upon the subjects of his research. Hammond (this volume) also deals with this dilemma.

Although much has changed since the late 1980s, notably in anthropology’s engagement with human rights (see e.g. Wilson and Mitchell 2003), there are lingering dilemmas in relation to issues such as the researcher’s neutrality. Thus Nancy Scheper-Hughes, following her research in various settings – poor shantytowns, AIDS-affected communities, a squatter camp in South Africa – believes she has reason “to pause and reconsider the traditional role of the anthropologist as neutral, dispassionate, cool and rational, objective observer of the human condition” (Scheper-Hughes 1995, 410). Defining ethics in terms of responsibility, accountability and answerability to ‘the other’, Scheper-Hughes argues that a politically and morally engaged discipline requires its practitioners to be ‘witnesses’ instead of ‘spectators’. Taking up a position very similar to that adopted by ‘journalists of attachment’ (see below), she warns:
If anthropologists deny themselves the power (because it implies a privileged position) to identify an ill or a wrong and choose to ignore (because it is not pretty) the extent to which dominated people sometimes play the role of their own executioners, they collaborate with the relations of power and silence that allow the destruction to continue. (Scheper-Hughes 1995, 419)

Relations of power and silence may indeed result in researchers opting for self-censorship, a condition they may justify as being a security necessity. Under these circumstances the question becomes: at what point does an ethic of silence become an ethic of complicity? Posing this question, Hutchinson (this volume) also asks what researchers should do with information they stumble across and would have preferred not to know (or not to have witnessed). For researchers of conflict, questions of this sort just pile up, mostly without a straightforward answer and often leading to compromise. For Hutchinson, the bottom line is that the claim to research impartiality may be an illusion, but if researchers do not cultivate it then nuanced research on violence may not be undertaken. In which case, “how will the world know what is really happening in places like Sudan?” (Hutchinson, this volume).

Hutchinson’s candid admission that impartiality may be an illusion does not, however, imply that the researcher must take sides or that every conflict situation is easy to read in terms of its rights and wrongs. In this context, we agree with Adam Kuper’s comment that Scheper-Hughes presents in her South African squatter camp ethnography with a scenario in which the right choice is easy to detect. In contrast, Kuper stresses, “most ethnographic situations are less dramatic and most political choices more complicated” (Kuper 1995, 425). Aihwa Ong (1995) makes a somewhat similar point: “I agree with Scheper-Hughes’s view that anthropology is ultimately about ethics, but I am uncomfortable with her sense of political righteousness. I think that, taken to the extreme, both positions [neutrality versus advocacy] are very dangerous, if not for anthropologists, then for the people they work with” (Ong 1995, 429). Indeed, much of the research contained in this volume takes the approach of trying to discern the ‘least worst option,’ the path least ethically treacherous rather than that which is right, true, or just.
Since frontline researchers and journalists face similar challenges, it is useful briefly to consider the emergence of advocacy journalism in the mid-1990s. In an article titled “Frontline anthropology”, Hoffman proffers that journalists covering war are increasingly unable “to go it alone and report on conflicts outside the media spotlight”. In other words, despite the proliferation of TV news channels and commentators on the internet, ‘the news’ is now “largely produced by official sources, pool reports or news services,” and controlled by a mere handful of media conglomerates (Hoffman 2003, 10).

Rebelling against the constraints of such embeddedness, some journalists in the 1990s relinquished their erstwhile neutrality to embark on a ‘journalism of attachment’. Rejecting neutrality as ‘an accomplice to all sorts of evil’ (Amanpour, CNN, cited in Hammond 2002, 177), advocacy journalists began to see it as their moral duty “to expose human rights abuses and reveal the plight of victims”; they needed to make “a stand against the moral failings of Western governments” (2002, 178). As a corollary, they also rejected the neutral stance of ‘traditional’ humanitarian interventions, and became unafraid to take sides in situations of conflict. Fergal Keane, for example, endorsed the ‘New Humanitarianism’ (of Médecins Sans Frontières) and argued that the Rwandan refugee camps that had sprung up after the genocide did not deserve any international assistance. The camps were a ‘humanitarian haven’ for the killers, there was nothing more to say (Keane 1995, 186).

Having initially ‘attached’ himself to the invading and ultimately victorious Rwandese Patriotic Front (RPF), Fergal Keane would later express regret about falling into the trap of blind partisanship. Choosing his words carefully, Keane writes about Rwanda:

…if we learn one thing from the last fifty years it is that stability built on a foundation of repression is no stability at all. Mr Kagame’s supporters will point to media hate messages in the run up to the genocide as a reason to control the flow of information tightly now. In a country still fragile after the horror of genocide, the argument goes, it would be irresponsible for the government to allow anybody to write exactly what they want. But what happens when that argument is used to silence legitimate debate or questioning of the ruling elite? The powerful will always find reasons to justify silencing those who threaten their power. And in the case of Rwanda in particular they will find foreign allies, including journalists, who will mitigate the abuses of human rights there on the
basis that the genocide created a situation of unprecedented difficulty for the new rulers (Keane 2007, 579–80).

A decade earlier, Keane had been gullible to political propaganda. His award-winning Season of Blood (which won the 1995 Orwell Prize) had carried the message (in Pottier’s words): “dear reader: do not waste your time with anything written [on Rwanda] between independence and 1990, the point at which the RPF invaded Rwanda and began to educate the world” (Pottier 2002, 64–66 for a review of the methodology used in Season of Blood). Despite some truly brave reporting when surrounded by extremists in refugee camps, Keane’s sense of righteousness (his journalism of attachment) was spectacularly one-sided. His brand of advocacy journalism was later criticized for having encouraged a “celebration of violence against those perceived as undeserving victims” (Hammond 2002, 180). If Keane’s admission of how the RPF duped ‘allied’ foreign journalists deserves our attention here, it is to remind social science researchers that they, too, face similar challenges and risks. In short, neutrality and impartiality (including their rejection) constitute an immensely complex ethical minefield (in this volume, see especially the chapters by Hutchinson and Wienia).

Methodological innovation as a response to ethical challenges

The specific ethical dilemmas of the work that researchers are engaged in while working in violent and conflict-affected settings lead (or should lead) researchers to question what methodological adaptations can be made to, for instance, protect informants, reduce risk for the research team, present both sides of a skewed conflict, or find reliable information in a climate of rumour, silence, innuendo, and suspicion. Anyone reading this book hoping to find a manual for how to conduct research in all conflict settings should stop reading here. For more than anything, researching in conflict situations requires the social scientist to think on his or her feet, changing research plans, survey or interview designs and developing ad hoc solutions to minimize risk and damage on a day by day, sometimes moment by moment, basis. John King (2009) characterizes this process as being similar to stage managing in a theatrical context.

Researching in conflict zones requires constant consideration of ethical issues – how is it possible to do no harm, or what set of actions contains the least risk of doing harm? How can one maintain a position
of neutrality in a politically and often emotionally charged environment? How can the researcher protect both her informants and herself (and others, including research assistants or enumerators) from harm while conducting research, and avoid negative repercussions of research once it is disseminated? How should one evaluate informants’ accounts when there may be political capital to be gained from misleading the researcher or legitimating claims to power through telling one version of the truth rather than another? How does one reconcile the roles of the researcher as a balanced, if not neutral, observer, with that of witness and/or activist, particularly in the face of cruelty, suffering, and desperation? In answering these questions, researchers must navigate ethical terrain by adapting their methodological plan of action to the local realities of the situation they are working in.

There is a small but growing reflexive literature on the realities of carrying out research in violent settings or conflict zones. Nordstrom and Robben’s *Fieldwork Under Fire* is one of the better known volumes in which researchers write candidly about the ethical conundrums they have faced while working in conflict areas. Our intention in this volume is to look at how people have applied ethical considerations to methodological adaptation and innovation.

Clearly the more the researcher knows about the society in which he is working, the better able he will be to navigate the minefield of sensitivities concerning representation, not to mention to maximize his own personal safety. Miller and Scollon (this volume) point out that a collaborative approach to research that emphasizes local perceptions and concepts of peace, security, and conflict is likely to provide more effective tools than top-down approaches favoured by policy makers and aid agencies. While this may seem something of an obvious point to some social science researchers, it is not yet common practice in applied research. Miller and Scollon make a plea to practitioners conducting research to privilege local perspectives over international formulaic concepts, particularly if the research is aimed at trying to promote peacebuilding or conflict resolution.

This may beg the question, what local concepts should one privilege? Given that we know that communities are never monolithic or static and that perspectives on conflict, security, history, and justice are highly subject to contestation and political manipulation, how should the researcher work to ensure as balanced an analysis as possible? In the face of obvious tyranny and oppression, is it even necessary to work towards such a balanced view? Several authors in this volume
deal with this issue (see Akanji, Ukiwo, Mupotsa, and Hutchinson). Akanji discusses how he refused to identify himself as belonging to either ethnic group involved in the conflict, insisting that he was ‘Nigerian’ despite efforts of his informants to pigeonhole him into one ethnic affiliation or the other. On the other hand, Ukiwo chose to present himself as being Efik (one of the parties to the conflict in Warri, Nigeria) when he was researching Efik perspectives on the conflict so that he could be perceived as an ‘insider’ and thus gain access to more in-depth information than would have been available had he been perceived as an outsider. When working with other communities for whom insider knowledge was not privileged, however, Ukiwo presented himself as an outsider. Mupotsa (this volume) expressly contributed her own experiences in semi-structured interviews in order to facilitate broaching sensitive issues of discourse, structural power relations, and gender. Mupotsa also takes up the question of how to deal with ‘our culture’ in her investigation of representations and interpretations of ‘women’s experience’ in Zimbabwe. Mupotsa (this volume) argues that the research made her abandon all thought of ‘unsituated objectivity’, opting instead for a full recognition of the fact that ‘the researched’ – always a heterogeneous body – exert their own agency vis-à-vis researchers.

Hammond’s discussion of silence on the part of her Afar informants suggests that in trying to understand ‘the whole story,’ it is essential to listen to both what is said and what is not said. Like Akanji, Hammond found that people were reluctant to discuss various aspects of their conflict for fear that they would be punished for expressing their view. While silences may be the result of explicit (and specific) threats, silent spaces may also emerge from prolonged periods of conflict, in which people feel so ground down by the oppression of their opponent that they stop complaining about their treatment. Rather than resignation, silence in such cases can open up spaces of relative freedom, whereby people are left alone as long as they do not complain loudly about their treatment. As Lee Ann Fujii points out, silences can be “polyvalent. Their meanings can be multiple and contradictory. They can both hide and reveal” (Fujii 2009, 158).

Wienia’s exploration of the ways that “facts fracture” into truth(s), silence, rumour, and interpretations with varying levels of authoritative license, creates an uncomfortable mis-match of expectations. While Wienia, as researcher, seeks to dismantle the idea of there being a single set of facts that explain the accounts he investigates, his informants
are convinced that he is aiming to uncover ‘the truth’. This leads to an uncomfortable feeling on the part of the researcher that the enterprise his informants thought they were contributing to was in fact quite different than they had expected, and that some of his informants might be disappointed with the product of the research. Again, while discomfort at the possibility of not fulfilling informants’ expectations is not unique to conflict/post-conflict situations, the stakes may be higher and the repercussions more severe in such tense environments.

Linked to the notion of silences is that of ‘invisibility’, whereby the perspectives and experiences of groups not useful to the politically powerful are not highlighted. Thus, in Doná’s examination of the experiences of Rwandan youth, only those whose experiences are useful to the state-building enterprise are publicly acknowledged and commemorated. Alternative experiences of youth are silenced through censorship, intimidation, or denial. Doná chooses to make visible the subaltern not only by seeking out the voices that are rarely heard, but by making explicit the use of youth voices and experiences in the state-building process.

Another way of dealing with silences, or of reluctance to speak frankly about sensitive issues, is to triangulate as much as possible between different sources. Thus, Akanji discusses his use of “colonial files, memoranda, minutes of meetings, field reports, Commission of Inquiry reports, government gazettes, magazines, newspapers and treaties” in addition to oral testimonies (using both structured and semi-structured interviews) representing multiple perspectives, and Ukiwo used multiple sources to develop historical reconstructions that revealed the vested interests of specific informants while at the same time revealing a more impartial view of events relating to the conflict. Many of the researchers in this volume describe their attempts to develop strong relationships with their informants prior to asking sensitive questions so as to put people at ease and foster a relationship of trust that would eventually lead to more candid interviews (for further discussion on the importance of trust, see Norman 2009). This is often facilitated by repeat visits to an area, particularly over a long period of time (see Hutchinson, this volume), although such protracted engagement is not always necessary, and trust may come from informants’ independent observations that the information they have divulged so far to the researcher has been treated confidentially and responsibly, or that the researcher clearly has the interests of their community at heart (which may, of course, invalidate the idea that the researcher should
strive for a position of neutrality). On the other hand, as Ukiwo points out, when informants ask the researcher point blank whether they have interviewed a particular person, the researcher may find themselves in the uncomfortable position of having to choose between “telling a lie and risking disapproval”; Ukiwo chose the latter. Others have chosen the former in order to protect themselves or their informants.

While it is sometimes the case that a researcher chooses a particular field site or country because of the conflict there, it is also the case that researchers find that a place that they came to know in peacetime later on becomes (or returns to) a state of conflict. Such is the case with Hutchinson’s work, where conflict in Sudan has gone through numerous ebbs and flows. When a researcher has a strong attachment to, or even arguably a debt to a community in terms of past collaboration, then determining the nature of the researcher’s responsibility can be a difficult task. What is the nature of responsibility – to act as witness, to help those who are in physical danger or who have been victims of the conflict? What about the government or another party to the conflict, who may have facilitated the researcher’s work in order to promote its own version of the truth, and upon whom the researcher may rely for physical protection and access to essential information? Hutchinson’s exploration of researcher responsibility in a context where she had been working for more than two decades, and in which questions of guilt and innocence were difficult to disentangle, suggests that drawing upon long-standing relationships and knowledge of customs can help protect the researcher and preserve space in which critical analysis can take shape even if it runs against the interest of those facilitating the research.

Since at least World War II when Ruth Benedict, Gregory Bateson, and others found their work being used to inform counterinsurgency operations in Southeast Asia (see, for example Eric Wakin’s *Anthropology Goes to War: Professional Ethics and Counterinsurgency in Thailand*), anthropologists and other social science researchers have grappled with the uncomfortable reality that their work is regularly used for purposes beyond their control. As noted above (and discussed at length in Miller and Scollon’s paper), members of the American Anthropological Association are currently engaged in a charged debate about the benefits and risks associated with conducting research for militaries in conflict zones. This debate hinges at least in part around the question of whether the researcher can control the use of their data and analysis so as to prevent any harm coming to their informants.
When asked if she would provide information to the US military, Margaret Mead famously responded that the military was welcome to read any of her published work. The implication was that anthropological knowledge should be public knowledge, and should not be carried out expressly for military purposes.

If anthropological knowledge is best seen as public knowledge, we are left to wonder whether our words can be used against either us or our informants; if they can, there may be justification for self-censorship, either in choosing to frame the research project in such a way that sensitive or uncomfortable truths are avoided, or else (perhaps more commonly) censoring ourselves in the presentation of what we have found. Several authors in this volume grapple with the difficulties of self-censorship, asking whether it is right to remain silent about injustice, for instance, to protect one’s own security or to ensure that one is able to return in the future to the same field site, or to protect the identity of informants even if such silence may mean the perpetuation of unjust or inhumane acts against individuals or groups.

One aspect of doing research in conflict contexts that is rarely given attention is the degree to which the researcher may face personal risk, and the steps they may take to protect themselves. In some cases, this neglect may be the legacy of discomfort that social scientists often feel about making themselves the subject of their research. Even after years of reflexive analysis, there still seems to be some concern that, particularly in such serious contexts as a conflict, it is somehow wrong to turn the lens on oneself. Other explanations may include a reluctance or inability of researchers to recognize when they face a grave threat. This may evolve from a sense of empathy with one’s informants whereby because they are not able to remove themselves from harm’s way, it is unreasonable for the researcher to do so. Whatever the other explanations, we feel that it is likely that at least part of the reason that researchers do not confront questions of risk and insecurity to themselves is because they are not prepared for this kind of situation. Very few research methods courses tackle the problem of researcher safety. Human subjects or institutional review board (IRB) committees rarely consider risk to researchers, and PhD students are not routinely asked to complete self-assessments of risk before entering the field. This is an enormous failing on the part of educators and their institutions.

In recent years NGOs have come to take more seriously the problem of staff security. Security training is offered to more staff, and provisions made for evacuation, protection of families, and the dynamics
of conflict and post-conflict scenarios that might affect aid workers’ own security and work. Academics working in these environments can learn quite a lot from these actors to make their own research experience safer and to be in a better position to assist those who they are working with who face similar threats. Julie Mertus (2009) describes some of the steps that can be borrowed from the aid sector to enhance the personal security of researchers.

In conclusion, we would hope that this volume contributes to greater attention in universities to preparing research students for the ethical and methodological (as well as emotional) challenges and dilemmas they should anticipate. As Elisabeth Wood (2006) argues, the point is to instil ethical principles to help prepare students to be able to make and adapt judgements in the field, rather than to encourage them to follow abstract rules.

To this end the volume includes a selection of papers, by a combination of highly experienced researchers and more recent entrants to research, from a mini-conference hosted by the Centre of African Studies at the School of Oriental and African Studies in London, in July 2008. The conference was a ‘thematic conference’ associated with the European Association of African Studies Centres, AEGIS. As in another related meeting held later that year at the City University of New York (CUNY), there was among many participants, especially current or recently graduated doctoral students, a palpable, explicit sense of relief and recognition: that the realities, dilemmas, and need for quick-witted ethical and methodological innovation in research in contexts of violence were being discussed openly when they had never adequately been aired during research training.5 Our hope, though, is that the issues raised at the conference and discussed in the following chapters are valuable not only to those researching in contexts of violence but also to a broader range of researchers, since the issues raised are far from confined to predicaments of extreme conflict.

Most obviously, the ethical and methodological challenges affect those conducting fieldwork research. This is reflected in the fact that most of the participants in the conference and contributors to this volume are anthropologists or political ethnographers. But, first, there are

5 Workshop on Field Research and Ethics in Post-Conflict Environments, Program on States and Security, The Graduate Center, City University of New York, December 4th–5th, 2008.
others who conduct fieldwork (qualitative and quantitative), including economists and geographers, to whom the issues confronted in this volume are relevant. And, second, those whose research on conflict affected (and other) contexts involves mainly desk-based studies of secondary, quantitative data are not immunised against ethical and methodological quandaries. In conceiving of and putting out a call for papers for the CAS/AEGIS mini-conference the organisers had hoped to encourage a dialogue on ethical and methodological challenges, in research on violence and violence-affected contexts in sub-Saharan Africa, across disciplines. The range of disciplines represented (and answering the call) in the conference and in this volume did not allow this dialogue to unfold as fully as might have been possible. This may signal quite how difficult it is to develop such a conversation and indeed how challenging it is to realise the often-vaunted ambitions for multi- or inter-disciplinary research in this field. The insights and experiences discussed in the chapters that follow do complement parallel initiatives – such as the CUNY workshop on field research ethics in post-conflict environments and a recent volume entitled *Surviving Field Research: Working in violent and difficult situations* (Sriram et al., 2009). However, given the frequently noted pretensions of economists to a privileged position among the social sciences, perhaps the main challenge in future is to develop a closer dialogue between economists, political economists, and other social scientists on these issues. This is especially important given the role of economists in policy debates and the apparent appeal of econometrics (often laying special claim to ‘objectivity’ and scientific rigour) to policy makers.

More broadly, a great deal of work – in different disciplines – is organised directly around policy questions and much of this work is directly commissioned by multilateral organisations or bilateral agencies and government departments. It is of great importance that policy makers should be aware of, insist on, and draw on work based on the application of sound ethical principles and on methods reflecting the kind of mature, informed flexibility recommended by this volume. Policy discourse is often prey to two influences: the influence of ‘factoids’ and the influence of ‘policy based evidence’ rather than evidence based policy (Cramer and Goodhand, 2010). Factoids are the sort of

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*Sriram et al. (2009) reflects the experiences, chiefly, of political scientists, international relations scholars and human rights scholars.*
institutionalised claims that acquire the force of robust fact through constant repetition within and across institutions and without being inspected closely. An example would be the claim that 50 per cent of countries return to war within a few years of signing a peace settlement meant to end a previous phase of violent conflict. Policy based evidence is the privileging of certain types of information or evidentiary claims to the exclusion of other, existing evidence and to the exclusion of further, challenging research, driven both by the practical concerns and institutional dynamics of organisations and by political interest and ideology. An example here would be the series of simplistic, often misleading claims about the role of narcotics in fuelling the resurgence of armed conflict in Afghanistan, the relationship between opium poppy cultivation and poverty, and the efficacy of eradication programmes. The production of factoids involves a kind of institutional inertia but it also often sails close to the wind of ethical generation of and manipulation of evidence. To the extent that such factoids rely on quantitative research, it is possibly much easier for quantitative researchers drawing on secondary datasets to protect themselves against the kind of reflection on neutrality and power relations in research that field researchers have to face. With policy-based evidence, the challenge is to break a collusion between interested policy makers and researchers/consultants that can all too easily ride roughshod over the experiences and interests of individuals ‘on the ground’. In both cases – that of factoids and that of policy-based evidence – the challenge is to insist on the same principles of honesty, transparency, respect, and sensitivity to power relations in the production and management of research and evidence that are at the core of fieldwork guidelines. We therefore commend the work reflected in this volume and in other related initiatives to policy making institutions and donor agencies just as much as to IRB personnel and research methods tutors.

Bibliography


The literature reveals that *Ife* and *Modakeke* belong to the Yoruba ethnic group. Structurally speaking, while the *Modakeke* people are of the *Oyo* sub-Yoruba identity group, the *Ife* people constitute the *Ife* sub-Yoruba group. The sub-ethnic conflict between *Ife* and *Modakeke* is one of the country’s protracted conflicts, and arguably the oldest in Africa. The history of the conflict dates back to 1849, when, twice in that year, armed hostilities broke out between the two communities. From 1881 to 1886, the communities again engaged each other in a series of armed confrontations. More recently, in 1997, all-out armed conflict broke out lasting four years. In between the cited periods conflict manifested itself through regular protests, violent riots, skirmishes, and court litigations; there was not a moment of respite. A study of extant literature reveals that the conflict originates in issues of identity, land rights, and the quest for internal self-determination. Over the years, successive governments have attempted to mediate between *Ife* and *Modakeke*, but with minimal success. Local non-governmental organizations, private individuals and the international community have also intervened at one time or other; they too have done so with mixed results.

When the nature of the conflict being studied is complex and protracted, daunting messages await the researcher, as I learned during my PhD research (2003–4) conducted some two years after the cessation of the most recent round of hostilities. Within the broader context of studying dilemmas in Nigeria’s democracy, I set out to understand the interplay of forces underlying the *Ife-Modakeke* conflict and its metamorphosis. My study focused on the role of migration; the pursuit of group rights; the nature and effects of state actions; elite and civil society involvement.
To appropriately address the objectives of the study and achieve a balanced perspective, the following questions were formulated: (i) why did the conflict continue for so long?; (ii) how and why did it metamorphose and escalate?; (iii) what roles, if any, did successive Nigerian governments play in generating, maintaining and managing the conflict?; and (iv) what roles, if any, did elites and civil society play? One overarching question was whether the conflict and its attendant effects could be viewed as evidence of failure on the part of the Nigerian state. Answers to the above questions, it was hoped, might help us understand why this communal conflict had remained unresolved despite the two ethnic sub-groups living side by side for over one hundred and sixty years. The violence had persisted in spite of the fact that the Nigerian state, under different dispensations (colonial, civilian and military), had grappled with this problem and its horrendous cost in human and material terms.

From the outset of the study, several hypotheses were formulated. The study hypothesized (i) that the Ife-Modakeke conflict metamorphosed and continued over time primarily because successive Nigerian governments failed to address its divisive issues; (ii) that a lack of respect for the group rights of ethnic others had lengthened the conflict, and (iii) that the politicisation and manipulation of these underlying issues by the local and national political elite had further fractured Ife-Modakeke relationships. Propositions were also laid out to synopsise the study’s line of reasoning. These included: (a) that the emergence and protracted nature of a conflict presupposes the existence of widespread relative deprivation or frustration among a sizeable number of community members; (b) that the changing nature of conflict may be a function of people’s attachment to their perceived group rights; (c) that when the state is perceived to be uncommitted to the group rights of its citizens, the likelihood of violent conflict is high; and (d) that violent conflict becomes inevitable when elites manipulate and politicise issues relating to the larger society.

Despite this guiding framework, conducting the research was still fraught with difficulties, which ranged from establishing a suitable research methodology to addressing pertinent ethical issues. The nature of these challenges and how they were managed is at the core of the present article. It is divided into four sections. The first section presents an historical overview of the Ife-Modakeke conflict; tracing its origin, evolution and nature across time and space. The second part examines the research methodology adopted for the study. The third
section interrogates the nature of the challenges confronted during the research, and how they were managed. The last section summarizes findings.

**Historical Overview of the Ife-Modakeke Conflict**

The history of armed hostilities and periodic violence between the *Ife* and *Modakeke* in south-western Nigeria dates back to 1849 when, according to available records, the communities fought each other twice. Again, during the sixteen years’ civil war (1877–1893) which followed and engulfed the whole of Yorubaland, bloody confrontations occurred, especially from 1881 to 1886. While there is no record of open armed confrontations during the British colonial era (1914–1960) and in the first decade following independence, hostilities resurfaced from the 1980s till the early years of the 21st century (2000–2002). In between the periods of intensive conflict, the relationship between the two communities was still far from cordial; antagonism and distrust prevailed. This can be seen, for example, in the court actions that sought legal redress for perceived injustices. The inter-war years were also characterized by protest actions, pockets of violent riots, and skirmishes.

Ever since its first occurrence, issues such as land rights, resource control and internal self-determination have underpinned the conflict. This requires some explanation. The *Ife-Modakeke* conflict is rooted in the massive displacement of the *Oyo* people in the 18th and 19th centuries, which was occasioned by the collapse of the *Oyo* Empire. The Oyo Empire was owned and run by an Oyo sub-Yoruba ethnic group. It was one of the numerous Yoruba empires and kingdoms in the pre-colonial period. The empire, however, collapsed due to persistent internal and leadership squabbles coupled with the invasion of Fulani Jihadists in the mid 19th century.

Following the collapse, many *Oyo* migrated to towns and cities belonging to other Yoruba sub-ethnic groups, including the territory of *Ife*. Historical records indicate that the relationship between the two groups was initially cordial and based on trust. In fact, it was so cordial that some of the displaced *Oyo* were drafted into the elite army of the *Ife* kingdom, while others entered its royal services. The extent of the communal cordiality and integration was evident, too, in frequent inter-marriages, including that of a displaced *Oyo* woman and...

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1 The *Oyo* Empire was owned and run by an Oyo sub-Yoruba ethnic group. It was one of the numerous Yoruba empires and kingdoms in the pre-colonial period. The empire, however, collapsed due to persistent internal and leadership squabbles coupled with the invasion of Fulani Jihadists in the mid 19th century.
Ife monarch, Ooni Akinyemero. This marriage later resulted in the birth of Crown Prince Abegunle Abewela (Fabunmi 1985, 116), who ascended to the throne of Ife kingdom in 1839.

Still, a turning point in the history of the relationship was reached well before Abegunle’s ascension. Already during the reigns of some of the successors to his father, relations between Ife and displaced Oyo had become so badly fractured that Ife started to regard and treat Oyo as migrants and strangers. The climate of distrust, oppression and repression against the displaced Oyo, especially in Ile Ife (the capital city of the Ife kingdom), resulted in Oyo being systematically subjected to cruelty (Johnson 1956, 525 and Akinjogbin 1992, 151–152). To help ease the problem of this fractured relationship, Ooni Abegunle Abewela, who had a maternal link with Oyo, then ordered their relocation to a place outside of the capital city. It was this group of relocated Oyo that took on the name Modakeke.2

But since the displaced Oyo (henceforth referred to as Modakeke) remained within the precinct of the Ife territory, the relocation was insufficient to address the crisis. According to Johnson (1921), “Ile-Ife and Modakeke being practically one town had only a small stream separating the two” (Johnson, 477). Even by dwelling in a separate settlement, Modakeke remained dependent of the Ife; they were still made to demonstrate loyalty to the Ife monarch (Ibid., 232). Also, as the Modakeke population grew in size, the parcel of land initially allocated for their resettlement became insufficient to meet their needs. The Modakeke then started leasing farmland directly from the Ife,3 a situation that created further distrust and animosity. In 1849, following the demise of Ooni Abegunle, the relationship deteriorated into bitter conflict as Oyo warriors at a settlement called Ibadan challenged Ife’s hegemony (Fabunmi 117, Akinjogbin, 153).

Colonial rule (1914–1960) did little to ease the tensions that revolved around the land rent (ishakole) Ife landowners demanded of Modakeke tenant farmers. When the latter pressed for an official enquiry, hoping that the authorities would pronounce the rents unfair and illegal,

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2 The literature is replete with the story of the contact between Ife and Modakeke and the subsequent development of hostilities between them. See for example, Johnson 1921; Ajayi and Akinitoye 1980, 280–302; Akinjogbin 1992, 148–170; Akanji 2007.
3 See the Ooni of Ife’s reply to the letter from the Baale and Council of Modakeke, June 25, 1947 in Oyo Prof. I, Vol. II, National Archives Ibadan. See also Akanji, 84.
the colonial government upheld the position of the Ife landowners through the law court. Devising a new strategy, the Modakeke then asked the colonial government and the Ooni of Ife to grant them a separate, rent-free homeland. Government ignored the demand. Later, in 1958, the Modakeke agitated for autonomy in the form of local government status, but, once again, their demand was turned down. Obafemi Awolowo, the Premier of the Western region, reasoned that “Ife town is one town and the request for a separate local council for Modakeke amounted to an attempt to divide a single town into two” (Daily Times, April 17, 1958, 7). The ruling deepened feelings of marginalization and alienation among the Modakeke, and, by implication, sustained the communal conflict.

After independence, the Ife political elite remained stoutly opposed to the autonomous local government status that Modakeke demanded. The Ife elite considered that granting local government status to the Modakeke was akin to balkanizing Ife territory.

The Modakeke request for self-determination within Nigeria remained an issue for Modakeke agitations from 1958 until 1996. However, it was the politicization by successive governments of the policy of territorial restructuring (i.e. state and local government creation), which aimed at assuaging the demands for more autonomy by ethnic and sub-ethnic minorities, that made the Ife-Modakeke conflict escalate. Specifically, despite the government’s readiness to restructure the country, the requests of the Modakeke people were repeatedly rejected. This was the case in 1958, 1976, 1981 and 1996, when the regional/state and federal governments respectively turned down, at the last minute, the request for a local government council. Such a council would have removed the Modakeke people from Ife dominance. In reaction to the refusal, Modakeke accused Ife of blocking the realisation of their dream. It was the immediate cause of the widespread violence which broke out in 1981 and 1996, which resulted in loss of life, internal displacement and the destruction of valuable property.

From the foregoing, it is possible to depict the fractured relationship as being a native-settler situation, whereby the Ife can be regarded as the native and the Modakeke as the settler. On the other hand, however,

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4 See Minutes of Representatives of the Modakeke Farmers and People with the Minister of Lands held in the Council Hall, Afin, Ife, 28th April 1955 in Ife Div. 1/1–113 National Archives, Ibadan.
the hostilities have been unprecedented in the history of Yorubaland. This last point is buttressed by the assertion that, “there were no conflicts in Yoruba history, whether in pre-colonial, colonial and post-colonial times which were as dangerous as that of Ife-Modakeke peoples” (Albert 1999, 167). The critical observation here is that the hostilities, which started as an intra-ethnic problem, have over the years widened into an issue of national and global concern. This raises the fundamental question of why and how the conflict mutated and degenerated. Answering this question is pertinent because the recurring nature of the conflict poses a threat to Nigeria’s nascent democracy, and raises concerns about the human rights of the members of the two communities, which the Nigerian state is expected to safeguard.

Research Methodology

Studying conflict falls within the realm of both archival and survey research. In its broadest conception, archival research is any research-based activity undertaken that involves the assessment of various types of documents and publications, while survey research uses questionnaires, in-depth interviews and observation of non-verbal clues to facilitate the scooping of vital information from living witnesses. It was anticipated that the survey results would enhance the findings from the archival search. However, in using the survey method, non-probability sampling procedures were adopted even though probability samples would have been more appropriate. The choice of non-probability samples was primarily caused by the lack of accurate population data and the (unconfirmed) fact that there are more Ife and Modakeke in the Diaspora (within and outside of Nigeria) than at home in Ile-Ife. Moreover, the choice of non-probability samples can be supported by the views expressed by Frankfort-Nachmias et al. (1992):

Although accurate estimates of the populations’ parameters can be made only with probability samples, social scientists do employ non-probability samples. The major reasons for this practice are convenience and economy, which under certain circumstances (e.g. exploratory research),

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5 Probability sampling refers to any technique that ensures a random sample; that is, a technique that ensures that every element in the sampling frame has an equal chance of being included in the sample. In contrast, non-probability sampling refers to any procedure in which elements have an equal chance of being included in the sample.
may outweigh the advantages of using probability sampling. Non-probability samples are also used when a sampling population cannot be precisely defined and when list of the sampling population is unavailable (Frankfort-Nachmias and Nachmias 1992, 175).

The questionnaire and oral interviews covered two of the four local government council areas into which the Ife and Modakeke communities are officially divided. Both indigenes (Ifé, Modakeke) and non-indigenes residing in the chosen areas were selected as respondents. The areas chosen were the Ife East Local Government Council Area (the local government in contention by the two communities during the 1997–2001/2002 conflict) and the Ife Central Local Government Council Area. The choice was informed by the history of the communal problem. Much of the history of the “genocide” perpetrated by the two communities took place in these areas. Using all four council areas for the survey interviews would have been unfair since the Modakeke people are mostly absent from the two areas not selected.

A total of 600 questionnaires was administered, out of which 231 usable copies were retrieved. The deficiency informed the need for supplementary follow-up interviewing. Besides, the study took its cue from the observations of Dane (1990) and Cramer and Franses (1999) that:

A survey research project may include as few as 100 participants or as many as 250 million (Dane 1990, 120).

Direct mail campaigns often provide large sample with a small number of positive responses. In analysing such samples…it is often expedient to omit the larger part of the abundant null observations (Cramer and Franses 1999).

In addition, for the sake of obtaining in-depth responses, the study focused on respondents within the age bracket of thirty years and above, irrespective of whether they were literate or illiterate. Respondents in this bracket were assumed to be more knowledgeable about the communal problem and to have witnessed one or more incidences of conflict. As an addendum, some community leaders and youth leaders (indigenous and non-indigenous) were also interviewed to enhance the validity and quality of the questionnaire.

Sources of Data; Methods of Data Gathering

Data was gathered principally from primary (archival, library) and secondary sources. I extensively reviewed and re-evaluated relevant
literature, including colonial files, memoranda, minutes of meetings, field reports, Commission of Inquiry reports, government gazettes, magazines, newspapers and treaties. I thus came across raw historical data beyond what is contained in the existing literature on the conflict. Library and archival research took place at the University of Ibadan; the British Council, Lagos office; the Nigerian Institute for Social and Economic Research (NISER) in Ibadan; the National Archives, Ibadan; and in private libraries within Nigeria. The information thus obtained was supplemented via structured and unstructured interviews with some of the living witnesses and principal actors in each of the two communities.

Moving beyond the archival and library search, the field investigation constituted the secondary source of information. Via the administration of the structured questionnaire, supplemented with unstructured oral interviews, I investigated the views of members of the two indigenous communities, as well as those of non-indigenous people residing in the area. This involved asking a set of questions, using the questionnaire schedule, to determine respondents’ perceptions on a wide range of issues. *Ife, Modakeke* and non-indigenous respondents were selected by way of random household sampling technique. It is important to point out here that the original plan was to cover six hundred respondents with two hundred respondents each of the three categories. The field investigation also targeted respondents of different social status, such as the local political elite, the youth, the workers, the educated and uneducated. However, as already seen, only 231 usable questionnaires were returned. The reason for the low response was due to the apathy respondents showed, especially towards questions that reminded them of the gory experiences of the immediate past. Also, many respondents were suspicious of my intentions despite repeated appeals and explanations. The effect of the low response was, however, reduced through the complimentary use of oral interviews with community and youth leaders among *Ife* and *Modakeke*.

To ensure accuracy and clarity both in the information sought and in the language used, the questionnaire was subjected to pilot testing. This involved critical reflections by the thesis supervisor, which resulted in a restructuring the questionnaire; some new questions were added and some removed. After testing the questionnaire on a sample of ten, only some minor further adjustments were needed.

For the unstructured oral interviews, intended to supplement the questionnaire results, respondents were selected via the purposive sam-
pling technique. (However, the snowball sampling technique, whereby one respondent identifies another, proved equally useful.) Thirty-four *Ife* and thirty *Modakeke* respondents, some of whom were community leaders and youth leaders, were interviewed. In these interviews I paid attention to both verbal and non-verbal information.

On returning from the fieldwork, the questionnaires were analysed by means of a content analysis for open-ended questions and simple descriptive statistical tools (such as frequency and percentage distribution) for closed-ended questions. Archival and library data were equally analysed for content. Overall, data analysis was theoretically guided by the multi-level approach of the generic theory/integrated framework of causality. This approach enabled me to explain the interplay of key forces in the conflict’s metamorphosis, especially those of the state, civil society, elites, migration, and the pursuit of group rights.

*Researching the Ife-Modakeke Conflict: Challenges and their Management*

The overall research approach proved useful as it allowed for flexibility on the part of the researcher, which in turn encouraged the researcher’s creativity and ingenuity.

As was expected in a study of this nature, the challenges were evident right from the start. Firstly, researching the conflict drew the ire of some people who expressed grave concern about the possibility of unearthing a conflict as old and protracted as the *Ife-Modakeke* conflict. Their concern was expressed by the principal of Oduduwa College, *Ile-Ife*, who openly, during an interview with him, remarked that he was skeptical that I would complete the research. The reason for his doubt was primarily because of the sensitivity of the topic, the experiences of the last armed confrontation (1997–2001/2002) in particular, which were still fresh in most peoples’ minds.

On a number of occasions, respondents asked “why the *Ife-Modakeke* conflict?” and wanted to know, in addition, whether I belonged to either of the two communities. The conflict was, no doubt, very well known to the average Nigerian, so why was further research needed? Answering the first question – why the *Ife-Modakeke* conflict? – proved more difficult than answering the second one. The second question was more easily handled as I revealed to all who were interested that I belonged to neither the *Ife* nor to the *Modakeke*. 
Not everyone was satisfied, though, especially not some elderly respondents who were quite knowledgeable about Yoruba history and bent on probing my identity a little further. Not satisfied with my way of demonstrating my neutrality, they then asked me to identify which of the sub-Yoruba identity groups I did belong to. The question was often framed in the following manner: “As a Yoruba man from Iwo town, as you claimed, are you an Oyo, or Ife, or Ijesha, or Egba man, or what are you?” While offering a straightforward response to this question can puncture any claim to neutrality in the research, my response and insistence that I was a Nigerian interested in understanding the conflict in order to make credible suggestions as to how it could be contained, doused every suspicion. The experience of meeting Chief Isaac Ajayi, a Modakeke chief and community leader, comes to mind here. This chief initially advised me first to go and study my roots and to determine whether I was Oyo or Ife. After my insistence that I was neutral and wanted to be so treated and seen, he quickly told me the story of the origins of my town, asking that I guess whether I was Oyo or Ife. Although I could not really meet his request, Chief Ajayi became of great assistance to the research. Like many other respondents and informants from Modakeke, he gave me what can be referred to as the Modakeke version of the history of the conflict, but his take on history revealed a lot about the Modakeke strategies for the prosecution of the conflict’s 1997–2001/2002 phase.

Regarding my choice of the Ife-Modakeke conflict for study, I reminded those who posed the question that no other conflict situation in Nigeria was as protracted as this one. Moreover, the fact that the conflict had remained unresolved for over one hundred and sixty year of continued coexistence was a pointer to the need to revisit the conflict. Though there were scholarly works on the subject, my research was perhaps one of only a few that employed an empirical methodology.

Another major problem confronting the study was in getting relevant and unbiased information. This was a very daunting challenge, and difficult to address, since the views of my respondents and informants were routinely tainted by their communal identity. For instance, no Modakeke respondent would ever comment in a way that would indict his or her community. This was also the case for a number of Ife respondents. Each one stuck to his/her community’s version of origin and causes.
However, the views expressed by indigenous (Ife and Modakeke) respondents and informants were weighed against those given by non-indigenous residents, which sometimes proved revelatory. Unfortunately, only a few non-indigenous respondents and informants were available for oral interviews, even after I promised that their identity and views would be protected and respected. (Non-indigenous respondents generally feared being attacked by their Modakeke or Ife hosts.) This affected the extent to which they were able to divulge information. And when they did so, it was with the utmost care and in the conviction that the information being sought was purely for academic purposes. Throughout the fieldwork, effort was made to assure and convince everyone of the strictly academic purpose of my research. Many remained unconvinced, however, which caused the poor response to the questionnaires.6

The attitude of many a respondent to the questionnaire survey can be gleaned from the violent reaction that occurred at Urban Day Secondary School, Mayfair, Ile-Ife.7 Prior to the administration of the questionnaires to staff, permission had been obtained from the school principal, who, along with some staff, had promised to respond to the questions. The principal had even encouraged the questionnaire’s distribution. Then, most surprisingly, both the principal and key members of staff became uncommitted, hostile and evasive when I asked them for the returns. This experience, right at the beginning of the fieldwork, created fear in the heart of this researcher and, in a way, prepared him for similar treatment elsewhere.

No doubt, most Ife and Modakeke respondents were nonchalant towards the questionnaire when compared with non-Ife and non-Modakeke respondents. This accounted for the shortfall in the number of useful questionnaires that were returned. Out of the two hundred questionnaires earmarked and randomly distributed among each of Ife, Modakeke, and the non-indigenous population, only sixty-five, sixty-nine and ninety-seven useable copies respectively were returned. While this shortfall was remedied through the use of oral interviews, they

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6 Suspecting that I was a government agent, a few Ife and Modakeke people asked me which government – federal or state – had sent me. In one Modakeke village, some respondents reacted by saying “Tell them [that is, the government] the conflict has not ended. If government does not do what is appropriate, we will fight till we die”.

7 Urban Day Secondary School, Mayfair is on the Modakeke part of Ile-Ife.
too were at times problematic. Most notably, some of the purposively selected principal community leaders and key actors, who could have offered useful additional contributions, remained unavailable. Their unavailability was due, in some cases, to death and illnesses, and in other cases, to sheer indisposition. Many of those who had witnessed sporadic violence in the colonial and the immediate post-colonial years were no longer alive, while some of the few remaining ones pencilled down for interview also died before they could be reached. Among the deceased was Chief Olaniyan Alawode (alias 007), a key community leader in Modakeke. Others selected for interview could not be contacted for different reasons. Thus the Head of the Modakeke community (the Ogunsua of Modakeke) turned out to be self-exiled somewhere in Ibadan, and every effort to contact him proved abortive. My inability to reach him made me drop all attempts to contact the Ooni (paramount ruler) of Ife, because of the need to maintain fairness and balance. Nevertheless, some other equally important community leaders and key players in the conflicts that engulfed the two communities between 1980 and 2001 (which in the main was the temporal scope of the study) made very useful contributions to the research.

A further factor hampering access to information was the seemingly closed-circuit nature of the Modakeke community, which heightened suspicions and caused threats to the life of the researcher. On some occasions, the researcher was accosted by people not included in the research. At other times, certain respondents and informants showed utter amazement at the researcher’s presence in the community without their knowledge. Two such experiences lent credence to this point. First, when the President of Modakeke Progressive Union, the umbrella body for all Modakeke home and abroad, was contacted for oral interview and the questionnaire survey, he reacted with shock and amazement. The President was jolted when he learnt from me that I had for some days already been carrying out the research in his community. With a charged countenance and a high-pitched voice, he demanded to know where I was staying and who my Modakeke host was. It was only when I explained that I was not staying inside

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8 ‘Informants’ volunteered information, mostly highly sensitive; ‘respondents’ responded to the questionnaire and/or gave solicited interviews.
Modakeke community, but travelled daily from a nearby town called Ilesa, that the President became more relaxed.

A similarly weird experience occurred at Akarabata where an unknown young man accosted the researcher and demanded to have a private audience that promised to reveal a lot of secret. The young man, in his late 20s or early 30s, had bitterness written all over his face, which sent shivers down the researcher’s spine. In the first place, the man accosted the researcher on the road in a manner suggestive that he had been trailing him for some time; he asked him if he were the person carrying out research on the Ife-Modakeke conflict. That the man was able to identify the researcher in the midst of so many people who were going about their daily businesses confused and disturbed the researcher, who then turned down the request for a private audience. This was a serious incident because the man had not been approached for either the questionnaire or oral interviews, and neither was his identity known. What these two experiences suggest is that the Modakeke community was a closed community, where non-members can easily be identified. Because of this, the researcher became extra careful and sensitive while still remaining committed to the research.

Alongside the closed-circuit nature of Modakeke community was the problem of an intra-communal crisis. At the time of the fieldwork, Modakeke people lived in a state of palpable fear due to a dispute involving commercial transport drivers. Rival factions of Modakeke community’s chapter of the National Union of Road Transport Workers (NURTW) were embroiled in feuds. The feuding polarized people, and fuelled suspicions and tension. On several occasions, the rival factions went on the rampage, destroying property and attacking each other. They also attacked members of the community who were perceived to be supporting rivals. The situation affected access to some important community leaders, such as the Ogunsua of Modakeke who had fled to Ibadan because his life was threatened (see above). This head of the Modakeke was accused of supporting one of the factions. To arrest the situation, policemen and members of the mobile police force were deployed to the community to safeguard lives and property.

The loss of information the two traditional Heads could have offered was overcome thanks to the invaluable contribution of another traditional ruler, the monarch (Oluwo) of Iwo town, His Royal Highness Oba Ashiru Tadese, who was interviewed in December 2003. In 1997/98, this monarch had been the chairman of a high-powered State Government constituted committee – the Osun State Council of
Obas’ Peace Committee\(^9\) – charged with investigating the immediate and more remote causes of the *Ife-Modakeke* conflict in an attempt to resolve it. The interaction with the monarch resulted in valuable first-hand information, some sensitive that could not have been accessed otherwise. Amazingly, the content of some of the Committee reports (findings and recommendations) that the monarch gave the researcher, was very different from what was contained in the existing literature.

Apart from the fieldwork, the archival and documentary search too had its challenges. For example, the search for relevant documents was slowed down by the problem of defaced or mutilated records, the unavailability of some files, and bureaucratic red tape. Some staffers of the Archives, who were supposed to help locate and photocopy official materials, seemed loath to help. This challenge, however, was managed through conscious and deliberate efforts on the researcher’s part to curry favour with the staff concerned. In the end, the researcher obtained many sources, which helped to reduce the impact of the problem of defaced or mutilated materials.

**Synopsis of Findings**

Findings from the archival and survey research confirmed that the communal conflict is rooted in migration. However, the quest for preservation of group rights, such as right to internal self-determination, was identified to have protracted the conflict. Furthermore, non-recognition of these rights by the Nigerian state had deepened the feeling of alienation and engendered mass mobilization by members of both communities. The study equally established that policies and actions by colonial and successive post-colonial Nigerian governments had fuelled the conflict. These related to the colonial tax system, territorial restructuring, and the nature of police involvement. Most of the respondents believed that local and national Yoruba political elites politicised the conflict by manipulating political appointments from the area. In the same vein, successive governments at both regional and state levels were identified to have manipulated information to further fracture *Ife-Modakeke* relationships. Within these communities, local elites and community-based groups were discovered to have helped in the provision of funds, arms and logistics. Overall, the

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\(^9\) Both Ife and Modakeke communities are in Osun state, south-western Nigeria.
study considered the protracted and recurring nature of the conflict as symptomatic of the failure of the Nigerian state to discharge its responsibility toward its citizens.

Concluding Remarks

Researching the *Ife-Modakeke* conflict proved challenging. The challenges, as demonstrated in this chapter, nearly overshadowed and frustrated the research methods adopted. To overcome the difficulties, the researcher devised different strategies to manage them and in the process unearthed a pool of relevant information. Also, and more importantly, by comparing the various data gathered in the course of the fieldwork and during library and archival searches, the research achieved its stated objectives. In writing up the final report, extant writings on the conflict were weighed against archival and documentary materials, against the views of the principal actors, and against the statistically treated responses to the questionnaire survey. The outcome was the presentation of a balanced, empirical study which advanced existing knowledge of the age-long *Ife-Modakeke* conflict.

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On the occasion of the 15th anniversary commemoration of the 1994 Rwandan genocide, one of the testimonies given during the opening ceremony held in the Rwandan capital Kigali was that of a 15-year-old girl born during the months of the genocide in the stadium where her family had sought sanctuary. She publicly recounted the story of her life that began as the genocide unfolded. She told how she lost both parents soon after the end of violence, about her subsequent placements in orphanages and foster care, her time spent on the street and finally her residence at a boarding school where she was then completing 6th grade. Her testimony was one of many speeches given by survivors, perpetrators and officials to reflect on the theme of the commemoration: Icyzere (Hope).

In conjunction with the anniversary, a ‘One Dollar Campaign’ was launched on April 4th 2009 with a lively concert performed by emerging Rwandan artists, many of whom had been children at the time of the genocide. The international campaign aims to raise funds to build houses for ‘genocide orphans’ who spend their school holidays at boarding schools because they have no place to go. These two anecdotes show the significant role that children and young adults play in post-genocide Rwanda as narrators of violence, and the human costs they bear both as agents for social change and as beneficiaries of transnational projects that address the ongoing challenges ensuing from the legacy of the past.

Christiansen and Prout (2002) write that research with children is embedded in social life, where children are both ‘research’ beings and ‘social’ beings; in research and social life they can be viewed as objects,
subjects, actors and participants (and co-researchers). The examples given above show Rwandan children’s position in social life as objects of assistance (orphans), as social actors during commemorations (narrators), and as participants in social change (advocates).

In research, they are ‘objects’ when their lives and experiences are investigated from the perspective of others such as parents, social workers or government officials, and where they have no power over the creation or production of knowledge about them. They are ‘subjects’ when their involvement in research is limited to that of respondents who answer questions developed by others, and where they have very limited power over the research process apart from deciding what information to convey or withhold. They are ‘actors’ capable of articulating their experiences when they are in dialogue with the researcher. They inform the content of the research not simply as respondents to pre-determined questions but as informants knowledgeable about their experiences. They are ‘participants and co-researchers’ when they are involved, informed, consulted and heard within the new social science methodologies that see research as the co-production of knowledge. In social life, children can be objects of policies and assistance, subjects of welfare programmes, social actors in inclusive programmes and participants in advocacy projects.

In this paper, I reflect on the process of increasing the articulation of my own position as a ‘research’ and ‘social’ subject in researching children and violence in evolving socio-political contexts. I analyse political violence both directly – through accounts of the Rwandan genocide itself – and retrospectively, through an analysis of the continuing relevance of the genocide legacy for the lives of those who were children at the time of violence. Robben and Nordstrom write that “understanding violence should undergo a process of change and reassessment in the course of fieldwork and writing” (1995, 4).

I show the value of long-term research on childhood and conflict, which enables the researcher to revisit children’s public, personal and sensitive stories of violence over time. I also wish to discuss the ways in which my understanding of my position as a researcher underwent changes over time on the basis of my social status in Rwanda. In other words, I want to explore the relationship between researcher and participant when both are simultaneously social and research subjects in highly charged socio-political environments.

I went to Rwanda at the beginning of 1996, a year and a half after the end of the genocide, and lived in the country for four years until
the end of 1999. This was not my first time researching violence, as I had conducted fieldwork with Guatemalan refugees living in camps in Mexico and with returnees to Guatemala. I was also, I felt, intellectually prepared for Rwanda, having attended a year-long seminar series on Rwanda in 1995, and having read on the topic. Yet in so many ways I was unprepared for the complexities of research and life in post-genocide Rwanda.

I went to the country as the director of a unit that was responsible for developing psycho-social training and research with children living in difficult circumstances. Later I worked as technical expert for a foreign donor and was responsible for social programmes for women and children. I was seconded on a part-time basis as technical adviser to the Rwandan Ministry of Youth, Sport and Culture to assist with the development of policies for marginalised youth, specifically street youth.

During my time in Rwanda, I was involved in research consultancies on family reunification, street life involvement, foster care and general child protection issues for non-governmental organisations, the government and the United Nations. In 2000, I returned for four months as a United Nations consultant to lead a policy-oriented research project on foster care arrangements for separated children, and I went back for five months in 2009 to conduct fieldwork on bystanders to the genocide. When I recently returned, those who had been children at the time of the genocide had grown up to be young adults in their 20s and 30s, while a new generation of children born after the genocide was living with its legacy.

My research with children is situated within processes of rapid social change in Rwanda, and it aims to show how researchers themselves help to render visible or invisible specific children’s stories in relation to the socio-political context in which they work. Specifically, I argue that research with children in Rwanda is framed by a nation-building project focussed on the genocidal past whose ideology shapes the position of children as social subjects and as research subjects. Consequently, children’s stories that fit within nation-building projects are publicly disseminated and displayed at the expense of those that sit less comfortably within national discourses, which are marginalised or become taboo topics.

A number of scholars have written about the ethical and practical challenges of conducting research in situations of violence and post-violence. These include edited collections covering a range of epistemological, ethical, and practical issues in researching socio-political
violence (Nordstrom and Robben 1995; Sluka 2000, Sriram et al. 2009). They deal with issues like access, veracity, security, identity, objectivity and behaviour (Sriram et al. 2009), ethnographers’ experiences of living with fear in the field (Green 1995), negotiating dangerous fieldwork (Hoffman 2003), and rethinking conventional ethical guidelines in light of anthropological research on violence (Kovats-Bernat 2002). Most of these writings address general methodological issues, and they cover mainly fieldwork challenges. Only a few of them (Boyden and de Berry 2004; MacMullin and Loughry 2000) examine the specific methodological challenges of researching children and political violence. In this paper, I aim to contribute to this increasing body of literature by examining some of the specific questions arising from conducting research with children in conflict.

In this paper I develop two themes: a) the place of research in changing socio-political environments, and b) the visibility and invisibility of children’s experiences of violence. The paper is structured in three main sections, which trace somewhat flexibly the chronology of my involvement with Rwanda since 1996. In the first section I describe how, somewhat naively, my first research projects fitted and contributed to the national narrative of genocide and nation-building; in the second I identify the challenges of attempting to research those stories which did not fit with the dominant narrative; and in the third I offer an account of how the socio-political context influenced my work as a researcher and as an individual.

**Children and violence in Rwanda**

The Rwandan genocide, which took place over three months between April and July 1994, resulted in the deaths of almost one million Tutsi and ‘moderate Hutus’ under the orchestration of Hutu extremists and with the involvement of civilians. At the time of the genocide, Rwanda’s population was estimated to be seven million people, of which approximately 84–85% were officially registered as Hutu, 14–15% as Tutsi and 1–2% as Twa. More than a million people died between April and July 1994 and over one million Rwandans have been judged in *gacaca* courts – a traditional grassroots justice system (Branningan and Jones 2009) – with accusations ranging from stealing to inciting genocide and killing. Among those who died, those who killed, and those who became displaced, many were children (Cantwell 1997).
During the genocide, children were targeted on their own and as members of families. While Tutsi and Hutu males as well as members of opposition parties were targeted at first, women and children became open targets of the genocide machine in May (Des Forges 1999). Tutsi children, described in the media as ‘small rats’, were to be killed to avoid the mistakes made in 1959, when Tutsi families, including children, were forced to flee and grew up in exile, especially in Uganda, where they became fighters in the Rwandan Patriotic Army (RPA) against the Rwandan army inside the country.

The public nature of the killings that took place across the country in 1994 meant that all children were involved in violence, as direct or indirect targets, witnesses, members of ‘moderate’ families, children of genocide perpetrators, or refugees. The victory of the RPA in July 1994 was accompanied by the mass exodus of almost two million Rwandans, many of whom repatriated in the years following the genocide and the closure of refugee camps in Congo. Although the genocide ended in July 1994, low-intensity violence continued. During the years I worked and lived in Rwanda, sporadic killings continued to take place across the country, and insecurity was present especially in the north-west of the country where many individuals affiliated to the political party of the previous regime conducted incursions that created instability. During fieldwork I conducted in 2000, visits to the north-west of the country still took place under military escort due to ongoing insecurity.

Sporadic and revenge killings continued to take place after the end of the genocide, and were carried out by both supporters of the genocide regime as well as the RPA, whose massacres have been documented (Des Forges 1999). Occasional violence still continues: for instance during the memorial ceremonies that commemorated the genocide in April 2009, random killings were broadcast in the media, and Rwandan newspapers reported that a grenade had been thrown into the grounds of the Kigali Genocide Memorial Centre in Kigali. If the genocide is internationally viewed as the incomprehensible apex of political violence, for many Rwandans this was the most extreme episode amidst a much longer history of violence which preceded and followed it, and whose impact on social and research life is still profoundly felt.
Through the western gaze, children are constructed as innocent victims of violence, passive beneficiaries of humanitarian projects, or threats to security (Boyden and Hart 2007). In contexts of violence and post-violence children simultaneously occupy ‘neutral humanitarian’ spaces as victims and beneficiaries, and ‘socio-political’ ones as intentional targets of violence, perpetrators or combatants.

For the post-genocide Rwandan Government of National Unity, the project of unity and reconciliation is centred on the legacy of the genocide, and on the view that children are the ‘future’ of Rwanda. As some informants told me, there is a perception that it is too late to change the views and prejudices of the older generation, hence the emphasis on youth and youth cultures. Unity is symbolised by the slogan ‘We are all Rwandans,’ and – is codified in a 2002 criminal law that punishes public incitement to discrimination or divisionism (Rwanda 2002, Article 8).2

Rwanda shares a common religion, language and culture, and its history of unity is being reconstructed with reference to a mythical pre-colonial past during which Rwandans lived harmoniously together. This period was followed by colonialism that codified ethnicity in identity cards and led to post-colonial divisionism, ethnic cleansing and the ultimate crime of genocide. Finally, post-genocide Rwanda is going through a process of transitional justice and reconciliation predicated upon a return to pre-colonial unity.

Behind these essentialised historical narratives and a “monopoly of knowledge construction” (Pottier 2002, 202), contested voices and counter-narratives are silenced (Breed 2008; Pottier 2002). The memory of the genocide is central to many post-genocide initiatives in the areas of justice, reconciliation and poverty reduction, as well as in

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2 Any person who makes public any speech, writing, pictures or images or any symbols over radio airwaves, television, in a meeting or public place, with the aim of discriminating [against] people or sowing sectarianism [divisionism] among them is sentenced to between one year and five years of imprisonment and fined between five hundred thousand (500,000) [US$ 1000] and two million (2,000,000) Rwandan francs [US$ 4,000] or only one of these two sanctions. (Rwanda 2002a, Article 8).

The law defines divisionism as “the use of any speech, written statement, or action that divides people, that is likely to spark conflicts among people, or that causes an uprising which might degenerate into strife among people based on discrimination” (Rwanda 2002a, Article 1).
many policies and welfare initiatives to support children. The genocide and its legacy are recurring reference points in national discourses that remind the population and the international community of the dangers of past ethnic violence, and that reiterate the value of a post-genocide national project of unity.

Within this context, children’s experiences are not ‘neutral’ but are dependent on their relative value within the state vision of unity and reconciliation. Stories that support or are perceived to be of value to the nation-building project are easily accessible, while those that are more controversial are marginalised or banned. Conducting research in such highly charged socio-political environments calls for an understanding of childhood and of the power of research data in socio-political context. As Christiansen (2004) writes, viewing power as inherent in research emphasises that this is a practice that is part of social life rather than an external contemplation of it.

**Constructing national discourses of children and violence:**

*nai ve research in socio-politically charged environments*

Analysis of government documents, interviews with government representatives, and participant observation of official ceremonies help the researcher to collect publicly available information that implicitly reveals which categories of children are central to the nation-building project, and how their life experiences are represented. In contexts of post-conflict, official discourses are helpful as data because they describe the meta-narrative of the country’s history, its values and vision, and the social position that children occupy within them.

Between 1996 and 2000, my ‘official’ position as representative of legally recognised bodies meant that information gathering took place within the framework of cooperation with the Government and non-governmental organisations, mostly (and unintentionally) in support of government-approved initiatives. Access to the field is usually described as challenging by researchers who want to conduct research in conflict and post-conflict contexts (Sriram et al. 2009). This is due to suspicion about the uses of data, and the frequent complaint that information is not fed back into the country where it was collected. Most of the discussions surrounding access and information-gathering stem from an unarticulated assumption about research (that it is framed by academic goals) and positionality (that being a researcher
is the main professional identity). These discussions take for granted that the author and the audience share a common social world, that of academia, and that fieldwork is conducted by independent scholars who are based in universities situated outside the countries where fieldwork takes place. However, it is more and more often the case that research is carried out by non-governmental organisations and international agencies independently or in collaboration with universities, some of which may be local.

Because of my multiple identities, once my status in the country was officially approved, I did not find access to information problematic. I could ask to see child policies or to discuss programmes; I was granted formal interviews with government officials and professionals, and I regularly engaged in informal conversations with different social actors in unofficial social spaces. I was able to carry out participant observation routinely in different environments, and to interview children and families across the country. Terms of reference for conducting research with children living in difficult conditions were collaboratively developed in consultation with advisory committees composed of representatives from non-governmental, governmental and international agencies. This meant that different stakeholders were familiar with and supported the research enterprise, and that research findings were openly discussed with the aim of developing practice and policy recommendations.

An evaluation of a family reunification programme for unaccompanied minors (Doná et al. 1998) conducted in partnership with an international non-governmental organisation highlighted the need for follow-up after family reunification, and when families and children identified this limitation of the programme as the most important one, changes to the existing and well-functioning programme of family reunification in the Great Lakes Region were suggested. In particular, it was recommended that an additional component – Follow-up – be added to the existing process of Identification, Documentation, Training and Reunification (IDTR). Research with children in foster families (Doná et al. 2002) revealed the abuse that some fostered children experienced, which was reported to local social services for action. Research on street life involvement (Veale et al. 1998) conducted in collaboration with the Ministry of Youth led to the development of a policy on street children.

Children themselves were involved in research and social life in different roles: objects, subjects, actors and participants and co-researchers.
They were objects of research on their social care needs when we looked at how they were represented in welfare policy documents and programmes. They were subjects when we asked them pre-determined questions about their experiences as separated children and the impact of the conflict on their lives; they were social actors when we worked with them to understand how they were interpreting and articulating their concerns. And they were co-researchers in helping us to develop the research strategy in the study on foster care (Doná 2006).

In social life, separated children were objects and beneficiaries of policies and programmes on care arrangements, family reunification and general integration into post-genocide society. They were subjects and social actors in programmes that involved them in decisions about care arrangements. And they were participants in the construction of national discourses on the role of young people in post-genocide Rwanda. The value of this kind of research for assisting the process of social reconstruction during and after conflict cannot be underestimated. Such research is greatly embedded in social life, and it has the potential to positively impact on social relations, but also to exacerbate conflict. It is for these reasons that it is imperative that its position in social life be critically analysed.

In discussing the Rwandan government monopoly on knowledge, Pottier (2002) is critical not only of the media propaganda strategies implemented by the government, but also of the ignorance of many western (especially anglophone) journalists who were covering Rwanda for the first time and could not see past the Rwandan Patriotic Front as saviour and the international community as guilty bystander. The propaganda success with western donors and media could not have been accomplished without its suppression of competing narratives, alternative voices and independent media within Rwanda.

These comments describe not only my position and that of most aid workers and researchers who went to Rwanda soon after the genocide, but also the organisational ethos of most agencies I worked for and collaborated with. The dominant discourses of post-genocide Rwanda, which naive researchers and practitioners helped to validate, were reliant on familiar concepts of victimhood and humanitarianism. They helped to identify and render visible specific categories of children in need, and to treat their experiences of violence as neutral ones. Unbeknownst to many researchers and practitioners, our work was predicated on unarticulated assumptions about children’s social positions vis-à-vis the genocide which constructed them as genocide
victims or as a threat to security. As genocide victims (see also ‘genocide orphan’ at the beginning of the paper) they were in need of assistance, while as potential threats to social stability (street children, minors involved in the conflict, etc.) they were in need of re-education.

The construction of two dominant groups of children meant that those who fitted within these categories became visible subjects of research and – social programmes, while those who belonged less comfortably within national discourses became invisible (Donà and Veale 2011). Visible categories comprised separated children, orphans and especially genocide orphans, children living in child-headed households, those in orphanages, street children, children in prison and those involved in the genocide. Invisible and unspoken were the experiences of children living in orphan-like conditions due to the imprisonment of their parents, children of mixed ethnicity, or children remaining or returning from exile. Generally, research conducted within the dominant framework was unquestionably accepted and made visible, while that which challenged this dominant framework was criticised, marginalised and made invisible.

Upon my return in 2009 to follow up on the lives of those who had been children during and after the genocide, my position and field experience differed substantially. I went to Rwanda as an anthropologist working at a university in the United Kingdom, and as somebody who had worked in Rwanda and was interested in understanding how children’s lives had changed. Access and information-gathering became much more challenging. On more than one occasion I contacted government departments or local authorities to get an update on their policies and programmes only to be kindly ‘passed on’ to somebody else. Various individuals told me that they were not the appropriate ones to answer my questions, or that I needed an official introduction letter from the minister or the Secretary General or a higher authority.

Not only had my ‘official’ role changed, but a different socio-political climate also reigned. The increased numbers of academics using Rwanda as a case study, as one Rwandan colleague told me, and writing papers that were critical of government initiatives meant that government staff were reluctant to interact with anybody who did not have written formal approval by the highest echelons of command in order to ‘protect’ themselves, as one informant explained. The familiar methodology through which I had gained access to official policy documents, key informants (many of whom had moved on) and participants,
and which would have enabled me to research the changes in official discourses about children’s positions in society, became unsuitable in this new context. As my visibility in Rwanda diminished, the desire for invisibility by potential informants increased. For me, decreased visibility meant reduced access and the need to revisit my methodology; for them, increased invisibility had become a strategy for survival and self-protection.

Soon after my arrival in 2009, I explained to a Rwandan colleague the purpose of my return and that I wanted to collect life histories of ordinary Rwandans, including children and young people. His reply was that “75% of somebody’s life history would be missing.” He went on to explain that people would not want to talk about their experiences in [the Democratic Republic of] Congo, a sensitive topic because of its association with the pre-genocide regime; they would not tell me if they had been in prison or had relatives in detention because of the shame they felt, and they would not want to discuss their ethnicity because of its possible criminal repercussions. Subsequent conversations and participant observation confirmed the accuracy of this initial comment, where silences accompanied invisibility and were prefaced by indirect references. Most sensitive information I obtained was prefaced by the phrase “let me give you a case, let me give you an example.” It was through indirect references that preserved the anonymity of the information that participants felt safe enough to talk about controversial and sensitive issues.

In this section I have highlighted some of the issues arising when conducting research in socio-politically charged contexts, which, I note, rely on unspoken assumptions about the positionality of both researchers and participants. Their visibility and invisibility contributed to the construction and validation of the visibility and invisibility of different categories of children as research and social subjects.

In the next section, I will continue to discuss the place of social research within social life. I will explore in more depth the tension between visible and invisible stories by examining how the research and practice of post-genocide memorialisation construes and consolidates the visibility of children of the genocide and the invisibility of children of war. The comparison of national narratives and fieldwork data enables me to examine the de-contextualised representation that characterises memorialisations of violence, in which children are instrumental in consolidating a national narrative of violence against humanity rather than of socio-political conflict.
Remembering violence: spoken, unspoken and unspeakable deaths

Concurrently with official discourses, children’s stories enter the public domain in communal events such as acts of remembering and memorialisation, which fulfil various functions. Through the recall of the violent past and the emotional affirmation of grief, fear and anger associated with loss, mourning may be directed to instrumental ends that include nationalism, political transition, modernisation, revolution, capitalism, institutionalised justice, human rights, and national projects (Saunders and Aghaie 2005). Memorialisation becomes an activity through which a mythical truth is conveyed to construct a new identity (Gillis 1994).

Usually, research on violence and its aftermath focuses on the experiences of children who are alive. Yet representations of children’s deaths during commemorative ceremonies and in memorial sites can offer valuable research data about the socio-political value that children have in post-conflict societies. As Véronique Tadjo wrote in The Shadow of Imana on the publicly displayed remains of those killed during the Rwandan genocide “these corpses bear witness, and will have no burial. They are nothing but bones… But these dead are screaming still… This is not a memorial but death laid bare, exposed in its rawness” (2002, 12).

Across the country the remains of the children who perished during the genocide are visible in churches and schools, sites of massacres which have been converted into memorial sites, where their bodies are left unburied and exposed to remind Rwandan society and the international community of the tragedy. At Ntarama Memorial Centre, exercise books of children are placed in separate boxes to highlight the abrupt ending of their lives. In the Kigali Genocide Memorial Centre, which opened in Kigali in 2004 to mark the 10th anniversary of the genocide, the deaths of children killed during the genocide are commemorated in a formalised and stylised manner. Children’s deaths are given special value and are displayed in a separate exhibition space, the Children Memorial (www.kigaligenocidememorial.org). Here their photos are accompanied by a caption that invites the viewer to empathise with them by learning about their hobbies, behaviours, significant others in their lives, and lastly how they were killed.

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3 Accessed on July 31st, 2009
Across the country, children’s deaths are remembered and represented via humanistic, personal and empathic lenses that construe them as innocent victims of violence. Local histories and socio-political dynamics of violence are absent from public memorialisation, where their deaths serve to represent the genocide as a crime against humanity rather than as an act of socio-political violence. The deaths of these children make them both objects of memorialisation and also symbolic social actors in the project of nation-building.

My residence in the country soon after the genocide and subsequent returns allowed me to observe the evolution of the value that certain deaths gained over others. ‘Genocide’ deaths, which occurred as a direct action of genocide, inhabited highly visible public spaces in post-genocide Rwanda, while ‘war deaths’ of children who died during the genocide and ongoing war became marginalised from public discourses, remained unspoken or became unspeakable.

While the deaths of those (Tutsi) children who were direct targets of violence because of their ethnicity ‘speak’ of the violent past, those of the children who died as members of ‘moderate Hutu’ families or of political opponents of the regime occupy ‘unspoken’ places in memorialisation. The war deaths of the young Rwandans who died while fighting for either the army of the RPA or the previous government are also absent from these representations of violence. And, it was only during my most recent fieldwork that I obtained information about the deaths of Twa children, the third ethnic group in Rwanda whose members refer to themselves as the ‘historically marginalised group’ and who are excluded from representations of the genocide.

‘Unspeakable’ are the lost lives of Hutu (and some Tutsi) children who died in massacres committed by the RPA as it advanced towards the capital and soon after the genocide (Des Forges 1999). These deaths have no place in national memorialisation because they challenge the heroic image of the RPA as the saviour and the victorious army that stopped the genocide. Hutu bones (including those of children) scattered throughout the Congolese forest, too, have no national place in memorialisation for they are all viewed as génocidaires and fail to elicit the empathetic identification of the international community (Ngwarsungu 2008). Memorials “invite us to remember, but they are far from neutral and universal; they are influenced by their locations, constructed from various subjects’ positions, and embody the ideals of those who erect them” (Ngwarsungu 2008, 83). Additionally, they are filled with conspicuous absences and organised amnesia (Saunders and Aghaie 2005).
Field research was situated within an evolved post-genocide national narrative of genocide, which memorialisation contributed to in order to make legitimate. While in the late 1990s multiple kinds of ‘genocide’ and ‘war’ deaths were (albeit quietly) narrated by respondents in our studies, mentioned in discussions about ongoing crises (north-west, refugee camps, return, displacement), or recalled by families during conversations about the past, over time the sensitivity of this information increased as the process of consolidation of a specific national narrative, reliant on a clear and simplified account of the genocide strengthened. Information that challenged or highlighted uncomfortable aspects of the national narrative became unspoken or unspeakable.

Outside Rwanda, when I interviewed young people in exile, I was surprised and relieved at the ease with which ‘unspeakable’ deaths inside the country were openly spoken about. For them, these fatalities were central and occupied visible and valuable spaces in the construction of a different (and oppositional) narrative of war. Inside Rwanda, publicly unspeakable deaths are usually remembered behind closed doors. I could obtain information about them from trusted friends or indirectly through colleagues who collected them for me from trusted individuals. During one interview, a young respondent became particularly emotionally upset while recounting the deaths of family members and relatives which took place during massacres committed by the RPA, losses he could neither mourn nor talk about in public.

Controversial deaths also impact on the existences of children who survived. One of the most sensitive issues I encountered when researching how those who were children at the time of the genocide grow up in post-genocide Rwanda was the predicament of children of mixed ethnicity. Their belonging to unmentionable ethnic groups places these children in uncomfortable and painful spaces, and positions them in between the post-genocide, value-laden social categories of rescapé (Tutsi survivor) and génocidaire (Hutu perpetrator). During memorialisation it is especially difficult for these young adults to mourn openly the fact that they have experienced losses on both sides.

At the beginning of my recent fieldwork, knowledgeable about but unaware of the impact of the law against divisionism on daily life, I used the term ‘mixed ethnicity’ to refer to these children, thinking that because I was not specifically using ethnic terms I could enquire about them. During a visit to a memorial site I asked the guide how the deaths of children of ‘mixed ethnicity’ were represented in memorial...
sites like the one I was visiting. He stepped back abruptly, raised his fingers to his mouth to silence me while looking around suspiciously to check if anybody was present. Their silenced deaths are the price to pay to build unity in post-genocide Rwanda.

The simple use of the term ‘mixed’ could get me into trouble, and I was explicitly told that children themselves would answer my questions but then would go home and denounce me. The explanation for why this might happen was interesting and made reference to the mythical past: as I was white and it was the white colonial power that had fixed ethnicity, I might be seen as wanting to bring back ethnic divisionism.

Like many Rwandans who use indirect language to talk to one another, I too had to find discreet expressions that I could use to describe the unspeakable. I uneasily settled for the permissible terms *rescapé* and *génocidaire* to refer to these children as those who had one parent who belonged to the group now called *rescapés* and the other parent who belonged to the group of which some members were considered *génocidaires*. It was not an ideal choice, as it belonged to the dominant post-genocide discourse of transitional justice and reconciliation. The impossibility of using ethnic terms meant that I had to find new ways to enquire about ethnicity without making direct reference to it. I compromised, this time with greater awareness of the constraints I was under, and made my choice on the basis of my observations of how Rwandans indirectly discussed ethnicity in post-genocide Rwanda.

During my first stay in Rwanda my research simplistically fitted within dominant and officially acceptable narratives of conflict and childhood, and examined what was visible and spoken about. Over the years, a greater awareness means that my research approach has shifted towards a much closer examination of children’s experiences which remain unspoken or are fragmentarily mentioned in passing, and I compare them to those that are visible and spoken about.

As ordinary Rwandans continue to confront the spoken, unspoken and unspeakable realities of their existence, I find myself in a different (less consequential) yet similar predicament as social and research being: to enquire into unspoken and unspeakable subjects could lead to my being accused of divisionism, negationism, or even worse of harbouring a genocide ideology.
To conclude the analysis of the relationship between researcher and participant as social and research subjects, I now describe the impact of fieldwork and its timing on my personal and research life. During the first period spent in Rwanda in the 1990s, I focussed on my professional skills and roles in the belief that I would ‘help’ by being technically competent and ‘get the job done’ to the best of my abilities. Upon reflection, this was my way of coping with the political and emotional complexities of life in Rwanda shortly after the genocide, where the population was still living in a state of collective shock and was slowly beginning to take stock of what had happened.

As I wrote at the beginning of this paper, while I knew something about Rwanda I did not know about the complexities of living and conducting research in such a multifaceted post-conflict society. As the difficulty of the situation overwhelmed many of us as researchers and aid workers, we looked at known and visible others for orientation on how to behave. To cope I retreated and socialised with other foreigners, I only tentatively questioned the way in which the genocide and its aftermath were simplistically represented by media and officials in Rwanda, and I focussed on the technical (methodological) aspects of my research/work. In an unknown and complex environment, informed by the limitations of my position as an outsider and having internalised international guilt, I rarely challenged the socio-political context in which I operated, although I did experience a degree of uneasiness that I could not articulate.

Having returned to Rwanda after many years, I observed how a new generation of foreign (young) researchers, knowledgeable about Rwanda rather than about what it means to live and conduct research in the country, was behaving very similarly to the way researchers and aid workers did over a decade ago. I experienced a degree of familiarity when listening to ‘complaints’ that Rwanda was a difficult place in which to work, or when hearing some unquestioningly describe their investigations under the auspices of local or national governmental structures.

As more researchers consider Rwanda a ‘case study’ for their intellectual interests, and many have in recent times taken interest in the study of gacaca as the exotic traditional system of transitional justice, fewer seem to question their positions or those of their informant participants as social and research subjects. More importantly, few openly
discuss the socio-political uses of gathering and disseminating information which *gacaca* epitomises.

The timing of my recent visit was influential in framing which information I obtained and its power in social and research life. *Gacaca*, the grassroots justice system that was initiated in 2001 and that officially closed in 2009 (though a few courts continue to function), is a central feature of the contemporary social landscape of the country. *Gacaca* has been successful in many ways by enabling survivors to hear what happened to their family members, relatives and friends, by enabling innocent individuals to be cleared of charges of genocide and released from prison, and by charging those who are communally judged guilty.

As one informant told me, initially *gacaca* was perceived as an information-gathering exercise in truth-finding. Children, for instance, would report what they saw their family members and relatives doing during the genocide, without realising that this would lead to their being accused by *gacaca*. Over time, *gacaca* inhibited the open sharing of information and resulted in increased levels of social distrust, and in discreet use of information for fear that it could be used against the person giving testimony by his/her relatives or neighbours. As *gacaca* unfolds, invisibility is viewed as the best strategy for survival, and secrecy and silences are its correlates. Individuals prefer not to speak of personal experiences or to make direct comments for fear that any information may be used against them or those they know. They are constantly reminded of the many politicians and journalists who were openly and visibly critical and as a result were arrested or forced into exile (Waldorf 2003). Information is best shared through indirect references to many sensitive topics and experiences, and by the narrative technique whereby the speaker says “let me give you a case, let me give you an example” telling while preserving anonymity and safety.

Although I was researching the experiences of bystanders to the Rwandan genocide, including children, the timing and the way in which information was dealt with in society in general impacted on my fieldwork and my social interactions. At the beginning of my latest period of fieldwork, I was not fully aware of the pervasiveness of the impact of *gacaca* on the way in which information was manoeuvred or of the contentious nature of most kinds of information. During the first few weeks of fieldwork, while not openly asking about ethnicity, I would make occasional references to it. I made explicit comments on the positive changes I saw as well as the challenges I knew about
from reading the literature and from discussions with Rwandans in the diaspora. I would enquire into the general fate of the families of those who were undergoing *gacaca*, or initiate conversations on the current situation in Congo.

I was met with uncomfortable smiles and many silences, while old friends and colleagues quickly guided me to understand what was permissible and not permissible as a subject of conversation at present, as opposed to ten years previously. And they reminded me and enlightened me on the carefully managed social manoeuvres they engaged in during social interactions to circumvent uneasy issues and uncomfortable questions.

While I was aware that the country had changed, it was only after I had experienced the changes sensorially, emotionally and relationally that I appreciated the impact they have on the everyday life of ordinary Rwandans and of foreigners who reside in the country who may or may not be able to articulate them. I learnt to respect the sensitivity and potential dangers of many kinds of information (which were not so prominent during my previous stay), and to internalise different manners of self-censorship. I initiated conversation with those I did not know by repeating the official version of current events or by only talking about the positive changes I had observed since my last stay. I would generally circumvent sensitive topics on most current affairs, and I would restrict the spheres of conversations to a few safe themes like family, health or religion. It was the rapid change in my behaviour that perplexed and worried me, as over a period of a few short months I began to experience those signs I had read about of life under repression: reduced trust in those I did not know well, secrecy, uncertainty, and fear. Like many Rwandans, I ended up making small talk while sharing in the knowledge of the existence of “public secrets” (Taussig 1992), what people know about but do not dare to speak of publicly.

Green (1995) writes about living with fear while conducting fieldwork in context of socio-political violence. I became aware of an increased psychological rather than physical fear for my safety, which translated into amplified suspicion, reticence to express personal opinions in public spaces, and discretion in giving information about what I was doing. The experience translated into increased worry at the realisation that I was engaging in ‘voluntary self-censorship’ of my opinions, movements and behaviours, and that this was having an impact on my identity. I was surprised to observe how rapid and powerful the effects of such self-censorship could be, and I shared what participants described as ‘heavy silences’ and an ‘imprisoned heart’.
Fear and self-censorship did not automatically end when I left the country. During a trip back to the UK in the middle of fieldwork, as I made explicit reference to ethnicity while explaining the Rwandan genocide to a friend, I instinctively lowered my voice and looked around me only to realise that I was sitting on a bus in London, at which point I felt relieved that I was free to speak of the unspoken and unspeakable.

Uneasiness about the implications of gathering and disseminating sensitive information continues to be a methodological and ethical issue in my work. For instance, I worry that the publication of papers like this one may have negative repercussions on my being granted a visa for Rwanda in the future; independently of whether this is the case or not, the very existence of this preoccupation testifies to the power of internalised fear and censorship.

Finally and conversely, while in Rwanda I experienced how once the invisible barrier of self-censorship and caution was overcome, sudden intimacy would emerge. Then participants who began to tell me their experiences would not stop talking; my interviews lasted for hours, and continued during repeat visits, as if participants had found a neutral and safe space in which they could open up. Mirroring this opening of spaces, I also found that the more aware I became of the impossibility to speak freely, the more I needed to decompress, and once I entered intimate spaces of trust I too would not want to leave them. I experienced something very different to self-censorship and the reverse of fear and suspicion, something which is not often discussed in the methodological literature on research on conflict which focuses on the challenges and problems of fieldwork: the creation of precious levels of intimacy and trust.

**Conclusion**

In 2009 at a conference held in Kigali before the opening ceremony to commemorate the 15th anniversary of the genocide, scholars from around the world met to discuss the genocide and its legacy. During the consultation, academics publicly accused other scholars outside the country of harbouring a genocide ideology and conducting research on behalf of the perpetrators of the genocide. These academics did not seem to realise that in the process of accusing others of harbouring a genocide ideology they naively spoke on behalf of the government, critical of research that criticises its vision and achievements, and became supporters of a specific post-genocide national narrative.
As researchers interested in understanding violence and its effects, we are alerted that research is part of social life, and that when social life is politically charged, so is research, including research with children, who are not ‘neutral’ beings occupying neutral humanitarian spaces but objects, subjects, social actors and participants in research and in nation-building projects.

In this paper, I have shown that research on violence and childhood that is framed within a dominant genocide narrative, whose memory and legacy shape the social position of children in society, can be problematic because it makes those categories of children who best fit the post-genocide national project visible at the expense of other categories of children who are rendered invisible.

I have described how some research projects I was involved in uncritically fitted and contributed to the national narrative of childhood and nation-building; I examined some of the challenges of researching those stories which do not fit within the dominant narrative; I recounted how the evolving socio-political context in which I conducted fieldwork influenced my experience as researcher and individual.

As research helps to render visible or invisible specific narratives in general, there is a need for research on violence which takes place in complex and rapidly evolving socio-political environments to constantly reassess and place under scrutiny its methodological and ethical assumptions, beyond the technicalities of methods and the challenges of procedures.

References


FOUR LAYERS OF SILENCE: COUNTERINSURGENCY IN NORTHEASTERN ETHIOPIA

Laura Hammond

In May 1996, on the first day of a three-week trek on foot to the lowest place on Earth, Ethiopia’s Danakil Depression, I was not expecting to run into a counterinsurgency operation. I knew that the Afar, the pastoral group living in this part of northeastern Ethiopia, had been socially, politically and economically marginalized by the Ethiopian state and their highland neighbours for generations (Bryden 1996a, b and c). Highlander agriculturalist and lowland pastoralist mutual distrust was a byproduct of the gradual expansion of the Abyssinian Empire during the 19th and early 20th centuries. Amhara and Tigrayan highlanders considered the Afar to be barbaric, uncivilized, and dangerous; Afar suspected highlanders of wanting to plunder their territory of its natural resources and to destroy Afar political autonomy in order to open up access routes to the Red Sea. Mistrust had yielded wildly inaccurate constructions of the Other on each side. Despite this, actual conflict between the two sides had been relatively muted in recent times. Neither I nor the Afar residents in the town of Ab ‘Ala (known by highlanders as Shekheit) I met prior to setting out on the journey anticipated that the Ethiopian state would choose this moment to search for members of the rebel movement known as the Afar Revolutionary Democratic United Front (ARDUF), known locally as Ugugumo. I did not understand the complexities of Afar-highlander relations, or the ways in which the tensions would become manifest through the expressions of silence that, in all of their various forms and meanings, were profound, powerful and revealing.

What we found, throughout our journey through Afarland, followed by a week in Ethiopian detention centres, and the aftermath of it all, was a network of silence constructed and maintained by various actors that featured multiple, overlapping layers. In this paper I extract four

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1 I would like to thank Matt Bryden, the friend and colleague who accompanied me on this journey. Thanks also to Carolyn Nordstrom, who encouraged me to examine these silences.
of these layers to show how power and vested interests conspired to ensure that this conflict remained invisible to all but the Afar. Through studying not only what people thought, said, and did about the conflict, but also what they did not say or do (but certainly thought), I suggest that silence is a fertile space in which the ethics and praxis of conducting fieldwork in situations of conflict are starkly revealed.

Walking into the Conflict

The trip into Afarland began as a reconnaissance mission to explore possibilities of initiating development programmes in the region. I had been working sporadically for the UN’s Emergencies Unit for Ethiopia as a field monitor, but was in between assignments. I hoped to be able to use the trip to write a proposal to UNICEF to support further research into health and education in the region. I was traveling with a Canadian colleague, who also worked as a field monitor for the United Nations and who, crucially, was under contract at the time. We were accompanied by an Afar representative of the Afar Relief Association (ARA), and two Afar guides. My lack of institutional affiliation would prove to be a serious problem.

Before embarking on our journey, we spent several days in Ab’Ala, the capital of the western zone of Afar Region. We explained our plans to local political leaders, police, and residents, and obtained official permission to go to the interior of the zone. We told people that we wanted to assess the need and local demand for health and education programs in this area. However, I had just spent two years conducting my PhD research in neighbouring Tigray Region, which was arguably the most favoured region in the country, and my secondary objective was to learn more about the Afar, whom I had heard were one of the most marginalized and neglected ethnic groups in Ethiopia. Having seen life at the ‘top,’ I wanted to see how those at the bottom of the political hierarchy were faring.

Afar territory trisects Ethiopia, Eritrea and Djibouti. Following the Red Sea coastline, the area is strategically important for all three countries. In addition, the Danakil Depression is an important source of salt, and rumours of the presence of other minerals, from petroleum

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ARA is now known as the Afar Pastoral Development Association (APDA). Thanks go to Ismael Ali Guray and Valerie Browning, who were and still are the directors of the organization, for helping to facilitate the trip.
to gemstones, abound. In the fertile southern floodplains around the Awash River, cotton is grown. For generations, the Afar have been hostile to the notion of state presence of any sort in their territory; they resent the domination by highlanders, which has for the most part been aimed at assimilating and sedentarizing them. Afar often express their conviction that one or more of the state governments in which their territory lies is interested in ‘pacifying’ them in order to rob them of their valuable natural resources (Flood 1975; Bryden 1996a and b).

Afar territory is one of the most researched areas of the world in archaeological terms – both ‘Lucy’ (*Australopithicus afarensis*) and the recently publicized ‘Ardi’ (*Ardipithicus ramidus*) were found in the region, the former paying tribute to the place through its name. However, little cultural anthropological research has been done into contemporary Afar society. The Afar have gained an unfortunate reputation through depictions in *National Geographic* and documentary films as being particularly ferocious. They are said to enjoy castrating their enemies and wearing the testicles around their necks. I never saw such jewelry, and the only evidence of castration I found were two elderly Afar eunuchs who had, they said, been attacked as children by invading armies from the Tigrayan highlands.

Three hours into the first day of our walk we found several dozen people, mostly women and children, sheltering in a series of caves. They said that they had fled from their homes in Eritrea to escape forcible conscription. We walked all day the next day and came to the home village of one of our guides. The village was deserted: only our guide’s wife, who had been expecting us, remained. All of the other residents had fled into the hills, having rolled up the walls of their straw houses and hidden them in the branches of trees. Livestock wandered freely, without even children to herd them.

Our guide’s wife told us that eight hours earlier, government helicopters had flown over the village and bombarded it with heavy artillery. As we walked around the village, we found pieces of shrapnel and craters in the earth made when the rockets had fallen to the ground and exploded. No one had been killed, we were told, but the Ethiopian army soldiers had interrogated local residents as to the whereabouts of *Ugugumo*. The Afar had denied any knowledge of the local rebels, insisting

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3 An important exception is Afar anthropologist Getachew Kassa Selassie’s work. See Getachew 2001.
that they had not been active in that area for several years. Here was my first glimpse of the first layer of silence, to which I will return.

The next day, while resting in the shade of a tree along a dry river bank, we saw three army helicopters flying overhead, towards the direction from which we had come. As we continued down the river valley over the next few days, we found additional settlements that had been strafed with artillery. In freshly abandoned military camps, we saw ration tins, biscuit wrappers, and remains of sheep that had been, according to locals, stolen from them and eaten. In every settlement, the story was the same: government helicopters had come, bombarded the area with heavy artillery, interrogated the residents who had responded with silence or denial of any knowledge of the rebels’ whereabouts, and then left. Only one death had been reported, a woman who had been hit by stray artillery. It seemed that the army’s primary intention had been to try to frighten people so that they would reveal the whereabouts of the rebels.

We walked more than 300 km in three weeks, dropping from an altitude of more than 1300 meters to 100 meters below sea level, and then returning to the highlands. There were no roads in these places, and local residents were more than a little surprised to see us walking into their territory. At the lowest point, we arrived at a police barracks in a place called Dorom. The police were surprised to see us, but they did not detain us. We tried to interview people about their priorities in health care and education, but found that people were more interested in talking about the helicopter ‘visits’. Everyone, including local politicians and police, claimed to be mystified by the attacks. They said that they did not understand why they were being attacked or what the government hoped to get out of such intimidation. This kind of treatment had been going on periodically for as long as they could remember. With the secession of Eritrea from Ethiopia in 1993, Afar pastoralists said that they felt that the two governments had become brothers and intended to “grind the Afar between them.” One man said “They are making coffee with us,” referring to the repeated pounding of beans between mortar and pestle. He said that most Afar were so tired of this treatment that they did not complain about it anymore. They silently went about their business.

Ugugumo, it seemed, represented the Afars’ sole protest against the government. Ugugumo was at once everywhere and nowhere. Significant enough of a threat to justify a widespread military campaign, yet seemingly never visibly present. Everyone talked about them, yet no one claimed to know anyone who was associated with them.
The Danakil Depression is marked by a chain of active and recently made inactive volcanoes. The largest active volcano, Erta’Ale, glows pink at night and the land around it forms a basin of volcanic rock, trapping rainwater to allow green pasture to flourish in a place one might expect to be parched and burnt by the sun. Cattle and goats graze along the base of the volcanic mountain, and because the land is so productive, it is home to a surprisingly large rural population.

As we left Dorom and walked northwards along the base of the volcanoes, we found what the government had been looking for. When we stopped to rest and sit out the mid-day heat, a group of five or ten Ugugumo militia caught up with us. We were told to stay inside a house, and for the next twelve hours our fate was discussed by the militia and local residents in another grass house in the same compound. Our guides spoke on our behalf, explaining that we had come to try to help the Afar people and insisting that we were different from foreign tourists and prospectors who had recently come to the region with the intention of removing minerals or opening the region up to outsiders. (They had been kidnapped for several days, and we were concerned that we might also be held as political pawns in the struggle with government forces.) Meanwhile, the women who lived in the settlement came to tell us, through graphic hand motions, that they fully expected us to have our throats slit.

Eventually, the militia became convinced of our good intentions, but decided that it was not safe for us to remain in the area, and declared that they would escort us out of the Danakil Depression. They walked us through a sandstorm and made sure that we had a safe place to sleep during the two nights that we traveled together. When we arrived in villages along the way, the Ugugumo militia – and by association we – were warmly welcomed and fed the best food that the households had to offer. The rebels were clearly well-known and well-liked by our hosts. They openly discussed with us their military objectives and their experiences as fighters. (Ironically, many of our conversations were held in Tigrinya, which I speak, but which they consider to be the language of the enemy highlanders). Eventually, a few kilometers outside our final destination, the town of Berhale, where we anticipated being met by a car with clean clothes and a case of beer, they left us to complete the journey on our own. It was unclear whether they had planned to leave us at this point, or whether news that the government had attacked one of their bases had made them change their plans, but they bade us farewell and went off to help their fellow fighters ward off the attack. We entered Berhale to the sound of
pounding bombardment coming from the direction of the *Ugugumo* base a few kilometers away, uncomfortably aware that our former captors, who had become our guides and protectors along the way, were likely to be injured or killed there.

No car was waiting for us in Berhale. The access roads to Berhale had been mined by the Ethiopian military and made a military garrison town with the presence of a full company of soldiers. As soon as we arrived, we were tipped off by local residents that we were likely to be arrested in the evening. The soldiers would wait to arrest us until they could do so under the cover of darkness, so as not to provoke the anger of the Afar who might be supporting us. With this warning, we had time to talk about which parts of our story we would tell and which parts we would keep quiet. We destroyed some film that documented the attacks, including the photos of artillery casings, craters from mortars exploding, and burned houses. We kept those which showed community meetings we had held that would support our story that we had been talking to communities about their development priorities. Thus began the second layer of silence. We did not want to expose our Afar hosts to unnecessary risk, nor did we want to be taken as spies.

A week earlier, we had promised our hosts that we would try to let people in Addis Ababa, the capital, know what they were being subjected to. “If you do not hear from us again,” we said, “it is not because we did not try.” “No,” one man lamented, “it will be because you have been arrested.” At some point, perhaps even that day, it was clear that someone had run ahead of us to warn the military authorities that we were coming and what we had seen.

In the evening, as predicted, the military came to the house where we were staying and arrested us. The commander said that although he understood that we had been given permission in Ab ‘Ala to be in the area, the order to arrest us had come from his military superiors. It was clear that military orders superseded those of the civilian administration. Our notes and film were confiscated and we were confined to the local police station. Following our arrest, we were flown by military helicopter from Berhale to Mekele, the regional capital of Tigray, and military base for the Northern regions. We were questioned by the Regional Force Commander, and held in military barracks. The next day, a Sunday, we were loaded onto another military helicopter and flown to Addis Ababa, where we were further interviewed by the Deputy Head of National Police. We were asked to tell our story of
what had happened to us individually, and to provide written accounts of what we had done and seen. We were then held at the Sostegna Prison in central Addis Ababa for five more days. No charges were filed, but we were not allowed to contact our embassies, the UN, or anyone else. Our ARA colleague was put into detention with the other prisoners. My Canadian colleague and I were held together during the day in an empty office with a single bed and a chair. Each night I was made to sleep on a sofa in the empty Officers’ mess hall; my colleague was taken to another empty office. While the conditions we stayed in were undoubtedly better than they would have been if we had been kept with the other prisoners, our unusual accommodation arrangements also meant that we were never officially registered as inmates at the prison, and we had no contact with other prisoners so we were not able to pass messages out of the prison as many others were able to do.

Finally, late on a Friday afternoon, just as we were beginning to lose hope of being released before the long weekend began, we were released. My colleague had been expecting to get married that weekend, and when he failed to arrive at his wedding in Djibouti, his fiancé called the Canadian Embassy to report him as missing. The Canadians’ interventions succeeded in securing our release.

Once we were released, we were followed around the capital for several more weeks by plainclothes policemen. I had borrowed a car from a friend for a few weeks, and when I returned it to her and left the country a few weeks later to take a break in Kenya, she was also followed. This feeling of being followed and listened in on wherever I went had the effect of creating a strong feeling of unease that altered my thoughts and behaviour with respect to what had happened to us. I will return to this subject later.

Silence as Cultural Construction

Anthropological examinations of the significance of silence have tended to focus on a conflation of expression and consciousness. Following Gramscian ideas about hegemony, silence is taken by many writers to be evidence of an acceptance of the established power structure, an unquestioning resignation to the status quo that involves those with less power acquiescing – through their silence – in their own subjugation. Thus in John and Jean Comaroff’s discussion of the workings of
hegemonic power, it is power that silences by preventing people not only from saying anything but also from thinking anything about their repression (1991, 23–24). Sheriff (2000) takes issue with this association of thought and utterance and suggests that an analysis of the cultural construction of silence may illuminate characteristics of unequal power relationships that would not otherwise be evident. Considering silence as a social construction may also reveal something about the perceived and agreed upon anticipated benefits or costs of expression. Sheriff proposes to examine at least some kinds of silences as forms of “cultural censorship”, since “unlike the activity of speech, which does not require more than a single actor, silence demands collaboration and the tacit communal understandings that such collaboration presupposes” (2000: 114). Looking at the basis upon which cultural censorship is constructed and the means it serves may help us to understand more fully the significance of observed silences.

While the notion of silence as resistance, or as a reaction to domination, has received a bit of attention in anthropological analyses (starting from Scott’s now-classic Weapons of the Weak [1991]), the use of silence by those with power cannot be understood through the same lens. When silence is exercised by those with power, the ultimate aim may be the protection of a strategy or a relationship. Even in these cases, however, I find Sheriff’s notion of silence as a cultural construction useful – in which there must be agreement that silence will have the desired effect and therefore should be maintained.

In what follows, I examine four layers of silence that featured in the experience described above. Each of these layers involved different actors, but each was also a consciously chosen path with specific aims. Each layer involved a set of actors at a different level of the power hierarchy. Unpeeling the layers of this particular onion can, I propose, reveal a great deal about the construction of power in this corner of Ethiopia, and more broadly has implications for the ways in which analysis of conflict and violence should be thought of.

First Layer of Silence: The Afar People

The Afar’s denial of any knowledge of the presence or whereabouts of Ugugumo came from a longstanding desire to carve out a separate political, geographic, and cultural space for themselves, away from the domination and oppression of the Ethiopian state. Afar revolutionary politics and collective identity is premised on the notion that Afarland
should be self-governed. In the best of all worlds, the Afar dreamt of an Afar-ruled state, incorporating their territories in Ethiopia, Eritrea and Djibouti. However, they were pragmatic enough to realize that this was unlikely to ever happen, particularly given that their territory includes the port of Assab and much of the territory leading to the port of Massawa, both of which are in Eritrea, and are thus particularly valuable to Eritrea and ultimately to Ethiopia. Thus, the “next best option” was to keep all forms of external authority out of their territory. Vehicles with government license plates were regularly targeted, and highlanders and foreigners were not welcome to work in the area. Many people told us that they did not go to the local clinics because they were staffed by highlanders who they felt did not treat them with respect.

Silence is most often understood either as an expression of passivity and compliance or of resistance. Yet, as Morrow (1996) points out, in between these two extremes rests another possibility: that resistance can be merely an attempt to keep domination and fear at bay. Afar who actively supported Ugugumo may have been resisting, but the majority of the people were silent in this intermediate sense. Neither compliant nor bent on subversion, their “revolutionary” aim was just to be left alone. In this sense, silence was a survival strategy in the face of a form of control which used fear as its basic operating principle (Green 1995).

In this case, the Afar were silent to non-Afar about the nature of their resistance, about the extent of their goals of self-determination, and about what they thought about non-Afars, in particular highlanders. To each other, they spoke freely about these things, and to some extent their rebellion was based upon these positions, but the discussion was kept internal and thus, to the outsider, hidden. Implicit in this maintenance of silence was the communally agreed-upon position that silence would in fact help to carve out a space to be left alone. To be effective, the strategy of silence had to be chosen by a large number of people. In such a way silence within Afarland can be seen as an act of cultural self-censorship.

Second Layer of Silence: the Ethiopian Government

It was clear when we started our trek that neither Afar residents nor their political leaders at the zonal and village levels were aware that the central government planned to unleash aerial terror on people in western Afarland that month. This was consistent with what seemed
to be a long strategic history of surprise attacks that kept people guessing, always wondering when the next attack would be. Fear and surprise were essential elements of the Ethiopian government’s strategy of flushing out the rebels. They hoped to take the rebels by surprise, though their efforts were thwarted by the Afar’s own strategy of meeting the attacks with silence.

The Ethiopian government also wanted to preserve the conspiracy of silence that surrounded its campaigns in Afarland. The government’s attacks on Afar territory were not reported in either the domestic or international press. Even people living near Afar Region did not know what was happening, though the Afar wireless news service, otherwise known as word of mouth, was extremely effective at spreading the message to Afar living in other areas. As an illustration of this, an Afar/Somali friend living in Djibouti heard the entire story of what had happened to us – our trek, arrest, and detention – in remarkably accurate detail from Afar who had travelled to Djibouti from the area we had been traveling in. News of our ordeal reached him while we were still being held in prison, and he later recounted what he had heard to me.

We were seen as threats to the Ethiopian government’s wall of silence surrounding the Afar – we had the power to potentially embarrass the government, and to raise awareness about what was being done to the Afar. The image of a government that beats up on its people was one that the Ethiopian People's Revolutionary Democratic Front (EPRDF), the ruling party, wanted to avoid; thus, police officers and military commanders went to great lengths to explain to us their views on why they were carrying out this counterinsurgency campaign. They said that they were sure that ARDUF/Ugugumo did not have the interests of most Afar people in mind, and that the government was in fact the champion of ordinary Afar who were being held hostage by this terrorist movement. They assured us that if we had understood the situation as well as they did, we would certainly see that they were right.

The silence maintained by the Ethiopian government was aimed at protecting and justifying the strategy of counter-insurgency that they were pursuing in Afarland. More than a decade later, it parallels Ethiopian government strategy concerning silence and counterinsurgency in another part of the country. The Ogaden, inhabited by Ethiopian Somali pastoralists, and where the Ogaden National Liberation Front (ONLF) is active, is another area in which insurgents have posed a threat to the state and, according to the EPRDF, have worked against
the interests of local residents for many years. In that case, the govern-
ment has tried to silence reports from journalists, academics, aid agen-
cies, and human rights organizations concerning the abuses it is car-
rying out against its own people. After it reported on abuses involved
in the counterinsurgency (see Human Rights Watch 2007 and 2008),
Human Rights Watch was banned from working in Ethiopia and its
work dismissed as a ‘pack of lies’ (see BBC 2007). International journal-
ists have also been arrested and deported, perhaps most notably New
York Times reporter Jeffrey Gettleman’s expulsion in 2007 (see Mesfin:
2007). Silence, coupled with denials of the few bits of leaked informa-
tion that do manage to make it into the public domain, are clearly a
favoured strategy of the Ethiopian government. As such, the strategy
is agreed upon and practiced collectively by government officials and
administrators. And as the next layer will show, this edifice of silence
is further supported by the complicitous silence of Ethiopia’s friends.

Third Layer of Silence: US Government

As I mentioned above, I was not allowed to contact the US Embassy
when I was being held in detention. However, when the photographic
film that had been taken from me (and which the police then devel-
oped into prints) and notes were returned to me, I found the calling
card of the First Secretary from the American Embassy tucked inside
the packet of photos. Had he slipped the card into the packet to let
me know that he had seen them? Or had someone from the Ethiopian
police gone to see him and inadvertently left his card in the packet
after the meeting? I was unable to find out, as the Secretary had left
his post by the time I could get to the Embassy to discuss the mat-
ter. What was clear was that there had been a conversation between
the Ethiopian and American governments about our arrest and about
what we had seen. Yet the Embassy had not intervened to secure our
release.

A week after our release, I met with the US Ambassador and told
him the whole story, including parts of the account that I had withheld
from the Ethiopian authorities. He listened politely, then proclaimed
to have had no knowledge of the event. He assured me that if he had
known that I was being held, he would have intervened right away
to secure my release. Even after I had reported what had happened,
however, the US government did nothing to raise the issue publicly of
the Ethiopian government’s military campaigns in Afarland. Since the EPRDF came to power in 1991, ousting the Marxist dictator Mengistu Haile Mariam and his government known as the Derg, the US had been a staunch ally of Ethiopia. Bringing attention to the plight of the Afar, and to the fact that the essential antagonism between highlanders and Afar pastoralists had not changed even as a result of its policy of ethnic federalism and devolution of power to the regions brought by the new government, might have called into question whether the new government was really as different from its predecessor as it claimed to be.

The US Embassy’s reaction – to remain silent and do nothing even when one of their citizens had been arrested – reveals quite a bit about US foreign policy in Ethiopia. It was clear that US interests in Ethiopia would not be jeopardized by the arrest of a single American who had been released unharmed after a week. It was also clear that the strong relationship between the US and Ethiopia would not be risked by raising the issue of counterinsurgency in Afar Region.

Ethiopia is seen as the most stable country in the Horn of Africa, one which the US can do business with, unlike its neighbours Eritrea, Sudan and Somalia. Donors in general, not just the United States, have generally not spoken out about human rights abuses committed in Ethiopia. Human rights workers and some aid agency staff will recognize this as a source of frustration when it seems that the donor community turns a blind eye, or keeps its mouth shut, in the face of overwhelming evidence of Ethiopia’s heavy-handed tactics against its own people to stifle dissent. Again, this strategy of silence involves complicity by individuals within particular donor government embassies, as well as between these embassies, and many would argue that it has allowed the EPRDF to effectively prevent political pluralism and freedom of speech.

Fourth Layer of Silence: The Anthropologist

The final layer of silence involved my own decision not to write about what I had seen and what had happened to me, and is the most difficult for me to unpack. My Canadian colleague wrote a sanitized account of our travels, focusing primarily on developmental priorities of the Afar and mentioning in general terms the Ethiopian government’s attempts to intimidate the rebels but omitting the account of our detention at the hands of both Ugugumo and the Ethiopian authorities (see Bryden 2006 all). We also told our complete story to representatives of the Inter-
national Committee of the Red Cross (ICRC), but we did not speak to journalists or otherwise publicize what we had witnessed. We refused to speak to the few journalists who heard something about our story.

Although I was never explicitly threatened, I was afraid that if I spoke or wrote publicly about the full story of what had happened to us, I would be forced to leave the country, or rearrested. This feeling was exacerbated by the knowledge that I had not been entirely forthcoming about certain aspects of our story, including the 48 hours that we had spent in the company of the Ugugumo militia. Moreover, during the interviews with Ethiopian military and police officials, it was made clear that there was a political rationale to Ethiopian strategy in Afar that was not, to them, open to contestation. Out of fear for myself and retribution that might be wrought on the Afar I had spent time with, I censored myself and did not write anything about this story until ten years later, while writing this paper. Even after having written a draft, I waited several more years before deciding to publish it.

After the arrest and despite my silence, my relations with Ethiopian government officials and other politicos were strained for several years. Senior management of the Relief Society of Tigray, a nongovernmental organization with whom I had had close associations before the arrest, became strained, and I was unwelcome in their offices. Rumours were spread throughout the Ethiopian government in Addis Ababa and the Tigray administration in the regional capital, Mekele, that I had been a spy, that I was against the government, and that I could not be trusted. Although no one ever said that I had been blacklisted by my arrest, it was clear that I was no longer seen as a sympathetic ferenjee (foreigner) by the government.

I felt uncomfortable working in Ethiopia for all of the reasons noted above, and left in 1997 to work for the United Nations Development Programme in Somalia for a year. When a border war between Ethiopia and Eritrea broke out in May 1998, I decided to return to Ethiopia. In this war, Afar Region was one of the main battlefields of the border war; ironically Afar were recast as ‘patriots’ by virtue of their proximity to the border, a border that they largely ignored. I took a job as a field officer for the UN Emergencies Unit for Ethiopia, but I was not able to travel to the northern war-affected regions for two more years; foreigners were required to obtain security clearance to travel to the militarized regions, and the government ignored my requests for clearance. This, I had learned, was a common practice in Ethiopia – not to say no, but to convey the same meaning by ignoring a
request for assistance or permission until it became clear that the lack of action constituted a refusal. It was only in late 1999, three years after the detention in Afar, that I was invited by the national Disaster Prevention and Preparedness Commission to travel back to Tigray to participate in a crop assessment along the front lines; I suspect that this was because someone – a senior government official – had decided that I could be trusted again since I had not spoken out against the Ethiopian government in the interim.

What happened to me in this story is important only insofar as I became in some ways complicit in this multi-layered construction of silence. Much has been made in the literature on anthropological responsibility of the role of anthropologist as witness or even advocate for those whose voices are silenced or who are otherwise oppressed (see Scheper-Hughes 1995, Nader 2000). In such discussions, the responsibility to ‘do no harm’ is critiqued, with some choosing to settle pragmatically for ‘do as little harm as possible’ or to recognize that the decision to engage in the research enterprise brings with it risks that the anthropologist may have little or no control over. However, there is an uncomfortable confession missing from these critiques, which is that researchers may choose to be silent in order to protect themselves or the future of their work in a country or region. I suspect that self-censorship of this kind happens much more often than most social scientists admit, and it is somewhat surprising that it is not acknowledged given that humanitarian actors and their analysts have grappled with the issue for years (see, for instance, Moore’s edited volume 1998). In those debates, nongovernmental organizations have defended their (mostly) occasional silences by saying that silence has been the necessary price of ensuring continued access to the people they are trying to help. Few social scientists would admit to a similar situation in their work.

Ultimately, my discomfort at my decision to censor myself led me to develop research projects outside Ethiopia, and led to my decision in 1999 to stop living in the Horn of Africa after seven years. I have written critically about other aspects of Ethiopian governmentality since then (see Hammond 2008 and 2011), and feel more able to do that since I now do not live in the country and can retreat to my home in London if my relationship with the government again becomes eroded. It may even be the case that, having silenced myself on that one occasion, I am now less likely to do so again in other contexts. What is true regardless is that the behaviour of the social scientist in such set-
tings is often more complicated than is generally acknowledged, and that the application of ‘do no harm’ thinking to the questions about what to say and what to withhold apply not only to the likelihood that the research subjects may come to harm, but the researcher may also be exposing themselves to greater risk. More intellectual honesty is needed to unpack this element of the research enterprise.

Conclusion

In this account, silence has many faces. It is the denial of knowledge and the insistence on the inactivity of revolutionary forces. It is the active support for a force that, while almost certainly incapable of realizing its ultimate objectives of establishing a free state, has at least successfully banished most forms of governmental authority (for both better and worse). It is the Ethiopian government’s attempts to hide its counterinsurgency activities from the international community, as well as from its own populace. It is the US government’s unwillingness to admit that the post-socialist regime in Ethiopia may not be as deserving of its support as it would like to think. And finally, it was my own reticence to speak about what I had witnessed, out of fear for my own safety and to ensure my ability to carry out research in the future.

In each of these layers of silence, there is a sense in which the communal construction of silence is at work. Silence serves a purpose that the parties to the act of censorship all agree to, and this is what gives it its effectiveness. In some cases, the recognition of the value of silence is also held by those towards whom the act of silence is expressed: in my case, the Ethiopian authorities clearly understood that my silence was valuable to them, and through their own version of silent intimidation, they succeeded in keeping me quiet. In none of the layers I have examined here has silence been associated with a lack of consciousness about the inequities at hand or of the implications of speaking out. This further supports the idea that awareness and expression are not necessarily two sides of the same coin.

Silence in conflict situations brings ethical questions to the fore. Before we are in such a situation, we can ask in hypothetical terms who may be put at risk, or even what is at stake, in writing about what we have seen, but when we find ourselves actually embroiled in it, we may be surprised by our actions.
Postscript

While visiting Ethiopia in the summer of 2007 (and having already committed myself to writing this paper), I ran into the man who had been principally responsible for my arrest and detention, the former Deputy Police Commissioner, who is now retired. In an almost surreal recapping of the events of 1996, he told me that he considered himself to have been my protector throughout the ordeal. He said that the rest of the police force had been convinced that we were spies. He seemed surprised when I told him that my reaction had been to become extremely frightened of the Ethiopian police for many years. “Why should you be afraid of us?” he asked, clearly amused. Yet he went on to recount with a laugh how, after he had us released, he watched my colleague and I leave his office and give each other a hug of relief in the car park outside. “We had you so scared!” he declared, clearly delighted at the thought. Still, despite the passage of time and his ability to now laugh at what had happened to me, he maintained his serious conviction that the government had been right to bomb the Afar villages, to attempt to break up the rebel movement. ARDUF continues to be active, and the Afar continue to be silent in the face of government repression.

Bibliography

Sites of violence are especially revealing contexts for exploring the soft underbelly of ethnographic practice. Prone to wildly volatile upheavals, war zone research can prove harrowing, physically and emotionally. Basic obligations of the researcher to secure informed consent, ensure beneficence, maintain impartiality, guarantee confidentiality, eschew privilege and create valid and balanced representations require constant vigilance in any context. War zone research, however, intensifies such challenges, often exposing underlying ethical enigmas in the process.

Perhaps this is especially true of field research involving multiple returns over extended periods to regions dominated by warfare and social upheaval. Or, at least, that was my experience while working among war-torn Nuer communities throughout Sudan’s second civil war (1983–2005). On the upside, multiple-return ethnographic studies can yield more dynamic, ambitious, and causally-nuanced analyses of war-provoked processes of socio-cultural change – analyses capable of filling in historical gaps with experientially resonant understandings of the shifting forces at play. Periodic reentries into the war zone provide multiple vantage points from which to assess the continual spinning and re-spinning of critical war-time events by key actors and opposition groups. Long-term research can also help identify serious survival challenges that the population in question will face in the near future. When wartime research builds upon extensive field experiences gained before the conflict erupted, the ethnographer is well placed to grasp the threads of social and cultural transformation and follow them throughout the labyrinth of war. Such knowledge can potentially contribute toward reducing unfettered military violence, providing emergency assistance or stabilizing tentative steps toward the restoration of peace.

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On the downside, anthropologists returning again and again to war-battered communities may be dogged by problematic feelings of personal responsibility with respect to the immediate human suffering they are witnessing and documenting. Having developed more intricate and extensive personal ties with the war-torn populations concerned, the anthropologist may feel under increasing pressure, internally and externally, to take more concrete steps to counter whatever threats to the community’s long-term survival appear most urgent and obvious.

Disciplinary-wide debates revolving around whether or not anthropologists should directly engage in pragmatic efforts to protect and enhance the long-term well-being, self-sufficiency, land rights, and so on, of their host populations are often far more immediate and poignant for anthropologists working repeatedly in dangerous fields. This experiential sharpening of the ethical dilemmas entailed, however, in no way guarantees that the anthropologist in question – however knowledgeable and well-meaning – will discover more effective solutions to ethical conundrums that have haunted the discipline from its inception. Although acts of “witnessing” may provide a conscience-salvaging balm for “one-time” field researchers exploring dangerous locales, recurrent returns over many years are likely to provoke rising expectations within war-battered communities that the anthropologist will “give back” in more concrete and lasting ways. In many troubled regions of the world today, the institutional lines between academic researchers, human rights activists, development agents, and humanitarian relief workers have begun to blur.

Although the bottom line of all ethical research practice remains a negative imperative to “do no harm,” this negative imperative hardly justifies embarking on ethnographic research among people caught in the immediate undertow of war. What about our ethical obligation to ensure beneficence? What imaginable “cost-benefit” analysis is ethically appropriate for researchers contemplating engaging in war zone research? And for those of us who have already engaged in such research, when is it ethical to disengage from the violence and upheaval experienced by our subjects of study? Otherwise expressed: What do ethnographers working in such turbulent places really “owe” the subjects of their studies? Is it “the gift” – à la Marcel Mauss – of intellectual honesty? The gift of political neutrality? Foresight? Advocacy? Protection? Resources? Powerlessness? These are the questions that, I suspect, haunt many of us working long-term in “unstable places.”
When I began research among the Sudanese Nuer in 1980, I did not set out to study civil warfare and violence. My initial 21 months of study among Nuer agro-pastoralists living in swamp-laden savannas of the greater Upper Nile region of southern Sudan was completed during a brief period of peace separating two devastating civil wars. Long before Darfur exploded in 2003, Sudan endured two gruelling north/south wars, spanning the periods 1955–1972 and 1983–2005. All in all, some three million southern Sudanese lost their lives during these two wars, and six million others were violently displaced. My initial field research in Nuer regions of southern Sudan was completed during this inter-civil war period of relative calm. After war returned in 1983, however, I continued to conduct periodic field research in Nuer areas, where the war’s violence was increasingly concentrated. I also expanded the geographic scope of my research program northwards to include recurrent field investigations of swelling populations of war-displaced Nuer and southern Sudanese living in squalid shantytowns and official displaced camps on the outskirts of Khartoum. I returned a total eight times to Sudan during that 21-year-long conflict for research periods ranging from three weeks to six months for a total of 19 months of war-time research. I gradually supplemented this research with more limited investigations of growing Nuer refugee populations in Ethiopia, Kenya, Canada, Australia, the United Kingdom and the United States.

As the years passed and government military attacks against oil-cursed Nuer villages intensified, I gradually moved beyond participant-observation to devote more energy to trying to ‘assist’ the subjects of my research. For instance, I served for a period on an international human rights monitoring team responsible for investigating all major military attacks against southern Sudanese civilians during lengthy and tempestuous peace negotiations. I also provided expert witness testimony in a U.S. Federal Court case raised by southern Sudanese civilians against an international energy company. And following the 2005 signing of a north/south peace agreement, I founded an educational non-profit organization that supports the development of primary schools in Nuer and Dinka regions of the south. While I distinguish such activism from my scholarly research efforts, both endeavours involved me in a series of unanticipated moral predicaments that often forced me to re-examine – in painfully uncertain ways – the ethical and methodological underpinnings of my research program.
With these broader ethical and methodological questions in mind, I wish to focus in this essay on a particularly troubling dimension of my long research association with the Sudanese Nuer – an association that now extends over more than a quarter of a century. Specifically, I want to explore the nature and limits of my ethical obligations as a foreign researcher toward the Sudanese state government in Khartoum. Standard disciplinary codes of conduct and internal review boards stress the fundamental obligation of foreign researchers to obey all national laws. State sovereignty rights are the bed-rock on which all international research rests. But what about situations in which the state is violently displacing and destroying the people under study?

Throughout much of Sudan’s second, north/south civil war, the government in Khartoum did not control the rural Nuer villages in which I worked and was, in fact, actively organizing military strikes against them. The state’s sovereignty claims over my principal research areas were largely an illusion projected toward the outside world by a small but savvy political/military/religious/commercial ruling elite based in Khartoum. Those rights existed primarily on paper, on maps, and in the minds of United Nations officials and international diplomats. Under such war-time circumstances, what are the ethical obligations of a foreign researcher to the Khartoum government or, for that matter, to its political and military rivals?

Even when I conducted research among war-displaced Nuer populations in the north, the central government’s all-too-palpable presence was hardly benign. From the late 1980s onwards, the make-shift homes of displaced southern Sudanese civilians ringing the outskirts of Khartoum were repeatedly flattened by government bulldozers with little or no advanced notice. Arbitrary arrests, forced conscription, public lashings and arbitrary imprisonment were routinely imposed on war-displaced southerners caught engaging in petty trade or transgressing the government’s harsh interpretation of Islamic law. War-displaced southerners were also subjected to police harassment and brutality throughout the war years and beyond.

My uncertainty about how to define my obligations to the Sudanese state flowed into a much deeper problem of knowing where the political and military allegiances of many Nuer, with whom I interacted, stood at any one point in time. More than any other southern region, Nuer areas of the greater Upper Nile experienced some of the greatest political and military instability during the war. Their vulnerability to internal division and manipulation stemmed directly from their strategic location on top of some of Sudan’s largest known oil deposits.
Their homelands also straddled the main military divide line separating forces allied with the Khartoum government and those allied with those of the Sudan People’s Liberation Movement/Army (SPLM/A) during the early years of the war. And later, when the SPLA fragmented, spawning a number of spin-off southern military rivals, Nuer regions of the greater Upper Nile provided one of the main battlegrounds for south-on-south military confrontations as well.

From the oil-rich lands of Western Upper Nile coveted by the Khartoum government, across the vast floodplains of the Sudd where south-on-south military confrontations intensified, to the eastern fringes of Nuer territories in southeastern Ethiopia, where southern rebels struggled to maintain base camps and through which hundreds of thousands of southern war refugees eventually fled, Nuer communities and lands lay at the epicentre of the violence that devastated southern Sudan throughout the war.

Disunited from the start, Nuer political and military leaders have also played pivotal roles in the perpetuation of the conflict. With the collapse of southern military unity during the 1991–2002 period, the Nuer fractured even further, as growing numbers of self-proclaimed military leaders flip-flopped across the government/rebel military divide. Throughout this period, there was a tremendous blurring of political and military loyalty lines. Consequently, it was not uncommon for me to discover that Nuer contacts who were allied with the Khartoum military during one research trip had shifted allegiance by the time I returned to the region. In brief, there were no stable lines of allegiance running around or across Nuer territories during the war.

On the negative side, this meant there was no neutral grounds from which I could survey the war. Nor could I assume the political and military complexity encountered in Nuer regions were generalizable across other southern regions. Nevertheless, there were advantages to working in such a fragile and fragmented war zone.

This tragic splintering of Nuer regional and military alliances had an upside, at least as far as my research was concerned. Nuer were operating on all sides of the conflict and were, in fact, continuously crisscrossing “enemy lines” in great numbers. For these reasons, my Nuer networks were capable of providing detailed accounts of critical events, as these were witnessed, experienced, and interpreted from various sides on the conflict. Otherwise put: I was forced to maneuver through highly ambiguous situations and personal encounters in which ‘the government’ and its nominal agents were rarely clear. In this world of unstable alliances, I felt compelled at times to adopt a
defensive shield of strategic transparency, as I jumped over the jagged fault lines of the war.

In what follows, I will describe briefly several tense encounters I had with suspicious representatives of the Khartoum government and how I sought to defuse their hostility. Although these situations and their resolutions are unique, they may offer some insight and helpful lessons for other foreign academic researchers traversing the shifting sands of war.

Lesson One: When Assets Become Liabilities, Bury Them!

Even before civil war severed Sudan, ‘the government’ (Hukuma) was not a unitary concept in Nuer eyes. During the early 1980s, Nuer drew a marked distinction between “the government of the left” and “the government of the right.” The government of the left was identified with regional administrative networks, chiefs, courts, police posts, district officers, and the like and was said to “want people to live.” “The government of the right” or “the army,” in contrast, “brought death.” During my peacetime research, “the government of the left” predominated, but after war broke out, “the government of the right” took over.

As a young PhD student arriving in 1980, I had a great many dealings with “the government of the left.” I meticulously acquired all necessary government research permits and travel permissions, slowly winding my way southwards from Khartoum through various regional and district offices on my way to the rural Upper Nile. By the time arrived, I had acquired a thick stack of government papers legitimizing my research aims, which I carried in my backpack. I finally ventured out alone on a three-month walking tour of Nuer settlements in the eastern Upper Nile.

My knowledge of the Nuer language was still extremely rudimentary. Consequently, the first few times I was asked upon arriving at a new Nuer village whether I was a “guest of God.” I initially misinterpreted this question to mean, “Are you a missionary?” and answered, “No.” I would then pull out the hefty wad of research permissions I had acquired and try to explain in, no doubt, incomprehensible Nuer fragments that I had come to learn about their language and culture. At other times, I simply presented my government papers on arrival at a new village, naively assuming that, even if no one could read them, it was important to show that I had them. I gradually realized that
these documents, far from opening the doors of Nuer hospitality, classified me in Nuer eyes as a “guest of the government” – not a good thing. Many Nuer reasoned that, since I was apparently a “guest of the government” and not a “guest of God”, the government was the party responsible for hosting me. Consequently, when there were no nominal government agents (such as police, chiefs or headmen) living in the first few Nuer villages into which I wandered, I sometimes went hungry. There were, of course, no restaurants, markets, or hotels on which I could fall back upon. Throughout this initial three-month walk-about, I was utterly dependent on the generosity of strangers.

This misunderstanding – and my hunger – provided my first insights into the extremely tenuous presence of both the “government of the left” and “the right” in the daily lives of rural Nuer men and women. I also began to realize how differently state powers were viewed in the north and south of the country. Experimenting, I soon discovered that identifying myself as a “guest of God” was preferable by far. A “guest of God,” I came to learn, means simply an unknown person who arrives unexpectedly and is in clear need of assistance. Any Nuer man or woman can proudly host such a guest and, in so doing, gain a measure of divine recognition for their kindness. That thick stack of government permissions was soon buried at the very bottom of my backpack and, from that time on, I made every effort to try to dissociate myself from the “government of the left” and the “government of the right” – a precautionary strategy I continued long into the war years.

Lesson Two: It’s Never Too Late To Negotiate

As the political situation in the south rapidly approached boiling point and security threats of all kinds increased, I, like many of the southern Sudanese women around me, faced a rising ethos of sexual entitlement on the part of southern and northern Sudanese soldiers. Were it not for my white skin and foreign origins, I suspect that I would not have managed all the fieldwork I did without being raped. In one situation that was truly touch-and-go, I was forced to negotiate a compromise by drawing on “traditional” Nuer/ Dinka cultural values and courtship practices, both of which maintained that it is the woman, not the man, who determines “how far things go.”

The most perilous situation I faced in this regard occurred while I was riding in a small, open-air skiff, together with more than a dozen
Nuer men and women, along the upper reaches of the White Nile. The skiff was owned by the “Egyptian Irrigation Department” and, for a fee, the Nuer driver took on passengers. I was chatting with and photographing some of the other passengers, when off in the distance, a larger, unmarked merchant boat floated by. This was in early 1983, just a few months before the furies of war descended full force on the region. As things turned out, this larger merchant vessel had been commandeered and manned by a Sudanese army patrol. Spotting my camera, an Arabic-speaking soldier broke out a bull horn and ordered our little skiff to dock alongside the merchant boat. I was ordered out of the skiff and aggressively interrogated and threatened by four northern “Arab” soldiers. Responding in Arabic, I answered all their questions and explained that I had not photographed their boat and could not possibly have known that it belonged to the army because it was unmarked. I apologized, removed the film from the camera and gave it to them. But they only became more threatening. They told me that they had the power to hold me indefinitely and that I would have to spend the night on their boat (with them). At that point, the commanding officer descended from the pilot’s lookout cabin. Unlike the others soldiers, the leading officer was not a northerner but a southerner and native Dinka-speaker. He had actually fought against the central government during Sudan’s first north/south civil war (1955–1972) and was subsequently integrated into the Sudan Army Forces as part of the 1972 north/south peace agreement. Of course, I only discovered this information later, after the officer ordered me to accompany him to the pilot’s cabin.

Meanwhile, the skiff with more than a dozen other passengers remained strapped to the side of the army boat. To make a long story short, I spent the afternoon by the officer’s side as he proceeded along the river, searching for southern “bandits.” By that time, I had learned a great deal about this officer’s personal history and had even asked him some tentative questions about how he might respond in the event that full-scale war re-erupted between the north and south. As we drifted along, several Nuer men appeared suddenly amid the tall papyrus lining the shore, driving four large oxen before them. These oxen were an unsolicited gift for the government army patrol, signifying both fear and peace. Several heavily-armed soldiers disembarked and promptly shot the oxen in the head before dragging them onto the boat, while the men who had brought them disappeared into the tall grasses.
As dusk fell, the Commander informed me that I would be spending the night on the boat, giving me the “choice” of spending it on the open lower deck (with four or five northern soldiers) or with him in his upper cabin. Realizing that my chances of escaping a night below without being gang-raped were slim, I “chose” to spend the night with the Dinka officer in his cabin.

Once in his cabin, I thought fast. I opened up a conversation by expressing sympathy for his predicament and acknowledging how lonely he must feel during extended patrols away from his family. I then explained that I was very worried about the possibility of becoming pregnant and, for that reason, we needed to come to a compromise. I would be willing to agree to hug and kiss him but, first, he must agree that he will not touch me below the waist. I then insisted that the officer shake my hand, thereby formally agreeing to these “terms of engagement.” Fortunately, I had donned a pair of jeans for the boat trip, rather than my standard dress, in the hope of protecting myself from the river’s insatiable mosquitoes. As we lay on his narrow berth, I tried to soften him with every touch. However, there came a moment, as I had feared, when he broke his agreement and forcibly tried to remove my jeans. The narrow berth on which we lay was quite high above the floor. When he persisted, despite my verbal protestations reminding him of our agreement, I finally managed to catch him off balance and, using the strength of my legs, heaved him off the bed. He crashed hard upon the floor. I held my breath, thinking: “Either he is going rise up furious, beat the hell out me and rape me, or he is going to remember his handshake.” The officer slowly picked himself off the floor, turned and looked at me for a moment, then left the cabin.

In the morning I was released, along with all the other skiff passengers still strapped alongside the boat. I don’t know whether or not I would have managed to escape this encounter unscathed were I a southern Sudanese woman. Nevertheless, in trying to reduce the probability of my being raped, as I did, I was drawing upon a fairly deep knowledge of the cultural norms governing the gender dynamics of Nuer “courtship” and was strategically betting that these norms were probably common within Dinka courtship traditions as well. Years later, this particular Dinka officer developed into an extremely violent and notoriously unpredictable southern warlord, who abruptly switched his military allegiance several times between the northern government and various southern rebel factions during the war but did not survive it. Years later, I recounted this event to two Dinka
women, both of whom knew this officer personally, and asked them what they thought was passing through his mind when he turned and left the cabin. These two women looked at each other and then agreed that he most likely thought: “It’s your loss.”

Although this is the closest I came to being raped during my field research, there were other close calls. However, the central lesson I took from this event – namely, “It’s never too late to negotiate” – served me well in defusing many other wartime encounters with suspicious and threatening government agents.

Lesson Three: Don’t be Boxed, Remain Ambiguous

When working in Khartoum, my general strategy was to project an image of complete transparency about my research and, whenever possible, lean on individual connections with northern and southern Sudanese forged before the war for additional support. I also began to use my knowledge of Sudanese Arabic more strategically. At times, I displayed my linguistic skills in an attempt to impress, mollify, or confuse overly zealous government security personnel. At other times, I disguised this knowledge in order to enhance my personal security. It is amazing what people will say in your presence when they think you cannot possibly understand.

My primary research language remained Nuer. Sometimes I would invite as many as five or six Nuer research collaborators to the lobby of my hotel, publicly chatting in Nuer with them for hours at a time. When questioned by suspicious security agents, I would parry in Arabic with arguments such as: “I don’t think it is right for foreign researchers to come to Sudan, gather information, leave, and then publish their results without returning first to make sure that their findings are correct. That’s why I have come to Sudan now. I want to double-check with as many Nuer as possible in order to make sure my understandings of their culture and history are correct before publishing them.” This forthright response was surprisingly effective in defusing the hostility of government security officials, but it was not always effective.

During a research trip to Khartoum in 1989–90, shortly after the coup d’état that first brought the current President of Sudan, Omar Hassan Ahmad al Bashir to power, I was arrested by a plain-clothed government security agent. My offence was fluency in the Nuer lan-
language, revealed by an animated conversation with a friend in a public café. After my friend was angrily cross-examined when I went to pay for our drinks, he suggested that the security agent redirect his questions to me, explaining that I also spoke Arabic. This suggestion only seemed to make the officer angrier, and after I had answered all his questions, he ordered me to accompany him to the central police station. I had just arrived in Khartoum that morning and was already wondering whether it would be possible to meet safely with any of my Nuer acquaintances. However, as I sat waiting nervously at the station crowded with government policemen, one of them lifted his wrap-around sunglasses, looked closer at me, and said in Nuer: “Why you’re Nyarial! Don’t you remember me? I knew you back in 1982 when you were living out in the countryside near Bentiu [Western Upper Nile].” “Remember Mohammed?” he continued, “He also knew you in Bentiu. He is now the head of the Khartoum police.”

By a remarkable coincidence, Omar Hassan Ahmad al Bashir was the head of the army unit stationed in the strategic, oil-rich region of the Western Upper Nile before seizing the reins of power in Khartoum in 1989. After the coup, he brought many of the policemen and army officers he had worked with in the regional capital, Bentiu, with him to Khartoum, promoting them to high positions within his new regime. This Nuer police officer who recognized me and his commanding officer were both among them. In fact, Mohammed turned out to be the police officer ultimately responsible for monitoring my presence and security during my second year of pre-war field research.

After a long and friendly chat in which Mohammad reminded me that he had once given me a lift in his Toyota to a distant Nuer town, I asked him if I could have an official letter from him allowing me to visit “my Nuer friends” in the spontaneous settlements and official displaced camps that were now scattered throughout and around greater Khartoum. A letter from him, I explained, would shield me from further harassment and spontaneous arrests from overzealous security officers. He readily obliged. I then sought the government’s permission to carry a camera and to take photographs through my contacts at the University of Khartoum, which was provided. So, when I was later arrested again, this time for taking photographs of a spontaneous Nuer settlement just prior to its forcible demolition, I had all of the necessary government paperwork to justify my presence and activities.

This arrest landed me in the offices of the far more opaque national security agency, more commonly known at that time as “Islamic security,”
where I faced a more skeptical interrogator. Noting nothing but a large, green, leather-bound copy of the Holy Quran on the officer’s desk, I set about trying to rupture whatever pre-conceived ideas he may have had about the political leanings of a white American woman discovered, camera in hand, amidst a community of war-displaced southern Sudanese civilians about to have their make-shift homes forcibly removed.

This arrest occurred only a few months after forces of the National Islamic Front (later renamed the National Congress Party), led by Omar Hassan Ahmad al Bashir, successfully toppled the elected government of Sadiq al Mahdi. Tensions were still running extremely high in Khartoum, as the coup leaders were in the process of systematically purging all potential dissidents from within government bureaucracies, court systems, national security operations, and military units and replacing them with loyal Islamists. Moreover, the war in the south was not going well for them, with major, new, SPLM/A military victories in the south being announced almost daily across the entire country from a powerful, rebel-controlled, radio transmitter located in southeastern Ethiopia. In order to soften whatever preconceptions my inquisitor may have had about my political leanings, I switched our conversation from English to Arabic and, after presenting him with my legitimating letters, tried to impress him with my knowledge of both Sudanese Arabic and Islam. I explained that I had studied both while living for a year in Egypt and, to support my claim, I even recited the opening chapter of the Holy Quran.

After making several phone calls and verifying the government letters I presented, he briefly left the room, whereupon I quickly removed the film in my camera, replaced it with a blank roll, and slipped the used film into my headscarf. I had taken the precaution to dress the part by wearing a demure dress with long sleeves, complete with a colourful headscarf. This precaution was part of my routine efforts to cultivate a more ambiguous identity, which would make it more difficult for northern government officials to immediately categorize me as politically partisan to the south. My interrogator returned seconds later and, after a few more phone calls, demanded that I hand over the film. I offered the blank roll from the camera. He then said I could go and even went so far as to offer to find me a ride to my hotel. As I rose to leave, I was very nervous that the film loosely stowed in my headscarf would suddenly fall to the floor. As I left his office, his last words to me were: “And stay away from the displaced camps!”
What were my ethical obligations to obey this command? I am not thinking here about the personal risks of possible disobedience but, rather, about the ethical underpinnings of my obligations as a foreign researcher to the security cabal that had recently come to power in Khartoum through a coup d'état. Certainly there was considerable pretense in my attempt to project an aura of political ambiguity and research transparency. Certainly, much of the information I was gathering, intentionally and inadvertently, could be considered contrary to the political interests of the newly installed military junta. But in what sense were the self-proclaimed leaders of National Islamic Front a legitimate government at all? What popular legitimacy did they really have at that time – remember this was only a few months after the coup. Granted they had seized control of the palace, the barracks, and TV and radio stations – and proclaimed themselves the national rulers of all Sudan. But they were also brutally suppressing not only the war-displaced southern communities I was studying in Khartoum, but also a majority of Sudanese citizens in the north, south, east, and west of the country.

Was I somehow obligated to blind myself to all the government brutality going on around me – such as the southern schools boys I watched being dragged off buses, beaten, abducted, and eventually shipped off to fight against their southern brothers? Just wandering around the central market square in Khartoum, I could watch northern and southern Sudanese women being publicly lashed for having committed the crime of engaging in the informal economy by selling petty trade items on the street or cups of tea from the doors of their homes in order to feed their children. Add to these eye-witnessed events, the detailed reports, provided by specific individuals, of their experiences of torture, jailing, beatings, and rape gathered orally during this and subsequent research trips to Khartoum. And what if I managed, say, to gain entry into a government prison and to document the abysmal conditions prevailing there? Or if I inadvertently learned about the location of a secret government detention house? What was I to do with such information? Was I morally obligated to disseminate it or not? If so, how and to whom?

I faced parallel difficulties working in rebel-controlled regions of the south. There, too, I often stumbled across allegations of military atrocities, war crimes, extra-judicial executions, rapes, and other serious human rights abuses reportedly committed not only by government troops but by SPLM/A forces and an ever-growing number of
spin-off southern military factions and warlords. SPLA officials as well as various regional warlords also considered themselves to be “the government” in the regions they controlled. They too sought to assert their “sovereign rights,” often by demonstrating their abilities “to kill with impunity.” And thus, I was gradually caught up not only in the turmoil on the ground, but also in a kind of sub-war of information, in which all parties to the conflict sought to suppress some “truths” and emphasize others. If all knowledge is a matter of perspective, where could I safely put my feet down?

Self-censorship is always an option and, sometimes, a security necessity. But at what point does an ethic of silence become an ethic of complicity? There was always the possibility of shunting some of the information too hot for me to handle toward various international human rights organizations, which could potentially integrate it anonymously into their reports. But at times such information weighed heavily upon me. Sometimes I came to know things that I wish I didn’t know. The bottom line is that there are ethical problems both with “taking sides” and with “not taking sides” in research environments as politically charged as Sudan’s.

There simply was no neutral vantage point from which I could coolly survey the scene. And even if there were, I am not at all sure a position of political neutrality would have been morally defensible in this research context, because neutrality would have been tantamount to supporting the status quo, which in this case favoured a national state government brutally suppressing large numbers of its own citizens. One wonders whether it is really possible to write any ethnography in a place like the Sudan that does not contain an implicit or explicit denunciation of the state. In such contexts, impartiality is an illusion – but sometimes a vital illusion which I consciously sought to cultivate through strategic transparency and blurred identities so as to protect both my informants and myself. Perhaps when trying to uncover “the truth” in perpetually violent environments, one can only follow the threads of scholarship so far.

Violence is the kind of subject that makes it difficult to maintain the illusion of research impartiality, especially when one becomes aware of the ramifications of the violence on those who are most vulnerable. But if this kind of scholarship is not done, how will the world know what is really happening in places like Sudan? Depending on where one falls with respect to these issues, my experiences may be
read either as a cautionary tale or as a comparative basis for exploring one’s own motivations and ethical choices in grappling with the immediacies of researching dangerous fields.

Note: This paper is based on fieldwork reflections and as such does not delve into the literature. However, a list of relevant sources is given below.


‘FROM NATION TO FAMILY’:
RESEARCHING GENDER AND SEXUALITY

Danai S. Mupotsa

Introduction

Considering the current political, social and economic crisis in Zimbabwe, along with the strong dependence on a discourse of ‘culture’ and respectability that nationalisms depend upon (See for example Yuval Davis 1997; McFadden 2001; Gouws 2005), I examine the multiple representations and interpretations of tradition and modernity as they apply to women, women’s bodies, masculinities, femininities, ‘culture’, sexuality and power. This requires an examination of the uses and abuses of calls to ‘culture’ in both justifying and naturalizing violence committed on the bodies of men and women (and the gendered nature of this violence) that are manifest within this crisis. While my interests are particularly concerned with the everyday practices often referred to as ‘our culture’ – the contentious issues surrounding being a woman, dressing for and occupying public space maintaining ‘respectability’ and adhering to the appropriate modes of conduct for a young woman – it is difficult to ignore how these discourses play a role in and are exacerbated by the current crisis.

In an open letter concerning the situation in Zimbabwe, Onannela Selolwane responds to Everjoice Win:

you identify the source of suffering clearly and unambiguously as politically motivated and organized violence. Like so much violence directed against women within the sanctity of the home and family, this is supposed to remain unnamed and unspoken, to protect families and men from shame. There is an assumption, as you rightly point out, that liberation war leaders, like husbands and heads of households, have earned an unlimited right to ‘chastise’ (read ‘abuse and violate’) with impunity those that are under their ‘guardship’. Because they are guardians, ‘providers’ and ‘protectors’, they themselves are protected from having their acts named as violence as this would signify that such acts are wrong morally and legally (Selolwane 2004, 77–78).
This paper investigates the methodological and ethical considerations for a study of young women, examining gender, sexuality, ‘culture’, and power as a means of scrutinizing the trope of ‘the nation’ in Zimbabwe. ‘Women’s experience’ is a particularly meaningful way to examine the constitution of both ‘the nation’ and ‘culture’. ‘Women’ in this sense must be viewed as both material and symbolic subjects. Examining ‘experience’ in this manner requires an epistemological shift from the position of unsituated objectivity. This in turn requires methodological and ethical transformation. These shifts allow us to avoid what has been described as ‘eating the other’,¹ that is, appropriating women’s experience and speaking as though ‘women’ are a universal subject. I assume the position of the participant/researcher as a means of attempting to bridge the gap between the ‘researcher’ and ‘researched’ and in doing so reflect on what are considered the main ethical concerns, when researching with human subjects: the concern for the safety of the research subjects; informed consent; privacy and confidentiality; and finally, how to deal with adverse situations that arise in the research process.

Denzin and Lincoln (2008, 199) remind us that the subject itself matters; ‘feminist self consciousness also identifies subtle forms of oppression and imbalance and it teaches us to address questions about whose interests are regarded worthy of debate.’ This has implications in regards to the nature and subject of the questions that my research wishes to ask, for instance, taking the narratives of young women on love, sex and marriage as relevant material for an analysis of the state of a nation. This also brings into question the appropriateness of unsituated objectivity in the production of knowledge.

Using feminist appropriations of standpoint theory, I interrogate the usefulness of ‘experience’, or perhaps ‘shared experience’ as a viewpoint from which to conduct social research. The much discussed ‘native ethnographer, ‘outsider within’ position requires careful interrogation. I offer a combination of feminist standpoint theory and feminist postmodernism, with a focus on both experience and discourse as a solution to this epistemological (and on the level of ‘eating the other’) ethical dilemma. Departing from the same ethical viewpoint, I elaborate on being both the ‘outsider within’ as well as a partici-

¹ The title of an essay by bell hooks (1999) where she interrogates the commodification of Otherness.
pant within this proposed research. Additionally, I explore potential research methods in light of this discussion.

**Researching Gender and Sexuality**

Ways of knowing have been at the forefront of feminist debates. In an essay titled ‘Engendering African Social Sciences: An Introduction,’ Ayesha Imam notes that ‘every theoretical and methodological framework of knowledge production has implicit values and assumptions about the nature of society, and will be resisted by those who do not have the same position’ (1997, 2). She adds that in an African context the notion of an ‘objective’ truth or knowledge has always been under scrutiny as ‘there had always been those who made anti-imperialist and class critiques in full recognition of their political implications’; in contrast, however, ‘the gender political implications of the sorts of knowledge produced has been almost studiously ignored and rejected by many African social scientists’ (Ibid., 14). These omissions are important to note, considering that half of human society is constituted of people gendered as feminine and as Imam so aptly states, a social science that does not recognize gender as a critical category is ‘an impoverished and distorted science, and cannot accurately explain social realities and hence cannot provide a way out of the present crisis in Africa’ (Ibid., 2).

Northern-based proponents note that traditional philosophies had, central to their imagination, the notion of objectivism, which presupposed that ‘knowers must detach themselves from their embodiment and their various beliefs (Assister 2000, 329)’. But as Assister argues, ‘feminist epistemologists have gone further, therefore and have argued that value-neutrality in science is a myth, because the descriptions, interpretations and explanatory phenomena in science inevitably involve social values. [And as] we are embodied and embedded creatures [...] these facts about us matter when making claims to know something’ (Ibid., 300). It is from this epistemological position that ‘women’s experiences cannot be understood within the consciousness

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2 While recognizing that efforts to incorporate gender analysis have been made in various fields of social science on/in Africa, I still contend that many approaches to using gender as a ‘tool’ for analysis continue to fail to account for the complexities the relations of gender and power sufficiently.
of those who rule; instead, feminism needs to develop a social form of expression and intelligibility that is separate from the male standpoint’ (McLaughlin 2003, 57).

Standpoint theories arise from Marxist critiques and argue that the oppressed have access to a different perspective on social life than those ‘in power,’ and feminist appropriations of this position argue the case for the investigation of ‘women’s experience’ (see Harstock 1987), particularly when the investigator has an interest in using ‘material experience to build collective theoretical understandings’ (McLaughlin 2003, 67). This is crucially important to note, because it highlights the importance of experience in constructing a standpoint, but the question of what ‘experience’ actually entails makes this discussion both more interesting and also more difficult as women are not all the same and we do not share the same material experiences, and therefore basing feminist standpoint theory on ‘women’s experience’ can be dangerously universalistic. As Blunt and Rose argue, ‘[claims] to know for absolute certain the true nature of all women, depend on and produce a space in which the essence of femininity is immediately accessible and transparently obvious’ (1994, 6).

Patricia Hill Collins’ ‘Black Feminist Thought’ gives one example. She begins by arguing that ‘Black women have had to struggle against white male interpretations of the world in order to express a self-defined standpoint’ (1991, 201). Her particular standpoint is designed to encompass ‘the experiences of being both Black and female in the United States’ (McLaughlin, 64) and is rooted in an ‘Afrocentric feminist epistemology’ which in turn, ‘is rooted in the everyday experiences of African American women.’ (Hill Collins 207). The implications of Hill Collins’ proposal are empowering, however the unease it provokes emerges from the very humanist approach her argument makes. As Jayat Lal argues,

the problems associated with standpoint epistemologies are paralleled in the presumed epistemology of the native insider. Both constructions are essentialist, and reduce either the native or the Third World3 woman to an assumed homogenous entity. Both suggest subjectivist, ideographic

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3 Hill Collins is referring to African American women in her case, while Lal’s analysis refers to ‘Third World women.’ I use them in the same discussion and here, interchangeably, as I am referring to feminist appropriations of standpoint theories emerging from critical race theory and postcolonial studies, as they both account for the intersecting oppressions of race and gender.
methodologies in the assumption that experience is the basis for knowledge. Both reduce the politics of location to the experience of (a presumed homogenous) identity. Furthermore, such constructions have the unintended effect of reinforcing the very distinctions that they are supposed to erase (1999, 114).

Susan Bordo warns that ‘attending too vigilantly to difference can just as problematically construct an Other who is an exotic alien, a breed apart.’ (Bordo 1999, 40). Reminding us of Foucault’s caution that ‘everything is dangerous,’ Bordo encourages a context-driven ‘hyper- and pessimistic activism, not an alliance with “correct” theory. For no theory – not even one that measures its adequacy in terms of justice to heterogeneity, locality, and nuance – can place itself beyond danger.’ (Ibid.) In attempting this ‘hyper- and pessimistic activism’, like Lal I wish to combine standpoint theory with a ‘postmodernist informed ethnography’ (Lal 1999, 102), rejecting a humanist approach to opt instead for the anti-humanist ‘subject’ position (Eagleton 1996, 189). This requires a recognition and analysis of both experience and discourse. Amina Mama describes discourses as:

Historically constructed regimes of knowledge [that] include the common-sense assumptions and taken-for-granted ideas, belief systems and myths that groups of people share and through which they understand each other. Discourses articulate and convey formal and informal knowledge and ideologies. They are constantly being reproduced and constituted, and can change and evolve in the process of communication. A discourse is a shared grid of knowledge that one or more people can ‘enter’ and through which explicit and implicit meanings are shared (Mama 1995, 98).

It has been often noted that ‘the use of Foucault has created a lot of tensions in feminist debates’ (Tamboukou 2003, 5). This has been due to ‘the lack of gender in his accounts, the inconsistencies in this portrayal of power and the unsuitability of his ethical framework.’ (McLaughlin, 120). Standpoint feminists are particularly concerned with his presentation as ‘this account of power appears to imply that any notion of structural power – say patriarchy – is false.’ (Ibid., 122). Feminists are not alone in their suspicion of Foucault’s presentation of power. Abiola Irele for example thinks that African writers ‘cannot afford the luxury of an unexerted nihilism or a morose anti-humanism’ (Abiola 2001, 87) that Foucault’s reading of history and power suggests.

This considered, I still recognize ‘Foucault’s theorisation of the subject as the most interesting area of his work, for feminists’ (Tamboukou, 5). Tamboukou notes his presentation of genealogy, writing:
Foucault’s project of genealogy has multifarious ways that the female subject has been historically and culturally constructed [...] genealogy is Foucault’s suggestion for doing research. It is both a mode of reflection on the nature and development of modern power and a theoretical tool for doing research. A key insight in genealogy is that truth cannot be separated from the procedures of its production. Consequently genealogy is concerned with the processes, procedures and apparatuses, whereby truth and knowledge are produced, in what Foucault calls the discursive regime of the modern era (Ibid., 6).

I find both theorizations useful and have had to consider a method of appropriating these theories and methods in a feminist manner, that is: in a manner that still offers a structural critique of power, while allowing for the more complex analysis of power that Foucault provides a model for.

The first method of appropriation I employ is in the selection of subject matter. Selecting a research area that involves tracing the genealogy of women’s experience (of youth, the body and space) begins this process. The structure of patriarchal power (like the other structural powers such as colonialism or nationalism) are very difficult to make absent as the discourses produced by/about/surrounding young women in/from Zimbabwe are unapologetically connected to such structures and their histories. Simultaneously these women are co/producers, participants, resistors and manipulators of said discourse and Foucault’s analysis of power as fluid (that is: not just from the top down) and complex allows for a ‘better’ feminist analysis; one that does not contribute to a dominant discourse of black women as the perpetual ‘beasts of burden,’ incapable of action or agency in the face of multiple oppressions. For example, I find Lila Abu-Lughod’s proposition of resistance as a diagnostic of power (Abu-Lughod 1990) to be quite useful.

Rosi Braidotti suggests:

But this recognition of a common condition of sisterhood in oppression cannot be the final aim; women may have common situations and experiences, but that are not, in any way, the same. In this respect, the idea of the politics of location is very important. This idea, developed into a theory of recognition of multiple differences that exist among women, stresses the importance of rejecting global statements about all women and of attempting instead to be as aware as possible the place from which one is speaking. Attention to the situated as opposed to the universalistic nature of statements is the key idea (1994, 163).
This is a useful elaboration of a situated epistemological standpoint similar to that which Bordo proposes (Bordo 1999, 40). It is also useful to begin to explore the position of being both an insider and an outsider within the research process. A number of authors have elaborated on the difficulties of being an ‘outsider’ – an ethnographer, and an ‘insider’, a member of the group or ethnos being studied – and have problematised the notion of ‘an authentic insider’ (Bordo 1999, 40). What I wish to do now is to consider the ethical and methodological choices I make as an ‘insider’ – that is, as a young, black woman from Zimbabwe, researching subjects who share the same material and symbolic experience on those terms.

Feminists have noted the necessity of self-reflexivity as a means of documenting the subjective aspects of the research process, the decisions taken at each stage, and the omnipresent power relations influencing these. The process of reflexivity or being explicit about the ‘I’ exemplifies the complexity of making decisions where power relations are involved. In addition, there is ‘a second dilemma to do with reflexivity regarding interpretation and the production of knowledge’ (Mauthner 2000, 301). As an ‘insider within’ who is interested in bridging the gap between ‘knowers’ and ‘subjects’, I propose my own entry into the research as both a participant and a researcher. That is, beyond ‘writing myself in’ as part of the project of self-reflexivity, I in fact participate in the project itself.

This endeavour is similar to that of Jayne Ifekwunigwe, who writes ‘first, embarking on such an endeavour would force a creative tension between the process of doing “insider/outsider” anthropology and the product, an ethnography that perpetuates the cultural anthropological remit: interest in other peoples and their ways of lie and concern to explain them within a frame of reference that includes ourselves.’ (2003, 202). In my attempt to close the distance between myself as a ‘subject’, and research participants as ‘objects’, placing myself in this ‘object’ position also has practical purpose when conducting research like this: for instance, the subject areas that this research attempts to address are topics often shrouded with shame or silence, and having the opportunity to share my own experiences within the research

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4 See for example Hill Collins, 1999; Narayan, 1989; Lazreg, Marnia, 1994; Onyango-Ouma 2006; and Obbo, 2006.
process does offer the prospect for research participants to feel comfortable sharing stories that they perhaps never had the opportunity to discuss.

I favour the semi/unstructured interview, which I find helpful because of its informal nature, because it allows the researcher to not only create a degree of rapport with the respondents, but also allows for a reciprocal relationship to foster between the two as the interview can take the shape of a conversation. This is beneficial when one attempts to discuss subjects that can be quite sensitive, such as sexuality, the body and life histories in space. I recognize that on the other hand, structured interviewing ‘involves exposing every informant in a sample to the same stimuli,’ (Russell 1994, 237), but this was not necessary to answer the questions my research interests require.

The oral interview ‘provides an opportunity for the researcher to hear the participant talk about a particular aspect of their life or experience’ (Willing 2001, 22), as feminist proponents of the interview technique such as Dana Jack and Kathryn Anderson note the potential of oral interviews in uncovering women’s experience but also offer that ‘to hear women’s perspectives accurately, we have to learn to listen in stereo, receiving both the dominant and muted channels clearly and tuning into them carefully to understand the relationship between them’ (Anderson and Jack 1991, 11). Listening carefully requires that instead of pursuing a particular point or set of questions strictly, only listening for a specific answer to that question, I need to follow the direction that the women I speak to take me in.

Anderson and Jack suggest that special considerations should be made when interviewing women, because women’s discussions of reality arise on two planes that often conflict: one is structured within a hegemonic patriarchal system, especially in relation to men’s dominant position, and the other is informed more closely by women’s personal experience. Anderson also notes the great distance between what women are willing to say at times and the questions a researcher wishes to ask. There are unwritten rules of conversation about appropriate questions and topics, which will hinder a researcher from encouraging women to make explicit the range of emotions surrounding the events and experiences they relate (Ibid., 13). Besides the fact that I want to extract information out of these women that they may or may not be interested in ‘giving up,’ a power dynamic arises between the researcher and the ‘researched,’ and this is problematic. Not only is it problematic: there also stands the possibility that in defence, the
‘researched’ may exercise their agency and offer a performance, rather than allow the researcher to extract from them. It is important that I participate in the research for these reasons. If I invest my own experiences and stories into the project itself I am also ‘giving up’ myself.5

In my experience of conversations about culture, sexuality and the like with my peers, I have noticed how it may sometimes be easier to speak about such experiences in the third person. One example comes to mind when a friend shared an experience of abortion. She told me about a friend who had found herself pregnant. Her sexual partner was an older, married man who advised her to terminate the pregnancy. As termination of pregnancies are illegal in Zimbabwe, (unless under very special circumstances),6 she was dependant on him to provide her with a doctor who was willing to perform the surgery (for an exorbitant fee). Some months later, a doctor inserted a number of pills into her cervix and explained that she would miscarry in a number of days. On the same day, the young woman underwent several hours of labour and heavy bleeding, after which a half-formed dead foetus emerged from her body while she sat on the toilet. The detail with which this woman shared this ‘gossip’ about ‘some girl’ she knew certainly led me to think that this was in fact her own experience – however, the shame of the experience prevented her from sharing the story as her own.

5 Ann Oakley (1991) speaks to this idea. This model has also been described as ‘collaborative story-telling’, an approach very similar to Latin American testimonios, in that it is the intention of the direct narrator (research participant) to use an interlocutor (the researcher) to bring his or her situation to the attention of an audience to which he or she would not have access because of his or her very condition of subalternity that the testimony bears witness,’ (Denzin and Lincoln 2008, 558). The difference would probably be that I, as a participant would also be involved in the project of telling my own story, as a member of the same subaltern group.

6 ‘Prior to the enactment of the Termination of Pregnancy Act of 1977, abortion legislation in Zimbabwe was governed by Roman-Dutch common law, which permitted an abortion to be performed only to save the life of the pregnant woman. The Termination of Pregnancy Act (No. 29 of 1977) extended the grounds under which a legal abortion could be obtained in Zimbabwe. The Act permits the performance of an abortion if continuation of the pregnancy so endangers the life of the woman or so constitutes a serious threat or permanent impairment to her physical health that the termination of the pregnancy is necessary to ensure her life or physical health, or where there is a serious risk that if the child to be born would suffer from a physical or mental defect of such a nature as to be severely handicapped, or where there is a reasonable possibility that the foetus is conceived as a result of unlawful intercourse. “Unlawful intercourse” is defined by the Act as rape, incest or intercourse with a mentally handicapped woman.’ United Nations Populations Division. 2002. Abortion Policies: A Global Review. http://www.un.org/esa/population/publications/abortion/. Accessed 18 August 2009. 190.
This could be viewed as a strategy for coping with what was certainly a traumatic experience – but also a strategy to prevent her from ‘losing’ respectability. I know this strategy well as I have employed it myself.

This story offers a good entry point to a discussion of the question of ethics. Laura Krefting offers a discussion of Guba’s Model of ‘Trustworthiness in Qualitative Research’, as an alternative to model of ‘accuracy’, and ‘validity’ as offered in quantitative research design. Krefting writes that ‘Guba’s model is based on the identification of four aspects of trustworthiness that are relevant to both quantitative and qualitative studies: (a) truth value, (b) applicability, (c) consistency, and (d) neutrality’ (Krefting 1991, 3). On a similar note, Christians adds that in traditional social science research the following code of ethics applies:

1. Informed consent: consistent to its commitment to individual autonomy
   a. Subjects must agree voluntarily to participate, [without] physical and psychological coercion.
   b. Their agreement must be based on full and open information.
2. Deception: emphasizing informed consent, social science codes of ethics uniformly oppose deception [and] deliberate misrepresentation is forbidden.
3. Privacy and confidentiality: the code of ethics insists on safeguards to protect people’s identities and those of the research locations. Confidentiality must be assured as the primary safeguard against unwanted exposure, the personal data ought to be secured or concealed and made public only behind a shield of anonymity.
4. Accuracy: ensuring that data are accurate is the cardinal principle in social science codes as well. Fabrications, fraudulent materials, omissions and contrivances are both nonscientific and unethical (Christians, 50).

In light of the previous anecdote, these considerations are important. For instance, this story as it was told to me was ‘gossip’. My use of this anecdote can in fact be used as inaccurate or untrustworthy based upon this set of principles because it represents the ‘spreading of rumours’. Win’s insights are useful here as she writes:

But you and I know the price that women pay for speaking publicly about the human rights violations they suffer. We know the questions that will be asked: what had she done? What was she wearing? Where
was she going? Who is she? Can we really believe her? In the case of Zimbabwe’s political violence against women, add another set of questions: which party is she from? Are you sure she wasn’t paid by the British? Is it really true that Robert Mugabe, a liberation war leader could do that? And in the case of the socio-economic crisis: surely these figures are exaggerated? Isn’t this just Western propaganda? (Win 2004, 75)

Selolwane adds:

[Win] also raises the issue of the ‘messenger’ who carries the stories of the suffering and violence to the world, only to be disbelieved or ridiculed as women so often are when they break the silence surrounding their abuse, and suggest this might be the reason why the credibility of such stories has been questioned in many (African) circles (Selolwane, 78).

These insights speak to why certain silences may be difficult to break when conducting such research. They also speak to the importance of research concerning the subject matter I have outlined. Not only are women ridiculed when we speak publicly about sexist cultural practices, as Win reveals, but we are often also blamed and our stories are delegitimized. The tool of speaking in the third person, for instance, may serve as a means of removing shame concerning a matter from one’s own body and hence respectability, and I wish to view ‘gossip’ in this sense as trustworthy research data.

The question of confidentiality is important too. As I intend on writing my own experience into the text it is important to consider the degree of confidentiality I grant myself. Additionally, conversations around such questions can also function as projects of consciousness-raising. There may be participants who wish to be named as this may present the opportunity to speak out about issues that concern them and for which they have never had an audience before. On the other hand, some of the insights shared may place research participants in danger for the political reasons Win and Selolwane outline above.

As my attempts at negotiating a standpoint reveal, the question of ‘neutrality’ comes to the fore. As I argued earlier, neutrality as it has been constructed in the social science is often problematic and will not be useful in my research endeavours.

Traditional research methodologies emphasize the importance of informed consent, highlighting that it is of utmost importance that informants’ agreement to participate in research must be based on full and open information. This is of concern to me, since the language I use to frame this research project – that of sexual and gender based
violence, within the spectrum of cultural practice – may be both foreign or offensive to research participants who may have their own language with which they articulate particular cultural practices. A woman may very well have an understanding of ‘gender-based violence’ that does not include any or all of the cultural practices that I may perceive as falling under the same category, for instance. I think that this is an important discussion to have with research participants and I suspect the semi-structured oral interview can serve the purpose of opening dialogue about what aspects of cultural practice we feel comfortable describing in certain language and those aspects we do not. This sort of discussion would certainly enrich my overall analysis.

Mauthner’s insights are also important here, as she notes that despite the attempts at a collaborative research process at the level of data collection and even when analysing the data, it is the researcher who writes up the document. I think it is important that we, as researchers not only offer full disclosure about our research interests at the beginning of the research project, but are also prepared to share the research findings and conclusions with the participants in our research. I believe this holds us accountable in regards to representing the views of the people contributing to our work and I think it offers an opportunity to enhance the quality of our work.

**Conclusion**

This paper has offered a short discussion of what I am calling a ‘feminist intervention’ in research methodology. This intervention occurs most importantly at the epistemological level, questioning the basic assumptions of traditional research methodology. I offer ‘conversations’ amongst my peers concerning culture as a means of conducting such research. Considering the ethics of social research, I suggest that ongoing interrogation of the relationship between the ‘researcher’ and ‘researched’ is very important. I also suggest that traditional codes of ethics that call for ‘neutrality’ and ‘accuracy’ and ‘trustworthiness’ need to be questioned in light of the specific research context, in order analyse the discursive practices of women.
References


Between 2006 and 2010, the United Nations Institute for Disarmament Research (UNIDIR) ran an initiative called the Security Needs Assessment Protocol (SNAP) project. Its purpose was to explore the value of a ‘best process approach’ to generating local knowledge and applying that knowledge as a strategic asset in the design of local action for peace and security. This work was a continuation of UNIDIR’s long-standing commitment to improving the viability, local acceptance, and efficacy of peace and security-building activities by the United Nations generally, and to assist agencies that are working to build community security, or else design their own development or humanitarian programmes in contexts of community insecurity. The research and development phase of SNAP concluded in 2010, leading to or otherwise supporting a series of new ventures. These include the founding of The Policy Lab as the successor to the agenda; the launching of the Center for Local Strategies Research at the University of Washington; the establishment of the Agenda for Strategic Design and Public Policy by UNIDIR, the Said Business School at the University of Oxford, and the Center for Local Strategies Research, and – thus far – the initiation of at least a half-dozen project and programme initiatives. SNAP’s primary investigative technique was cultural research grounded in the ethnography of communication, and its strategic vision was influenced by an intellectual agenda suggested by Adda B. Bozeman in the 1960s and 1970s.

SNAP begins with the assumption that effective programming for local communities starts with local knowledge, and that local knowledge is best applied through an innovative process of service design.\(^2\)

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\(^1\) The views expressed in this paper are the sole responsibility of the individual authors. They do not necessarily reflect the views or opinions of the United Nations, UNIDIR, its staff members or sponsors. Special thanks to Lisa Rudnick for her instrumental ideas on these matters and Nikhil Acharya, Assistant Researcher at UNIDIR for research support, insights and editing.

\(^2\) All references to SNAP build off Miller and Rudnick’s cooperative work between 2003 and 2009, with support and ideas from the project’s Advisory Group, which
Towards this end, the UNIDIR programme of work is aimed at creating systematic, rigorous and replicable means of assessing local security problems as they are understood by community members themselves. This Security Needs Assessment Protocol may be used by interested UN agencies and others that implement security, development, and humanitarian field activities in conditions of community insecurity.

It became clear near the beginning of the project that the issue of ethics in the conduct of cultural research on peace and security in the service of UN operations, and often in conflict zones was insufficiently addressed within the UN system itself. As a consequence, the subject would need to be dealt with head-on. The work we produced in the service of that project has since proved to have wider application and implications, thereby justifying an article for a wider audience with interest in these matters.

The need to address ethical concerns came from a general observation that UN personnel, like those in other international agencies, live and work within the highly complex constraints of their agency’s mandates, international and national laws, inter-agency cooperative agreements, as well as within sociocultural groups comprised of different nations and cultures. Researchers and scholars on cultural matters are often members of professional associations with codes of conducts or ethical review mechanisms. Consequently, personnel who conduct Security Needs Assessments will try and achieve the highest-quality analysis for the benefit of policy and social change in the midst of complex ethical constraints – some of which are explicit, but many of which are not.3

We believe that the work of such personnel is sufficiently complicated that it cannot be solved by simple introspection on one’s own behaviour and a general willingness to act ethically. Indeed, the model of inquiry that SNAP uses is premised on the fact that research find-

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3 A draft of this paper was written in November 2007 as a Background Paper for UNIDIR in order to provide some guidance on this complex topic internally. Consequently, this was a purposive investigation to provide clarity on how the Security Needs Assessment Protocol could be built and applied in an ethical way. Following its presentation at the School of Oriental and African Studies at the University of London in July, 2008, we concluded that it may be suitable and interesting to a wider audience.

has included (in no particular order): Donal Carbaugh, Gerry Philipsen, Ron Scollon, Tamar Katriel, Rom Harré, Michael Agar, Kwesi Yankah, Michael Berry, Randolph Kent, Wendy Cukier, Anna Wierzbicka, and Fathali Moghaddam. Ideas on service design have been developed with the support of Lavrans Løvlie and his colleagues at Live Work in London and Oslo.
ings will be used to effect social change. Assuring that the work is conducted and applied ethically is crucial to the well-being of all involved. It is also necessary for the utility and sustainable success of the designs to be applied to the research itself.

The process of conducting this research and applying its findings through programming is fraught with challenges that need to be boldly addressed for the simple reason that field-level activities by the UN, international organizations, states and others are being designed and implemented every day without due consideration to their inherent ethical complexities. When referring to programmes, or programming, we are directing attention to such activities as, mine clearance, small arms collection campaigns, public outreach initiatives, the reintegration of excombatants into civilian society, research on child soldiers in conflict zones, disrupting human trafficking and organized crime systems, conducting community-level workshops and meetings, designing and implementing national programmes on reconciliation and justice, and efforts to stem abuses by the security sector and to increase their accountability and oversight. In short, programming directs us to profound and significant processes of social change that can be designed wisely, ethically, or otherwise.

Consequently, there is an urgent need to articulate the types of challenges faced when conducting cultural research on peace and security generally, and not just in the context of Security Needs Assessments specifically. This paper is therefore a first step towards that end and introduces a new model of ethics for cultural research and programme design on peace and security. It is called Cooperative Ethics and – though it will need a great deal of refinement and work by ourselves and others – we believe it may prove beneficial in advancing this agenda.

This paper is divided into four sections. Part I, The State of Contemporary Debate on the Ethics of Cultural Research, examines the relevant history of applied cultural research and the ethical guidelines established to direct it. Attention is focused on the American Anthropological Association (AAA)’s Code of Ethics and the history of events leading to its adoption. The AAA since 1902 has been the ‘world’s largest organization of individuals interested in anthropology.’

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is not a general history of the Code or its application, but rather a purposeful and limited inquiry into its primary structuring ideas.\textsuperscript{5}

Part II, Two Models of Ethical Action and the Nexus of Controversy, differentiates between the ‘Science Model of Ethics’ that grounds the AAA’s approach to research with a second model that we call the ‘Mandate Model of Ethics’. To illustrate the Mandate Model as juxtaposed to the Science Model, we look at the controversy around the U.S. Army’s Human Terrain System in which cultural research done by anthropologists and sociologists is being used to assist military matters. This section demonstrates that the primary ethical concerns of the AAA, codified in 1998 and reiterated in 2007, are challenged by a second form of research legitimacy gleaned from political channels and mandated actions in the context of legal agreements. The way that people negotiate ‘ethical’ conduct is, we argue, generally through one of these two channels, and the two have yet to be successfully negotiated into a unified system.

Part III, Towards a Post-Scientific Ethic in Cultural Research, explains how neither the Science Model of ethics nor the Mandate Model of ethics provide sufficient guidance for cultural research into peace and security for the benefit of designing policy and practice. We explain how the concern for protecting studied people which evolved from the 1947 Nuremberg Code is grounded in a theory of anthropology-as-science that does not afford it space to engage in deliberate acts of policymaking and social change, which are the hallmark practices associated with international security, development, and humanitarian action. The alternative model is therefore the Mandate Model, which we believe is currently active in discourses on ethics by policy actors, but we also believe to be insufficient for providing a basis of mutually ethical engagement with studied communities.

The point of policy research on peace and security, whether that research is grounded in cultural research, statistics, or otherwise, is to inform later actions. Likewise, those actions are intended to change the status quo of the studied society in order to bring about some amelioration of a tension. The principle of ‘do no harm’ has been widely accepted by both the scientific community and now the humanitarian relief and development communities (Anderson 1999). However, there is still no way for either the scientist or the policy-

\textsuperscript{5} While SNAP is not grounded in the academic field of anthropology \textit{per se}, the AAA Code of Ethics nevertheless exerts a profound impact the professional study of cultural systems. It is therefore a benchmark that is worthy of attention.
maker to know what will constitute harmful action in any given social interaction, especially where communication and actions move across cultural and linguistic borders.

On this basis, we explain how modern efforts to apply cultural research to the task of designing peace and security initiatives such as the Security Needs Assessment Protocol now require a set of ethical practices that will allow for better encounters between the researcher and the studied community in the context of institutional, mandated action intent on policy advice and social change. We call this the ‘Cooperative Model’. The key to the Cooperative Model is mutual negotiation of meanings among stakeholders, especially highlighting the contributions of the communities themselves.

Part IV, Towards a Model of Cooperative Ethics in Cultural Research on Peace and Security, generates a first-pass at questions and themes that may help structure the encounters between agencies and the beneficiary communities when applied cultural research for designing or redesigning policies and practices. A comparative chart is provided on the Scientific Model, the Mandate Model and the Cooperative Model, which is then explained.

While each dialogue and real-world research encounter will inevitably be different, a common set of themes – and a common set of practices to use in advancing ethical action in the context of those themes – is helpful to explore. This section is therefore exploratory and is intended to stimulate engagement and invite elaboration.

The paper ends with final thoughts about the urgency of this project to develop and employ a Cooperative Model for cultural ethics in support of peace and security given the on-going, daily work of international actors and the likely expansion of work in this area.

Part I: The State of Contemporary Debate on the Ethics of Cultural Research

In 1947, the Nuremberg Code was adopted on permissible medical experiments following an investigation into the actions of Nazis during World War II and the Holocaust. The code established ‘voluntary consent of the human subject’ as ‘absolutely essential’ in its first of ten directives.6

6 There is some scholarly debate on whether the 1947 Code was the first to provide explicit rules governing medical research on human beings. Jochen Vollamann
From 1947 onward, codes of ethics were written and adopted by many organizations and societies where scientific research was conducted either on or simply with people. One such resolution was passed by the AAA in 1948 on freedom of publication and protection of the interests of the persons and groups studied. The American Anthropological Association, though not involved in medical experimentation per se, nevertheless drew an explicit relationship between medical experimentation on human subjects and acts of social investigation, thereby necessitating informed consent. In doing so, it helped establish the people or peoples studied as being ‘human subjects’ in modern parlance, and in turn, positioned the anthropologist as ‘scientist’, and hence subject to the codes that govern scientific inquiry. The Association wrote:

Be it resolved: (1) that the American Anthropological Association strongly urge all sponsoring institutions to guarantee their research scientists complete freedom to interpret and publish their findings without censorship or interference; provided that (2) the interests of the persons and communities or other groups studied are protected; and that (3) in the event that the sponsoring institution does not wish to publish the results nor be identified with the publication, it permit publication of the results, without use of its name as sponsoring agency, through other channels.

Though informed consent is axiomatic today, and the ethics seem obvious, it is helpful to recall that as recently at the nineteenth century research was conducted on patients in hospitals without their knowledge under an ‘ethos of science and medical progress’ Vollman and Winau (1996).

The AAA was not alone in evoking and applying the Nuremburg Code, however. So too did many other research communities that also did not undertake medical experimentation. In the case of anthropology and sociology, this was motivated by the Nuremberg Code, but and Rolf Winau, for example, demonstrate how ‘new research [from 1996] indicators that ethical issues of informed consent in guidelines for human experimentation were recognized as early as the nineteenth century.’ Determining the original dates for extant codes, they explain, is important for judging the defenses used by Nazi doctors. Many argued that there were no explicit rules and that the research practices in Germany were not different from those of allied countries. What is more important here, however, is noting how the widespread – and morally potent – code from 1947 was influential in affecting international research conduct from that time onward, whether or not it was in fact the first. For more information see Vollman, Jochen and Rolf Winau (1996). 313(7070) .
also strengthened by their own historical concerns about how their research findings had been and were still being used to support colonialism and often racist policies.

The 2001 Code of Ethics for the International Sociological Association, for example, echoes the 1947 Nuremberg Code and the 1948 resolution by the AAA. They write in their Preamble:

Sociologists work to develop a reliable and valid body of scientific knowledge based on research and, thereby, to contribute to the improvement of the global human condition. The primary goals of the Code of Ethics, a symbol of the identity of the ISA, are (1) to protect the welfare of groups and individuals with whom and on whom sociologists work or who are involved in sociologists' research efforts and (2) to guide the behaviour and hence the expectations of ISA members, both between themselves and toward the society at large. Those who accept its principles are expected to interpret them in good faith, to respect them, to make sure they are respected and to make them widely known.

These concerns over the welfare and protection of those studied were even applied in fields where there was no direct relationship between the researcher and people at all. Rather, the relationship was abstracted to bring ethical guidelines to the possible implications of research findings on people more generally.

Illustrative of this abstraction is this inclusion in the Ethical Guidelines of the American Mathematical Society, adopted in January 1994:

When mathematical work may affect the public health, safety or general welfare, it is the responsibility of mathematicians to disclose the implications of their work to their employers and to the public, if necessary. Should this bring retaliation, the Society will examine the ways in which it may want to help the ‘whistle-blower’, particularly when the disclosure has been made to the Society.

It is in anthropology, however, where the greatest similarities exist between the conduct of SNAP itself and the academic community. This is due to the subject itself, namely the study of cultural systems, but more importantly due to the similarity of research practices involved in that investigation. As we will explain, however, the alignment is incomplete and therefore leaves significant questions unanswered in terms of ethical conduct.

Between 1948 and 1999 (the most recent version of the AAA’s code examined here), two ethical challenges have faithfully remained in all
documents on ethics and professional responsibility for anthropologists. The first is the matter of ‘openness’ (and its counterpart, secrecy), and the second is ‘protection’ of people subject to anthropological study.  

We believe that both these concerns are premised on the notion of ‘anthropology as science’ in the first case, and ‘anthropologists as scientists’ in the second. The difference is subtle but important. The first is an alignment of scholarly practices with scientific method, and as such, requires that the discipline be afforded the same privileges and protections as other sciences. The second appreciates that individual anthropologists are scientists and that people are the subject of their study. As such, it places a moral and ethical burden on them to protect the ‘human subjects’ of their inquiries in line with the Nuremberg Code of 1947 even if the Code itself is not invoked.

There are ready illustrations of both these tenets. In 1948, the same year that the aforementioned resolution was passed, the Executive Board of the AAA submitted a statement on human rights to the United Nations as it was preparing a document that later became the 1948 Universal Declaration on Human Rights. The statement provided by the AAA was deemed controversial at the time due to conflicting notions of ‘science’ and ‘universal rights’ and whether the AAA can or should be addressing the latter in an advocacy role.

H.G. Barnett, for example, argued that the AAA’s statement ‘places the Association on record in a way that embarrasses its position as a scientific organization.’ He goes on to write that ‘It is an inescapable fact that we cannot at the same time be moralists (or policymakers) and scientists (Barnett 1948, 352, quoted in Engle 2001, 543).’ Julian Stewart, also writing in 1948, was in general agreement. He wrote, ‘As a scientific organization, the Association has no business dealing with the rights of man’ (Stewart 1948, 351 quoted in Engle, 543). Whether their arguments about science and morality are accepted today is not relevant to the case we are building. What matters is how they made their arguments on the basis of anthropology being a science, and indeed, emphatically so.

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7 Many other topics are covered in the Principles of Professional Responsibility for AAA members, but most are specific to universities (including teaching responsibilities and other matters). They are not addressed here as we are purposely interested in the extent to which the AAA ethical guidelines can be used to inform research on security needs assessments specifically, and policy research more generally.
In November 1965, anthropologist Ralph L. Beals was requested by the Council of Fellows of the AAA, through the Executive Board, to carry out an investigation into the problems of anthropological research. The consequent result was Beal’s *Background Information on Problems of Anthropological Research and Ethics*.

According to Beals, writing in a 1967 retrospective on the lessons from that document, the 1948 Resolution had not led to a great deal of follow-up. It was not until ‘government agencies began to support anthropological research abroad that questions began to be asked both in the United States and in other countries’ (Beals 1967, 472).

By the mid-1960s, however, the Vietnam War and the increasingly-complex relations between scholars and government had prompted growing concern in the scientific community that anthropologists in particular, and social scientists more generally, needed guidance on how to avoid or ameliorate conflicts of interest between science and policy. Specifically, should anthropology as a science, and anthropologists as being ethically responsible scientists, use their skills to inform governmental policies that could, in turn, be used to harm the people being studied?

On the basis of Beal’s well-received paper, the Fellows of AAA voted in April 1967 to draft and accept the *Statement on Problems of Anthropological Research and Ethics* (Wakin 1992, 7). Interestingly, the stated purpose of that document was ‘to maintain the independence and integrity of anthropology as a science…’, which reinvigorated the original concerns from 1948. To be sure that their message was heard beyond the confines of disciplinary boundaries, they mentioned how they were ‘concerned here with problems that affect all the fields of anthropology and which, in varying ways, are shared by the social and behavioural sciences.’

From 1968 onward, attention to issues of ethics and professional responsibility never waned. Though a few intermittent versions may have escaped our attention, the progression appears to be:

- 1966, November: An extended report of Beal’s investigation was presented to the Executive Committee in summary form.
- 1967, January: A revised version of the summary report was published in the fellow newsletter.
- 1967, April: The Statement on Problems of Anthropological Research and Ethics was submitted by mail ballot to the Fellows. This led to a vote in which 92.5% of the 788 Fellows returned valid ballots.
• 1971, May: The Principles of Professional Responsibility was adopted and published.
• 1971 as amended through 1976: The Principles of Professional Responsibility was amended.
• 1971 as amended though November 1986: The Principles of Professional Responsibility was amended again.
• 1971 as amended through October 1990: The Principles of Professional Responsibility was amended yet again.
• 1998, June: The Code of Ethics of the American Anthropological Association was released.

In November 2007, the AAA released the *AAA Commission on the Engagement of Anthropology with the US Security and Intelligence Communities, Final Report*. When it comes to fieldwork and anthropological research on security they concluded that ‘As long as anthropologists are following appropriate guidelines for disclosure, informed consent, protection of subjects and data, and dissemination of research findings, the Commission found no special considerations that should apply to work conducted inside the national security community.’ However, they emphasize:

> The form of fieldwork that *did* engender a great deal of debate among the Commission members was a (then-hypothetical) situation in which anthropologists would be performing fieldwork on behalf of a military or intelligence program, among a local population, for the purpose of supporting operations on the ground. This raised profound questions about whether or not such activities could be conducted under the AAA’s Code of Ethics, not to mention the requirements of most human studies review boards. Although we considered this situation as a hypothetical example, the emergence of the Human Terrain System [by the U.S. Army] demonstrated that our hypothetical musings were not so far off the mark. We discuss the HTS in the recommendations below.

In the next section we compare the concerns of the AAA to another model for conceptualizing the ethical actions of the HTS to demonstrate that the two models are less in disagreement than situated in entirely different discourses about ‘research’ and what it actually is.

*Part II: Two existing models of ethical action and the nexus of controversy*

In the previous section we built the case for understanding the Code of Ethics for the AAA as being a product of viewing Anthropology as sci-
ence, and anthropologists as scientists. We see a linear progression (i.e. without serious debate or revision) running from the 1947 Nuremberg Code to the 1948 AAA Resolution, through Beal’s 1967 report, to the 1971 Principles, straight through to the 1998 Code of Ethics and the November, 2007 report.

In this section we aim to name and describe what we see as two extant, influential, if still implicit models for conceptualizing and judging ethical action as it impacts matters of security and cultural research. This first system, described above, we have dubbed the ‘Science Model of Ethics’. In this model, as the AAA report demonstrates, the use of science to cause harm – or direct how harm can be caused – is ethically untenable.

The second is illustrated by the HTS. Though the internal debate among anthropologists continues, there is nevertheless wide-ranging concern about, or else condemnation of HTS social scientists by their own academic communities who see their engagement with the Army’s efforts as dubious or unethical (Gonzales 2009). What we aim to demonstrate here is how the HTS is actually – if implicitly – grounding its own counter-arguments about ethics in a different discourse with a different history. This in turn provides it with cues and ethical parameters for action derived from an entirely different logical structure than the code that guides the AAA and its members. We call this second discourse the ‘Mandate Model of Ethics.’ Premises about ethical action pertaining to cultural research will necessarily clash (and often unreflectively) when people employ these two models of ethical discourse to try and ‘make sense’ of what is happening in ‘research sites’ such as Afghanistan and Iraq.

The Human Terrain System as an example of the Mandate Model for Ethical Cultural Research

In Military Review, Kipp, Grau, Prinslow and Smith (hereafter Kipp et al.) published an article called ‘The Human Terrain System: A CORDS for the 21st Century’. The article begins:

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8 See also “Failure in the field: The US military’s human-terrain programme needs to be brought to a swift close” in Nature, 456, pp. 676, December 2008.
9 This question has also been usefully developed in the final report of the AAA Commission on the Engagement of Anthropology with the US Security and Intelligence Communities, November 4, 2007.
Conducting military operations in a low-intensity conflict without ethno-
graphic and cultural intelligence is like building a house without your
thumbs; it is a wasteful, clumsy, and unnecessarily slow process at best,
with a high probability for frustration and failure. But while waste on a
building site means merely loss of time and materials, waste on the battle-
field means loss of life, both civilian and military, with a high potential
for failure having grave geopolitical consequences to the loser (2006, 8).

The Human Terrain System itself, as they explain, is a program to ‘help
address…shortcomings in cultural knowledge and capacities [of the
military]’ and is run by the Foreign Military Studies Office (FMSO),
which is a U.S. Army Training and Doctrine Command organization
that supports the Combined Arms Center at Fort Leavenworth, Kansas.
Because the Army leads in doctrinal development for all U.S. military
forces, it also leads conceptual work in joint warfighting and support-
ing systems. The HTS is such a system, through it remains in its early
stages. The lead author of the piece is the Director of the FMSO.

The purpose of the programme is to address ‘cultural awareness
shortcomings at the operational and tactical levels by giving brigade
commanders an organic capacity to help understand and deal with
‘human terrain” – the social, ethnographic, cultural, economic, and
political elements of the people among whom a force is operating’
(p. 9). In practice, this means employing a five-person ‘Human Ter-
rain Team’ that will ‘be embedded in each forward-deployed brigade
or regimental staff’ to offer knowledgeable assistance.

The authors of this piece – each one a current or retired military
officer at the time the article was published – are quite explicit about
their goals. They wish to achieve more effectiveness in military actions
in order to win conflicts with the lowest number of casualties for civil-
ians and military personnel. Seen from another perspective, neither the
value of winning nor the value of saving civilian and military lives is
in doubt. They are axiomatic to the argument they are making. In this
sense, the HTS is (for them) therefore just another tool to achieve the
government’s policy objectives, which the military has been directed
to fill.

The author’s axiomatic take on the HTS as ethically legitimate is,
in our view, entirely warranted when seen from the Mandate Model
perspective – despite it being possibly unethical and illegitimate from
a Science Model perspective (though the verdict on this remains con-
tested). Our goal here is not to judge the program but rather to explain
how it functions as a deeply controversial activity depending on the
model of ethics one employs to value it. And, as the UN and govern-
ment agencies also operate under mandate systems, we explore what this portends for peace operations and cultural research being conducted in its support.

We believe that the reason for the authors’ sense of legitimacy also has its origins in the 1947 Nuremberg Code, but for very different reasons. At those proceedings, it wasn’t only the scientific community that took away lessons. If the Nazi doctors argued that informed consent was not required and hence made their repugnant actions permissible, the military officers regularly argued that they were merely following orders and hence had a foundation on which to excuse their own conduct.

In rejecting the former claim, those studied were positioned as ‘human subjects’ and the notion of ‘informed consent’ was elevated to a chief concern of the scientific community. In the rejection of the latter claim, the notion of ‘lawful action’ became a foundational concept in what soon became the Uniform Code of Military Justice (UCMJ) in the United States, which became effective in May 1951. It was here, from the same proceedings, that two different genealogies on the ethics of applied social research were born – and today exist as separate discourses on ethical action for cultural research.

It is helpful to linger for a moment on how that second genealogy progressed. The U.S. Federal and Military Officer Oath of Office, which comes from US Code Sec. 3331 (01/24/94) Oath of Office (Title 5, Part III, Subpart B, Chapter 33, Subchapter II, Sec. 3331) reads:

Oath of office – An individual, except the President, elected or appointed to an office of honor or profit in the civil service or uniformed services, shall take the following oath: ‘I, AB, do solemnly swear (or affirm) that I will support and defend the Constitution of the United States against all enemies, foreign and domestic; that I will bear true faith and allegiance to the same; that I take this obligation freely, without any mental reservation or purpose of evasion; and that I will well and faithfully discharge the duties of the office on which I am about to enter. So help me God.’

The oath makes officers swear to defend the Constitution, not the country or its leaders. It is therefore no accident that the UCMJ continually refers to ‘lawful action,’ and by implication, recognizes the

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possibility and existence of unlawful action. The notion of lawfulness is regularly evoked as the foundation for conduct and the grounds for reprimand. For example, even in the most extraordinary circumstance of assaulting or willfully disobeying a superior commissioned officer (as in Section 890, Article 90), the issue of lawful commands (and hence a recognition of unlawful commands) is provided:

Any person subject to this chapter who –
(1) strikes his superior commissioned officer or draws or lifts up any weapon or offers any violence against him while he is in the execution of his officer; or
(2) willfully disobeys a lawful command of his superior commissioned officer; shall be punished, if the offense is committed in time of war, by death or such other punishment as a court-martial may direct, and if the offense is committed at any other time, by such punishment, other than death, as a court-martial may direct.

What is in place here is a system that is explicitly grounded on an oath to uphold law which is, in turn, subject to Constitutional law, and – by virtue of the Constitutional structure – the democratic system that maintains the practices of Constitutionality in the U.S. context. To act lawfully is, in this model, to act ethically, because ethical action is grounded in democratic practice, and hence the will of the people. This is a bold move in our argument, so it is worthy of some explanation.

If the Science Model of Ethics is, for the AAA, founded on the twin notions of anthropology-as-science and anthropologists-as-scientists, we are going to suggest that the Mandate Model of Ethics, for the U.S. Army, is grounded – through the oath, and through law via the UCMJ – on the notion of soldier-as-citizen. This, of course, is not a new notion and can easily be traced back to at least the Roman Empire where it was notably important.

When a soldier-as-citizen chooses to engage in research on anything, from radioactivity to cultural practices, that person is engaged in a purposeful activity to fulfil a role as a member of a community with distinct properties and needs that are effectively co-terminus with national borders. While two or more nations may share identical values and even engage in identical practices (for example, they may fight together for a common cause), the soldier-as-citizen is beholden to the state. For the American soldier-as-citizen, the soldier is a citizen who acts within the laws, and for the protection of the United States. While the exact chain of argumentation is mutable, a reasonable formulation may look like this:
‘The People’ maintain a Constitution which ensures democracy which creates representative government which directs the military which makes for legitimate action in the service of the state which provides a ground for ethical action such as cultural research on security.

At the centre of this formulation is a Western, liberal political philosophy that emerged during the Enlightenment about social contracts and the responsibilities of the person to the state, particularly in a democratic state. If the ‘sovereign’ is no longer the aristocracy but the people, then the will of the people is expressed and adjusted through the organs and policies of the state. When people act lawfully in the exercise of those policies, they are upholding and executing the social contract. When the soldier-as-citizen undertakes a lawful research activity, it can be read in this way as ethically justified and even as noble effect for a common good.

This is quite different indeed from a philosophy of science that sees the individual as a member of a scientific, trans-border community. Also harkening back to the Enlightenment, this view sees the scientific enterprise as a universal one, which only stands to reason since the laws of nature are universally experienced and hence can be studied by anyone. But this community of scientists is unified, not by a bounded social contract and joint expression through democratic activities, but rather by a common goal of scientific understanding. If we accept the premise that anthropology is a science, which it aspires to be (as documented in the AAA’s proceedings), then anthropologist-as-scientist would imply an orientation to contributing to the universal undertaking that is the scientific goal. It would militate, in turn, against ‘anthropologist as citizen’ working in the service of a non-universalist goal and in the distinct service of the state, which could also imply against the wishes of those being studied.

Some quick illustrations are helpful. In Beal’s 1967 article in *Current Anthropology*, his first two sentences read: ‘Former President Lázaro Cárdenas of Mexico once wrote, “The wish to learn has no frontiers.” In the physical sciences, science is international in a very direct way.’ He then goes on to align anthropology with this tradition and emphasizes the need for international comparative research to make the most significant advances.

But when an American soldier is asked to be a researcher, the waters become clouded. Being an American soldier is not an ‘international activity.’ It is a national one.
A semiotic description helps here. If we mix anthropologist-as-scientist with soldier-as-citizen, we get anthropologist-as-citizen, and soldier-as-scientist. What tensions would these people experience?

The first person would naturally want to apply his or her skills as a researcher in the service of the country. The second person would identify him or herself as an internationalist member of a scientific community but working in a parochial service.

In fact, these people exist. And unsurprisingly to us, they are conflicted. The first person becomes conflicted when the scientific community subverts that person’s actions as ‘unethical’ because it rejects the principles that undergird the Science Model of Ethics, including notions of openness and transborder learning. The second person is conflicted when the pursuit of parochial national interests appears to harm the pursuit of human advancement through the open sharing of knowledge.

In the next section we will attempt to move beyond the current impasses between the two models described here. We find this necessary because a new research agenda is emerging – at the United Nations and elsewhere – that requires cultural research on peace and security in order to help design actions of benefit to those being studied. In this case, the work is not necessarily contributing to scientific advancement (though it might) because it is oriented to the discovery or articulation of local knowledge for the purpose of culturally-situated action. So this research cannot comfortably sit with the ethics of the scientific model. But this research is also not being produced solely in the service of the researcher’s own community (i.e. the ‘state’ or in this case the UN), but rather aspires towards generating, interpreting and applying knowledge that could contribute to cooperative engagement between or among two or more cultural systems. This makes the Mandate Model equally insufficient for ethical guidance because the researcher is working in the service of a mutual goal (i.e. peace and security) with those being studied. These people are no longer ‘human subjects’ but cooperative agents in the mutual process of discovery. What is required, therefore, is a new footing for ethical guidance when undertaking cultural research on peace and security that will result in action.

Ours is not so much a reconciliation of the two models, but is rather a third model entirely that looks forward to a new set of actions in which cultural research is used to assist some matter of policy (rather than contribute to science), and when the people studied are not to be
treated as human subjects in need of protection but rather as cooperative partners in that endeavour. Towards this end, we aim to show how new ways of conceptualizing ethical responsibilities in the context of mandated but non-coercive action require a move beyond the current controversies examined in the previous section.

If we do not engage these problems successfully, the conclusions reached today may once again have international consequences that could limit the value of knowledge being used to design more cooperative and successful interventions with local populations. This is important because, as we have asserted, policy research and invasive international action takes place everyday whether or not it is duly informed by sufficient ethical considerations.

Part III: Towards a Post-Scientific Ethic in Cultural Research

Kipp and the co-authors of the Military Review article make some rather interesting claims that few cultural researchers would readily dismiss. In trying to make the case for the importance of cultural research (a case that anthropologists have to make daily to their departments, universities, at conferences, to interested students), they write that, ‘What has emerged… is a broad consensus that civil society in Iraq and Afghanistan… constitutes the real center of gravity.’ They go on to conclude that, ‘to truly understand [their] expectations and desires, it is imperative to view them from the perspective of the cultures in which the insurgencies are being waged.’

Seven years earlier, in writing about small arms proliferation and conflict in West Africa, Lasana Kouyate, then-Executive-Secretary of the Economic Community of West African States (ECOWAS), wrote the following:

[For the Declaration of a Moratorium on the Importation, Exportation and Manufacture of Small Arms and Light Weapons in West Africa] to have practical impact on the ground, we need the involvement of civil society; that is, the direct implication of those who suffer most from the scourge of small arms and light weapons proliferation. For effective and efficient action to take place on sustainable basis, ordinary women and men from West Africa must be closely involved in the process. This people-based engagement is certainly the best guarantee for success.

From a lay perspective, one might be forgiven for thinking that the view of the Training and Doctrine Command of the U.S. Military –
generally under assault from those who study culture professionally – sounds a great deal like the former head of the biggest West African regional organization concerned with stemming the flow of weapons, and hence perpetuation of conflict. And to complicate matters further, Kouyate published his views with the United Nations Institute for Disarmament Research in a volume entitled *Bound to Cooperate: Conflict, Peace and People in Sierra Leone* (UNIDIR 2006).

It is our view that it is both pragmatic and ethical to learn what local populations consider reasonable if the goal is to ‘build local ownership.’ Learning about the plurality of local ways of thinking about and practicing ‘security’ is now being transformed from mere academic curiosity about comparative anthropology to a pragmatic endeavour to enrich front-line operational ability within the United Nations, international organisations, and government agencies in order to build peace, not to win wars. And yet, as we see from the quotes by Kipp et al. and Kouyate, building peace seems to share a lot in common with the activities needed to win wars. This is why the prior distinctions between science and citizenship are breaking down. Indeed, U.S. military doctrine on stability operations and counter-insurgency warfare, for example, increasingly pay heed to ‘winning the peace’ as an operational requirement.\(^\text{11}\)

The AAA’s 2007 Commission on Engagement of Anthropology with US Security and Intelligence Communities, despite the name of their report, did not limit itself to U.S. organizations. To their credit, for example, they included international NGOs in both the report’s narrative and in its table called ‘Dimensions of Engagement with the Security Sector – Notes.’ However, the report pays scant attention to security research in the pursuit of peace.

While it is quite comprehensive concerning the ethical problems of using scientific research in conducting warfare-specific actions, it provides little guidance for a project such as the Security Needs Assessment Protocol or any other effort to design and plan peace or security initiatives that A) are not unilaterally determined by one state acting in (or on) another, or B) is in pursuit of scientific learning.

To wit, SNAP, and other related assessment or programme design processes, are operating under a UN mandate with the consent of the

relevant national government. That consent can take many forms, but one common one is a jointly signed document between the UN country teams and the government called the United Nations Development Assistance Framework (UNDAF). If research of any kind is conducted in support of the implementation of the UNDAF, it is – in principle – legal. Whether it is ethical, however, is a pending question, because the actual people being studied have not been consented. It is only through an abstracted notion of ‘representation’ that one can suggest that individual consent has been inferred. But this vein of argumentation strikes us as too thin as to be tenable, and still requires creative attention to situated ethical action among a given community, or with a particular person.

This concern is far-reaching because cultural research that is (or may someday be) used the pursuit of peacebuilding may take place under many different mandates and towards a variety of different ends, Examples include international or regional stability operations, UN peacekeeping operations, NGO peacebuilding initiatives, support for humanitarian operations, and others. In fact, cultural research could be used in any context in which knowledge of local cultural practices can inform the design of local action to advance an agenda for peace, reconciliation, relief, or recovery. It may also be used to support conflict prevention, crisis management, and post-conflict reconstruction. The list, indeed, goes on partly because these categories are mutable, evolving, and overlap.

In light of this new agenda for cultural research on designing peace and security solutions, we believe that the AAA’s report ultimately does not reconcile the two models of ethics we document here, but rather asks how far their own research ethic can be extended in the face of changing practices and the convergence of the two agendas – that of science and policy.

**Part IV: Towards a Model of Cooperative Ethics in Cultural Research on Peace and Security**

Cultural research to support civilian peace and security activities is on the rise, and is only going to increase in the years to come as it demonstrates utility for policymaking and project implementation. What is therefore needed is a system for thinking through ethical conduct in a manner that is designed to address the particular practices that characterize it. The Science Model was never designed to do this, as
it positions those studied as ‘subjects’ needing ‘protection’ rather than partners engaged in the mutual negotiation of meaning and action. Likewise, the Mandate Model is insufficient because it equates ethical conduct with legal conduct within a single social system that is not dependent on negotiated practices with other social systems.

Consequently, we have coined the term ‘Cooperative Ethics’ as the basis for a new model for cultural research (Miller and Scollon, 2007). The cooperative ethics approach is used because the two systems now available to guide SNAP’s approach are insufficient for the unique task of applying cultural research towards the end of peacebuilding or the creation of security services for communities.

The cooperative ethics model has two crucial features that differentiate it from other available ethical research codes. First, the beneficiary individuals and communities are recognized and positioned to be ‘agentive’ and hence are involved in establishing the meanings of engagements with community-external agencies and other stakeholders. As such, they are not ‘human subjects’ as implicit to most scientific models of ethics including that used by the American Anthropology Association.

Second, such engagements are understood to be – and must function as – multilateral negotiations for ethical common ground in the pursuit of situated action towards common goals. We fully recognize, however, that the pursuit of cooperative ethical encounters does not guarantee or necessitate that a mutually agreeable outcome will or even can be reached.

The differences between the three models can roughly be contrasted in the table below:

<table>
<thead>
<tr>
<th></th>
<th><strong>Science Model</strong></th>
<th><strong>Mandate Model</strong></th>
<th><strong>Cooperative Model</strong></th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Action focus of ethics</strong></td>
<td>Scientifically established truth</td>
<td>Compliance within legal parameters</td>
<td>Negotiation among the stakeholders of peace-building actions</td>
</tr>
<tr>
<td><strong>Loyalty</strong></td>
<td>Science community; individuals or groups under study</td>
<td>Mandating authority (e.g. the state, supra-state agencies)</td>
<td>Mandating authority; beneficiaries; inter-agency participants; national/international supporters &amp; hosts</td>
</tr>
</tbody>
</table>
Table (cont.)

<table>
<thead>
<tr>
<th></th>
<th>Science Model</th>
<th>Mandate Model</th>
<th>Cooperative Model</th>
</tr>
</thead>
<tbody>
<tr>
<td>Authority</td>
<td>Professional credentials; scientific principles; rules of evidence; informed consent of subjects</td>
<td>State or international law; chain of command; authorized position in the chain of command</td>
<td>Sponsor agencies; sponsor nations; research and design participation of local beneficiaries</td>
</tr>
<tr>
<td>Primary ethical sanctions</td>
<td>Censure by professional community</td>
<td>International/national law, employers, professional community</td>
<td>Withdrawal of sponsorship; resistance or non-compliance of local community members</td>
</tr>
<tr>
<td>Test of success</td>
<td>Reliability and validity of findings (i.e. support or approbation of professional community); no harm to subjects</td>
<td>Compliance with mandate</td>
<td>Acceptance and use of cooperative generated knowledge by and for the beneficiary community and other related stakeholders</td>
</tr>
<tr>
<td>Power relationships</td>
<td>Bilateral-asymmetric; scientific decisions and processes are unilateral</td>
<td>Unilateral, non-negotiable</td>
<td>Multilateral, negotiated</td>
</tr>
</tbody>
</table>

Diagram from Miller and Scollon, November 2007

The action focus of ethics refers to the practices a person is engaged in that have ethical ramifications. For anthropologists following the AAA Guidelines for Ethical Action, the action focus is on the creation of scientific truths through cultural research and generally among a group of people who are studied. In this case, the evaluation of ethical conduct is directed towards those practices that are intended to achieve that scientific purpose. This may refer to such practices as interviewing, surveying, leading conversations or workshops, observation techniques, participation in group activities, reporting on these

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12 At the SOAS conference in London (July 2008), it was suggested that a fourth column should be added called, for example, ‘local models.’ After some discussion, however, we decided not to do this. The three models presented here do not map static, extant systems that reside ‘in’ a community. Rather, they lay out three forms of interaction.
practices, or sharing information, among other normal activities for anthropologists or other cultural research professionals. For agencies exercising their mandates, the Mandate Model provides guidance on ethical action by making it synonymous with legal action under the rubric of the relevant mandate. For SNAP assessment teams, and others making use of the cooperative model, the action focus is negotiated with the given community in question towards the end of finding a modus operandi for the conduct of mutually acceptable practices.

The loyalty of the people engaged in research is always towards some actual and imagined community whose sanction is sought, or opprobrium is feared. For academics, the loyalty is contested between the need to find and articulate truths about the social world, but also towards the subjects of the study itself. For agencies working under a mandate, the loyalty is to the mandating authority, though this can create tensions for individuals who always come to that institutional decision as an act of some volition, which in turn means there may be other (personal) ethical views that – at some juncture – can be challenged by, or be made incommensurable with – that institutional loyalty. In the cooperative model, the background and embedded tensions evident in the other systems are instead foregrounded as the subject of attention and negotiation with the range of actors with a stake in the practice being pursued.

For each model, there is an authority for the determination of ethical action. Whereas loyalty refers to acts of volition for individuals, the authority refers to the supra-institutional reference point for determining whether a practice is ‘ethical’ and hence gives both guidance and coherence to the group that refers to it. For academics, that authority is constituted of professional credentials; scientific principles; rules of evidence; informed consent of subjects. For those carrying out mandated action, authority for action comes from state or international law; the chain of command; and one’s authorized position in the chain of command for making decisions and acting. And for those engaged in cooperative ethics, it is the complex and deliberately negotiated space among sponsor agencies; sponsor nations; and the research and design participation of local beneficiaries.

Primary ethical sanctions refer to the primary consequences faced by an individual for breaking ethical codes. For academics, the sanctions come from the professional community, in the form of loss of social standing and reputation and withholding or withdrawing support for that person’s participation in the group’s activities. For those carry-
ing out mandates, including state or international authorities, sanctions generally come from legal action for non-compliance with some aspect of the mandate and the rights, rules and obligations associated with the performance. It can also take the same form that it does for academics as with loss of reputation and withholding or withdrawing support from professional activities. Uniquely for those employing the cooperative model, sanctions come not only from funding bodies and – perhaps in time – from professional bodies that endorse cooperative ethics, but most importantly by non-compliance in the practices by the community members themselves. If they refuse to engage in the research activity, or deem the actions undertaken in the midst of the research to be unethical, sanctions will come from them in the form of non-compliance.

All research ventures are purposive, and so some criteria has to be used to deem the venture successful or not. For academics, those criteria are the reliability and validity of findings coupled with the admonition to ‘do no harm’ and, centrally, ensure consent. For mandated agencies, ethical success is determined by the fulfilment of the mandate, which was legally sanctioned by the authoritative body to whom the practitioners have loyalty. And in the cooperative ethics model, success is determined in two distinct ways. It is first determined by the acceptance and use of cooperatively generated knowledge by and for the community and other related stakeholders. It is also deemed successful if it can be shown that a constant application of the approach research model produces effective local results in repeated cases of different communities with different local conditions.

The final but important distinction between the three models comes from the exercise of power. For academics, the exercise of power is through a bilateral relationship with those being studied and the power differential is asymmetric with more power residing with the academics for whom the scientific process and practices are determined a priori within the context of their professions (in terms of methods, techniques for inquiry, publication, representation, and other matters).

Notably, a tension exists here with matters of consent and possible harm caused by the exercise of professional processes (such as disseminating knowledge that could threaten the studied individuals or communities), but this does not change the power relations, it only underscores the necessity of attending to the ethical implications of it in terms of research practices. For mandated action, power is entirely
unilateral and comes from the mandated agency whereby the group in question – even if consulted and engaged as partners in the process – is ultimately subjected to the mandate that was determined or agreed elsewhere and without consultation.

This does not, in and of itself, render the mandate ‘unethical’, but it does point to the real possibility that the authority and loyalty to the mandating authority (or the process that created the mandate) may not be shared among those being studied or engaged. A clear example in when people in the provinces do not recognize the authority of the national government, thereby rendering decisions made by the capital tenuous or even irrelevant on a pragmatic level. This brings about a very real concern for ethical conduct, for the safety and security of the research team (which may not have the permission of the studied group, despite having gone through official state channels), and for the likelihood of succeeding in the research itself. The cooperative model views the power relations as negotiated and shared among all stakeholders to the research process. This is not an ideological stance about power differentials but a practical response to an empirical reality.

The benefit of this model is that the group studied is no longer positioned as ‘human subjects’ needing ‘protection’ from the ‘harm’ that could be caused by the researchers. Rather, the group members ideally become partners in the creation of mutually beneficial knowledge for mutual (if different) gain.

The challenge is that such negotiations have to advance through community practices that could just as well be rapid as laboriously long, thereby creating financial and time pressures on a research team that have political costs and can also increase their exposure to risk. However, if the goal of a cultural research project is to help produce locally viable, situated programme designs – not merely to generate data and contribute to the debate – then the risks and costs must be well considered in light of the ultimate value of the entire endeavour.

The Cooperative Model takes it as given that these ethical questions cannot be answered independently of a dialogic process of learning how beneficiary communities or locales are socially organized, what practices they have for community decision-making, for representing those decisions to others, or in enforcing decisions once they have been made within the community.

Wherever and as soon as possible, it is important to initiate dialogue across the delivery/beneficiary (or international/local) fault lines as
these will inform the inter-agency and community dialogues that occur. Service design solutions by the implementing agencies or for other interventions should be brought into the beneficiary community before their implementation when possible. Their needs and knowledge of their own social practices should be brought into dialogue with the implementing agencies before the service prototypes are turned into blueprints and employed.

The Agenda in Wider Context

Every day, powerful international actors are exerting influence over matters of peace and security around the world. An agenda is now forming around applied cultural research in the service of greater peace and security. It is an agenda that was once richly conceptualized and advanced by earlier scholars such as George Simmel, Harold Lasswell and perhaps most importantly by Adda B. Bozeman. But it is also an agenda that has been lost and diverted. Today, it needs to be revitalized with both new tools from the academy that lend themselves to empirical, interpretive research, and by new ethical models that provide new footing for advancing this agenda.

At present, the AAA Code of Ethics does not provide sufficient grounds to conduct cooperative cultural research on peace and security that has the intention of changing the status quo among those studied. From our experience at the United Nations, these guidelines are not, cannot, and will not be followed by major implementing agencies at the UN, among NGOs, or by government agencies. But the work itself is beginning to happen and may eventually become part and parcel of the means by which projects, programmes, or operations are designed and planned.

We believe that merely positioning such research within the mandate model – where it now operates – is untenable because national governments cannot a priori determine what local communities will consider ethical behaviour, and ultimately the needs of individuals must be considered centrally in any valuation of ethical action and encounters. We also believe that development and humanitarian communities cannot possibly know how to “do no harm” in the absence of comparative cultural research into local practices and ethical systems for valuing them.
As work continues on the application of cultural research to the design of local action for peace and security, we hope that a clearer understanding of the competing models of ethical thinking will open new channels for engagement with the subject and eventually, we can jointly create more viable and valuable peace and security solutions on the basis of richer understandings of the different social systems that exist in the world.

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HIDDEN AGENDAS IN CONFLICT RESEARCH:
INFORMANTS’ INTERESTS AND RESEARCH OBJECTIVITY IN THE NIGER DELTA

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Introduction

Research on violent conflict is often considered perilous because information is difficult to come by. It is assumed that warlords and combatants are reluctant to share information because of fears of retribution by state or supra state war crimes tribunals or that such information might be intercepted and used by enemy forces. This is why one of the ethical considerations of social research in conflict settings is to protect the identities of informants, especially those likely to be linked to war crimes. The desire to protect the identity of the informant also stems from the assumption that informants are disinterested parties in social research. The researcher is then considered to be the active person out to extract information from the informant. This assumption occludes the agency and interests of informants; it overlooks the fact that in violent conflict informants have agendas they wish to promote. As this chapter shows, informants are often proactive in using the interview opportunity to state their own side of the story, which they will claim has been misrepresented by their opponents and powerful third parties. Informants are not just sources trying to cooperate to achieve the researcher’s objectives. They are also rational actors who want to use the researcher as their mouthpiece and advocate.

Apart from promoting the corporate interests of their communities, individual informants may also want to further their individual careers and roles. At different phases of the conflict, they may want to be invited to negotiation and reconciliation meetings, and to aspire to be in the corridors of power when conflict ends. Such informants not only want to be mentioned by name but often overstate their contributions to group mobilization and the war effort. Ambitious informants may insist on getting copies of research reports, which they will use to make leadership claims within their groups. In other words, the observant researcher will discover that his/her research becomes an arena
of struggle for preeminence by elites seeking recognition as community leaders and spokespersons. Given these aspirations, informants become keen to talk with researchers, especially those likely to give them greater publicity, for example by posting their reports on the World Wide Web. In African contexts, this gives 'outside' Western or African researchers attached to Western institutions more access than 'insiders'.

This chapter discusses methodological and ethical issues that arise from the self-interest of informants. It seeks to show that the objectivity of research in conflict settings is compromised not just by the researcher’s interests but by those of informants as well. The task of attaining objectivity in social research in conflict settings thus becomes herculean, because sources and narratives, such as local ethnic histories, are seldom objective. The chapter is based on the author’s doctoral research on violent conflict in two multi-ethnic cities in Nigeria’s Niger Delta.

The study

The objective of my PhD research was to find out why some multi-ethnic and divided societies manage to maintain peace amidst conflict while other such societies suffer intermittent and sporadic violence. The research was partly inspired by the recent interest in studying ethnic peace, an interest opposed to the prevalent perception that we live in a world in which violent identity-based conflict is ubiquitous. My research challenged the perception of a conflict-ridden world, suggesting instead that what needs more interrogation is ethnic peace. As Crawford Young puts it, ‘in most places the everyday lived experience of cultural pluralism is civil, often harmonious’, despite the fact that 90 per cent of the world’s nation-states are culturally plural (Young 1999, 2). Even in those countries with histories of violent conflict, it has been established that violence is often localized, not generalized. For instance, some studies of Burundi indicate that particular regions have been spared the carnage that the country has experienced over time (Lemarchand 2004). One of the major contributions of the new research direction is that it has encouraged comparative studies of so-called ‘violent’ and ‘non-violent’ societies.

It is against this background that my study adopted a comparative approach. I selected two cities (administrative divisions) in Nigeria’s Niger Delta region that are similar in terms of ethnic structure and
history of conflict, but different in the sense that while conflict in one case has been characterized by intermittent violence, conflict in the other has been consistently non-violent. These are Calabar and Warri. Calabar is located in the south-eastern part of Nigeria, almost bordering the Republic of Cameroon, while Warri is located in mid-western Nigeria. Both Calabar and Warri attained prominence as major ports for external trade and centres of early colonial administration and missionary activities. Calabar and Warri each have three ethnic groups that compete for preeminence, with every group laying claim to indigeneity. The ethnic groups in Calabar are the Efik, Efut and Qua; the groups in Warri are Ijaw, Itsekiri and Urhobo. The ethnic structure of Warri is, however, more complex because the Ijaw and Urhobo are also found in other divisions and/or states in the country. In Calabar, in contrast, the ethnic groups are all indigenous. Both cities have witnessed inter-ethnic conflict since the early colonial period. The bones of contention have remained the same: land ownership, the status of chieftaincies, and political representation. However, while Warri has been the site of several incidents of violent confrontation between persons purporting to defend the interests of their respective ethnic groups, Calabar has not witnessed any such incidents.

My research showed that the contrast in ethnic relations and levels of violence was linked to the absence of inclusive political institutions in Warri over the past century. In Warri, the evolution of inclusive political institutions was stymied by the persistence of horizontal inequalities in political, socio-economic and cultural status, which were mutually reinforcing. Horizontal inequalities were particularly explosive when the state and other powerful third parties took actions that were interpreted by the competing groups as intended to either consolidate or eliminate inequalities. This was particularly the case where historically dominant groups feared losing their dominance to hitherto disadvantaged groups. Horizontal inequalities were reinforced and legitimized by indigeneity, while homeland discourses were constructed to exclude some groups from the enjoyment of rights as bona fide members of Warri political community. The complex ethnic structure of Warri in which two ethnic groups in the Division were part of larger ethnic groups found in the other Divisions and States provided the context for the construction and legitimation of these homeland discourses. It also generated political anxiety among historically dominant groups which believed that their neighbours could draw on the support of their ethnic brethren to reverse historical advantages.
The different history of settlements in Calabar, which brought together groups without so-called ethnic brethren elsewhere, precluded the emergence of homeland discourses and encouraged the evolution of the politics of inclusion. The bone of contention in Calabar remains the relative share of each group, and not the total exclusion of any one group. Not surprisingly, instead of the exclusionary homeland discourses that emerged in Warri, Calabar has witnessed the emergence of a unifying discourse of brotherhood. The dominant group justifies its dominance on the grounds that it is the ‘senior’ brother in a tripartite brotherhood. Under the rules of primogeniture, the ‘senior’ brother cannot take everything. The practical implication of this rationality is that horizontal inequalities are more pronounced in the political sphere and less so in the socio-economic and cultural spheres. *Ipso facto*, therefore, those most affected are members of the political class. Since there are no indigeneity discourses, the non-elite members of the ethnic groups do not suffer discrimination. There is thus no deprived community in the true sense of the word, and this adversely affects the prospects for violent group mobilization. In Warri, in contrast, where horizontal inequalities are consistent, violent group mobilization has been eased by the fact that ordinary people share the fortunes and misfortunes of the elite.

The study also revealed the contributory roles of certain historical factors, such as the different patterns of slave and legitimate trades, missionary activities, indirect rule, multi-party politics and oil politics, which have made Warri more prone to outbreaks of violence conflict than Calabar. By showing how these historical factors influenced the behaviour of elites in both divisions, the thesis debunked the argument of earlier scholars and commentators who credited the political sophistication of Calabar elites for the relative peace there, and enjoined Warri elites, especially the historically dominant Itsekiri elite, to follow suit (See Falk 1933 and Ekeh 2004).

**Analytical approaches and sources of evidence**

The methods I employed for my research were largely determined by the nature of the phenomenon. As already indicated, the conflicts started way back in the late nineteenth century when the nascent colonial administration began to recognize the groups as distinct. The competing claims of chieftaincy, political representation and socio-economic...
and cultural marginalization that resulted, have persisted to date. This influenced the choice of both historical and social survey methods.

In fact, the historical method was not a matter of choice for this researcher. It was rather imposed by the social actors who take a historical perspective. Actors drew attention to how things had been in the past and how they had acquired their present shape. The historical narration is not only about changes but also about continuities, as can be seen for instance in the continuing discourses of horizontal inequalities, which are embedded in history. Contemporary attitudes, ambitions and actions are conditioned by historical memory. This memory is constantly being worked upon and reworked in the processes of ethnic mobilization. Current events are explained in the context of historical antecedents. Thus, if this researcher was to listen to his sources, he must pay attention to history.

The historical and contemporary salience of the phenomenon led me to adopt a three-pronged methodology. First, historical reconstruction methodology was used through archives, historical texts and interviews to reconstruct the histories of group formation and ethnic relations for the two case studies. Second, textual analysis of elite perceptions of inequalities was used to show how the historical processes are reflected in elite intellectual production. Third, mass perception surveys were used to identify the views of non-elites in order to see how these related to both historical processes and the views of elites.

My sources of evidence were therefore varied. I consulted the archives for the intelligence reports, the annual reports and files of correspondence between colonial officers to find out how colonial officers thought about their experiences. I also examined the writings of early travellers, traders and anthropologists, who documented their encounters with ‘the natives’. Other primary sources of evidence used in the research were semi-structured interviews, perception surveys, observation, position papers and memoranda of ethnic groups and ethnic organizations, government publications and newspapers. I also conducted interviews with leaders of ethnic organizations, traditional rulers, government officials, politicians, religious leaders, youth leaders and academics.

Since most of the interviewees were members of the elites, selected because of the positions they occupied or had occupied, supplementary perception surveys were conducted to garner the perceptions of the average member of the population. Both sources mattered because the thesis sought to explore whether the convergence of elite and mass
interests was necessary for ethnic violence to occur. Observation tech-
niques were particularly useful in the area of socio-economic inequal-
ities, since the location of public institutions and social amenities
commonly referred to as ‘government presence’ is regularly contested.
The researcher was alerted to this technique because many sources
enjoined him to ‘go round and see things for yourself’.

Methodological and ethical challenges

As I finally settled on the choice of topic, I wrestled with a number of
methodological and ethical considerations. The first was thrown-up
by the timing of my project, which coincided with the phase usually,
albeit erroneously, termed the post-conflict phase. In 2004, when I
started the study, the violence in Warri had ended and the issues which
threatened ethnic peace in Calabar were being proactively addressed by
the Cross River State Government. I was worried that the stakehold-
ers in the conflicts might withhold cooperation as they considered my
endeavours capable of reopening old wounds. I also feared that given
the bloody violence in Warri, many people, especially those who had
lost family members and friends and property, would find my research
emotionally challenging. This, I feared, could adversely impact on the
ongoing processes of reconciliation and healing.

Fortunately, however, a pre-study visit to Calabar and Warri showed
my fears and underlying assumptions to be unfounded. Contrary to
my expectations, I found that elites, mostly among the community
leaders and intellectuals, were not reticent to talk about the violent
conflict in Warri or about the events that brought Calabar inch-close
to the brink of precipice. I also learned from my informants that the
situation was not one of post-conflict, as conflict was ongoing; vio-
lence or the threat of violence had either ended or been suspended.
In fact, I felt encouraged by the views of my early informants that my
study was worthwhile. I also found that the persons I interacted with
in Warri welcomed every opportunity to advertise the indignities they
had allegedly suffered; they were not in a hurry to forget about the
violence, possibly because they were indirect victims of the violence,
though some might have lost properties and opportunities. My infor-
mants in Warri did not feel emotional about discussing the violent
conflict in Warri, especially when ‘remembering’ became politically
advantageous. Moreover, direct victims of state-inspired violence and
communal conflict did not shy away from speaking out either, even in emotional agony, since testifying was likely to enhance the prospects of attracting compensation.¹

Still, in order to avoid raising hopes of compensation or contributing in any other way to the emotional trauma of victims, I decided ab initio not to list the victims as part of my sources. Three considerations were critical to this decision. First, my objective was to document the processes that led to violent conflict, not the tragedies individuals had suffered. It seemed appropriate for me to concentrate on the processes of ethnic mobilization, the content of ethnic grievances, the nature of state and third party intervention, and their consequences for conflict pathways. Second, I felt somehow that the media and several human rights organizations (national and international) had already creditably documented the severity of human rights violations and human suffering in Warri. Thirdly, I felt I would attract hostile attention if I started asking questions about the violence and, inevitably, its perpetrators. It was safer for me to avoid probing specific incidences of violence.

Another major methodological-cum-ethical consideration I wrestled with was the possibility of maintaining objectivity and neutrality. Though distinct phenomena, I take them together because they are somewhat corollaries. Social scientists studying violent conflict are expected to be objective and neutral. Objectivity refers to reporting the facts ‘as they are’ and not being swayed by emotions and personal or group interests in evidence gathering, analysis and reporting. Neutrality refers to not being biased in favour of or against any of the parties in conflict, and to being fair to all parties. The researcher is consequently expected to consult and hear from all sides to the conflict and reflect their views and positions in his/her report. Ultimately, objectivity and neutrality require the researcher to avoid value judgments in the presentation of social facts. However, although this is considered a basic requirement in social science methodology, social scientists are increasingly agreeing that it is most improbable, if not impossible, to attain absolute objectivity and neutrality. There is much

¹ For instance, in 2000, several Ogoni women who were victims of rape by soldiers deployed to the area between 1994 and 1998 appeared before the Human Rights Violations Investigations Commission to tell their stories. Although they had their faces covered before the commission, set up by the newly elected Obasanjo administration, their identities were not hidden from community members who knew them.
debate on whether there is such a thing as absolute truth. Increasingly, the consensus view is that reality is context-specific: what a person perceives depends on where he/she is standing. Consequently, since each researcher is socially embedded in some identity, background and value system, the interests and values of his/her social group are deemed to influence his/her worldview.

Under these circumstances, the new wisdom is for researchers to declare their identity and biases from the inception of the research. An honest declaration is considered necessary to address the ethical concern that informants must not be left in the dark about the researcher’s identity. The approach is recommended, even though transparency can jaundice the attitude of informants towards the researcher. This is especially the case when the researcher is associated with or perceived as belonging to an opposing group. Since researchers belonging to the ethnic other may be unacceptable to informants, and may possibly be attacked as a reprisal for violence perpetrated by members of their own group, there is a growing tendency to prefer ‘outsiders’ to ‘insiders’ in conflict research. Where insiders are welcomed at all, they are often expected to study and report the story or version embraced by their own groups. Their materials and presentations are then synthesized by ‘outsiders’ to create a balanced picture. This was the approach adopted by the Academic Peace Works Associates in their study of the Warri Crisis. Three professors from Ijaw, Itsekiri and Urhobo ethnic groups were commissioned to produce a report on the causes of the violence as perceived by their own people. Their reports – reproduced as appendices – were then synthesized by ‘outsider’ editors, i.e., two Nigerian political scientists and an American peace activist married to a Nigerian man (Imobighe, Bassey and Asuni 2002).

This kind of approach, in my view, has a drawback in that it undermines the prospects of the research process aiding resolution, reconciliation and peace building. It makes researchers ethnic bigots who merely reinforce the views of their ethnic kith and kin. The conflict is easily framed in a zero-sum context which endangers the prospects of arriving at common understandings. Consequently, the possibility of collaborative research promoting dialogue between rival ethnic groups is lost. It is my considered opinion that the immersion of ‘insider’

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2 For a discussion of the positivist and hermeneutic approaches see Hermann 2001, 77–92.
researchers in the conflict in Warri, and the deployment of their intellectual capacities towards the prosecution of the conflict, seriously endangered the efforts at conflict resolution.

As an ‘outsider’, I initially reckoned that I would not have any difficulty in achieving objectivity and neutrality. My strategy was to emphasize my credentials as an Oxford student. When informants demanded to know my ethnic identity, I simply announced that I was Igbo; the Igbo not being party to the conflicts in Calabar and Warri. However, in the course of the study I came to realize that my ethnic group had been indirectly implicated in the conflicts, because Igbo are perceived as aspiring to domination. As one of Nigeria’s major ethnic groups, Igbo stand accused of dominating minority ethnic groups such as those found in Calabar and Warri. In the nationalist and early post-colonial eras, the National Council of Nigeria and Citizens (NCNC), the party associated with Igbo, was accused of playing divide and rule politics in both locations. Thus Igbo dominance in trade in Calabar, amidst competition for declining job opportunities, generated resentment among the Efik, the historically dominant group. Until today, some Efik elites still accuse Igbo of nurturing Qua and Efut nationalism.

Recognizing that this history could affect my acceptability among Efik informants, I decided to emphasize another aspect of my identity, namely that my mother and wife are Efik. Although ethnicity is determined through paternity in Nigeria, my maternal and conjugal affiliations helped to open doors with some Efik elites, who subsequently regarded me as an ‘insider’, some even called me ‘our son’. I did not have any reason to hide this aspect of my identity because it was evident that Qua and Efut informants did not expect it would affect my sense of judgment. After all, the high level of intermarriages among Efik, Efut and Qua meant that most people from the area were related in some ways to persons from other ethnic groups. In fact, I was surprised that one Efut chief also referred to me as ‘our son’ on learning that my mother was Efik.

Despite such endearments, I tried to maintain my status as an ‘outsider’ in both Calabar and Warri. I avoided familiarity with informants by, for instance, not disclosing my proficiency in the Efik language. I approached every informant in a formal manner, with a letter of request on my Student Associate Oxford Letter Head paper accompanied with a letter of introduction by my Oxford professor. Another strategy taken to maintain emotional detachment was my decision not to take up residence among any of the groups in conflict. This decision
proved useful in the case of Warri as I discovered that some ethnic elites were interested in knowing where I was lodging. In Warri, I lodged in a centrally located guest house, which also accommodated the Joint Military Task Force, deployed for keeping the peace.

However, since even ‘outsider’ researchers can be swayed by emotions and empathy for disadvantaged or endangered groups, I avoided getting involved in commentary on emotive issues such as land ownership, settlement patterns and the actual incidence of displacements and violence. This decision was taken to reduce the likelihood of ethnic leaders quoting arguments, statements, rationalizations and judgments out of context in order to lend credence to their biased positions. The discovery that ethnic leaders tend to cut and paste scholarly works and court judgments to attain their parochial interests had a sobering effect on me. It inclined me towards becoming modest about my expectations of the research. I felt it would be presumptuous on my part to think that any additional comment on emotive issues would make a difference in a context where the rival groups are still disputing rulings of the Privy Council and the Nigerian Supreme Court. Consequently, while reflecting the judgments and responses of various groups to them, I, unlike other scholars, refrained from making definitive or suggestive statements on who owns the land.3

My identity as an Oxford doctoral student was sufficient to remove all obstacles that my ethnic identity would have posed for the study. Even civil servants, who are usually reluctant to talk to researchers, were eager to interact and gave me a warm reception. I soon became suspicious, though. As informants enthusiastically cooperated with me and provided information, it became evident that such cooperation was not entirely altruistic. They were interested because they saw my research as another avenue for advertising their positions and prosecuting the conflict. As each informant told me that s/he awaited receiving a copy of my thesis, I began wondering what their intentions were. It troubled me when in the course of my examination of ethnic histories, pamphlets and memoranda to commissions of enquiry, I discovered that ethnic elites were desperate to adduce any shred of scientific evidence which supported the position of their own ethnic groups. The same ethnic elites who regarded supportive information

3 See, for instance, Imobighe 2003.
as gospel truth would also dispute any position contrary to their interests, notwithstanding the credibility of the researcher in question.

In sum, informants proved not so much interested in the objectivity and neutrality of the researcher as in the researcher adopting their stance and/or not adopting a position that might vindicate the opposing group. In other words, researchers must regard informants as political activists whose interests in research lie not in the unveiling of ‘uncomfortable truths’ but in the ascendancy of the particular ‘perspectives’ they cherish. In what follows I use my fieldwork experience to highlight some ways through which informants’ interests interfere with the pursuit of objectivity in conflict research.

**Hoarding information**

The practice of consulting several sources of information in order to attain objectivity and neutrality is hampered by the penchant informants have for hoarding information they consider prejudicial to their cause. In the course of fieldwork, some informants feigned ignorance or suddenly became evasive about particular events when the details of the information undermined their positions or claims. Instead of responding to questions, they queried me about the source of my information or dismissed the events as insignificant. I encountered this in attempts to ascertain views on the commission of enquiry, court cases and statistics on political appointment. Many informants either refused to be drawn into discussions or tactfully skipped ‘uncomfortable’ discussions.

I found this evasive attitude most frustrating when sourcing documents and publications. Given the poor state of documentation in public institutions in Nigeria, researchers often have to source documents and publications directly from stakeholders. I thus relied greatly on informants to provide me with documents I could not find in the Government Printer store and the Archives. I was surprised, however, to find out that even the most enthusiastic informant was selective in which documents were to be released to me. While they offered reports and rulings which favoured their group and/or undermined the position of rival groups, they withheld reports they considered unfavourable to their cause. A case in point was the report on chieftaincy classification in Calabar. A paramount chief who showed enormous support for my work and had supplied me with several documents claimed he did not have this particular document. When I
met another chief from his clan and enquired about the document, he directed me to the paramount chief whom he described as a repository of everything concerning Calabar. He was certain the paramount chief had the document because it had been cited in the memorandum to a recent commission of enquiry. The paramount chief was privileged to have most of the documents because his late father, who was also a paramount ruler, was one of the first educated persons from his ethnic group. When I contacted the paramount ruler again for the document he admitted he might have a copy and promised to look for it. I gave up after realizing that I had overstayed my welcome.

Another form of information hoarding was the disappearance of some documents and publications from government files, government printers’ stores, archives and libraries. I was not surprised when librarians, storekeepers and archivists claimed the materials I needed were not available. However, it was very frustrating to find that some sections of available intelligence reports were missing from the files of the Nigerian National Archives, without the archivists being able to account for them. I suspected that ethnic leaders had extracted the information they considered sensitive to ensure it would not fall into the ‘wrong hands’. In some cases, luckily, I later found the documents with the missing portions intact in the British National Archives at Kew Gardens, which told me why they had been removed from archives in Nigeria.

I also found that interested informants could obstruct access to published secondary materials. This was the case with the first published local history of the Itsekiri, one of the Warri ethnic groups. None of the Itsekiri leaders, including local ethnic historians, admitted they had a copy of the book. The reason for this became clearer when I found out that the book generated controversy when published in 1936. In an introduction to the second edition of the book, P.C. Lloyd, a British anthropologist, claimed the book was ‘discredited and in Warri, copies were destroyed or hidden by their owners so that very few are now in circulation’ (Moore 1970, v). The author was a controversial figure who led a royalist movement against the prevailing political establishment of the Indirect Rule regime, and whose version of ethnic history would have undermined the legitimacy of the status quo. The fact that my informants from the three ethnic groups claimed they did not have the book suggests that some aspects of its historical narrative remains controversial to protagonists and antagonists alike. For instance, while alleging that William Moore’s ‘spite for the Agbarha (Urhobo) was
legendary’, Ekeh used his narrative to substantiate the Ijaw-Urhobo claim of their prior settlement in Warri.\(^4\) By hoarding and/or destroying information, informants tried to undermine the researcher’s attempts to access all the information needed for an objective and balanced analysis.\(^5\)

_Posing as gate-keepers and sole authorities_

A related attitude I found troubling was when informants posed as gate-keepers, sole authorities or representatives of their communities. Some informants, chiefs especially, wanted either to be the only person I interacted with or to influence my selection of interviewees. I unwittingly fell into this trap in my bid to respect local tradition, which requires that one pays a courtesy call on the traditional ruler to acquaint him of one’s mission in his domain. This approach can be very useful in hierarchical societies when people look up to their chiefs and potentates. However, one runs into problems in societies where people are more independent-minded and there are intra-communal or inter-clan conflicts. In Calabar, some chiefs I met were explicit in directing me to persons whom I later found out were their loyalists; they also more generally indicated whom I should not talk to. This placed me in a disadvantaged position. Even though I was not bound to follow their advice, I felt it was expedient to do so in order to avoid possible sanctions. After I found myself in an embarrassing situation in Calabar, I did not wish to appear as an ingrate or as someone with ulterior motives.

The embarrassment was linked to my first contact with a clan head from one of the ethnic groups. Generous with his time, he supplied me with most of the documents and publications he had. He also told me that I had gotten everything I needed from his group and did not need to meet any other persons. However, I still desired meeting the

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\(^4\) See Ekeh (Ed.), 154. It is important to note that Ekeh, a renowned Nigerian political scientist from the Urhobo ethnic group, is the convenor of the Urhobo Historical Society. The group, which is made up of Urhobo elites, have supported research, conferences and publications aimed at countering the claims of the Itsekiri in the conflict over ownership of Warri. It also maintains a website- ‘www.waado.org’.

\(^5\) I later found a photocopy of the book in the personal library of a Port Harcourt based historian.
paramount ruler of this group. Unfortunately for me, while I was wait-
ing to see the paramount ruler in his palace, the clan chief turned
up and looked at me disapprovingly before going in to see the ruler. 
Shortly afterwards, the palace secretary emerged to tell me I had already
met with the person the paramount chief wanted me to speak with. I
felt small, really small, thinking I had betrayed the trust of a man who
had helped me so much. Later, when I had cause to return to the clan
chief a document he had given me to photocopy, I apologized and
explained that I had approached the paramount ruler because I needed
more sources. Surprisingly graceful, he told me what I least expected:
the cause of their ethnic group was not well served by chiefs granting
interviews and making contradictory statements based on ignorance
or selfish interest. This was why the paramount chief, who was old and
only half-literate, had appointed him to be spokesperson whenever
anyone wanted to know the group’s position. He advised me to make
do with whatever information he made available to me.

After this incident, I did not venture to interview any other person
from this ethnic group. Readers of my thesis without knowledge of
this background may criticize me for a lack of balance since I inter-
viewed only one person from the group as against several from the
competing groups. However, it is evident in this case that the apparent
lack of balance/objectivity was caused not by myself but by the para-
mount chief’s desire to present a unified, politically correct account of
the events under investigation. The clan chief in question was selected
to speak for the group because he was politically savvy, having served
as deputy chair of the local government council.

I also discovered that some chieftains were annoyed when they real-
ized I had spoken to other persons in their community, or to lead-
ers from rival groups, before coming to them. This affected rapport,
making such informants overly defensive and reactive. I consequently
decided never to refer again to any previous meetings and to pretend
to be hearing everything for the first time. Although this gave me the
opportunity to cross check information, I found it frustrating in terms
of time management when I just wanted to get some specific piece of
information. Besides, my decision not to refer to previous meetings
was subjected to testing when the informant specifically asked if I had
met with anyone else. I had to choose between telling a lie and risking
disapproval, and chose the latter as there were no ethical dilemmas
associated with it.
Another factor affecting objectivity in conflict research is that ambitious key informants tend to misrepresent issues to advance their own careers or interests. Misrepresentation comes in several guises. In some cases, the informant uses the research opportunity to advertise himself as the bona fide spokesperson of his/her group. Such projection is expected to enhance visibility and ipso facto patronage opportunities. This is usually done through either adopting non-existent traditional titles or exaggerating one’s importance, and mostly involves discrediting other contenders to the position. The intent is to make one visible for inclusion in peace committees and other political positions.

In Calabar, I came across an informant who paraded himself as the prime minister of his ethnic group, a position I was later told was non-existent. I then discovered that this informant, who was a son of a former traditional ruler, was positioning himself for appointment as the next paramount ruler even though the current ruler was from his own clan, which ruled him out given that the principle of rotation applies when traditional rulers are selected. The so-called prime minister’s version of the conflict aimed not just to undermine claims by rival ethnic groups, but also to emphasize the leading role of his family in the group’s struggle for self-determination. He understated the contributions of other families and clans.

Similarly, in Warri, I encountered a youth leader who boasted about holding the ace to the resolution of the conflict. He even bragged about leading the attack on a neighbouring group, and referred to instances where the state governor had pleaded with him and provided financial inducements so he would to arrange a truce. The only materials he gave me were those he had authored himself, mostly press statements that had widely circulated, even on the internet. He also dodged facilitating access to other youth leaders and elders of his ethnic group. Since he controlled most youth leaders, I knew it would be risky trying to contact others as he would get to know about it. This youth leader’s ambition robbed me of the opportunity to get a more nuanced picture. I was not surprised, however, when shortly after my departure from the field this youth leader was appointed to a special presidential committee for the development of the Niger Delta region. Later, though, he was disowned by the group he headed for going against the leadership’s decision that he should not participate in the presidential council.
Despite the noted drawbacks, elite ambitions are also useful to the researcher as they may reveal intra-community tensions. They may show that there is as much competition within groups as between them. The experience taught me to probe beyond the grandstanding of some elite informants.

Conclusion

This chapter has argued that the challenge of attaining objectivity in conflict research needs to consider the ways and manners in which respondents influence objectivity. Informants are not disinterested parties out to support research, rather they are persons (and groups) who desire to influence the outcome of research. The information they provide and do not provide, the sources they allow us to consult with and not consult with, and the materials they make available or withhold go a long way in influencing the facts we work with, and ultimately our conclusions. Conflict researchers who, because of justice and respect for human rights, must develop trust and emotional attachment with informants should nonetheless be careful not to underestimate the informants’ interests and deny their agency. This is to avoid producing a simplified version of the conflict, which is tantamount to a regurgitation of particular perspectives.

My study of ethnic conflicts in Calabar and Warri brought me face to face with methods informants use to manipulate conflict research. I see these same methods also at work in my current study of conflicts in the Niger Delta in which communities are mobilizing, and being mobilized, against state and multinational corporations. Like the ethnic elites in Calabar and Warri, Niger Delta elites have successfully portrayed the growing insurgency simply as a consequence of the exploitative distributional system of oil revenues within the Nigerian federation, poor environmental records and inappropriate corporate social responsibility programmes of oil companies. But the sheer volume of information and images that leaders present predisposes researchers to unwittingly adopt over-simplified versions of the causes of conflict. My current experience confirms that researchers must question whether objective research is actually possible in over-politicized environments.
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Numerous NGOs working in conflict-torn north-eastern Ghana have stressed the need for dialogue in peace-building but in my research I was rather confronted with a silence-generating legalistic culture. This chapter explores the relationship between truth, silence, rumour and violence as well as the epistemological and methodological challenges for conducting fieldwork in, and especially writing about, such a situation. Legalization produced so much silence, due to a widespread notion that conflict issues can only be addressed in a legal or quasi-legal setting and must be hushed up in daily life. Further, because, as we shall see, ethnographic texts have a prominent role as authoritative statements in such discourses, I had to contemplate my representation rather than my presence in the field. This chapter is about these considerations. But let us first illustrate the silence with the case of a soccer match.

**Football without tackles**

On 15 May 2005, I watched the Chamba Soccer Heroes play a friendly match against the Nanumba Nationals, the Bimbilla town team that successfully plays in Ghana’s second league. The Chamba team was made up of Konkomba players and that of Bimbilla town of Nanumba. That this friendly match was exceptional has to be understood against the background of three episodes of communal violence between members of the Konkomba and Nanumba ethnic groups in 1981, 1994 and 1995 in the Nanumba districts (congruent with the Nanumba Traditional Area of Nanun), in the Northern Region of Ghana. Each lasted for only a few days, but left thousands of civilians dead. These were the most intense outbursts of violence in Ghana, an otherwise relatively peaceful country enjoying civil rule. A hotchpotch of relief-providing non-governmental organisations (NGOs) brokered a successful peace accord between delegates of both groups in 1996 (cf Wienia 2009).
The Chamba football team had arranged a large yam truck to convey around forty players and supporters (myself included) to Bimbilla town (30 km to the east) for a nominal fee. In a cacophony of drumming, the truck left Chamba, a predominantly Konkomba town of some 7000 inhabitants. The journey went through rolling hills on which farmers amidst abundant yam mounts cheered and raised their hoes as the truck passed by. Halfway to Bimbilla, we crossed Dakpam with its still ruined chiefly palace, its crumbling colonial office from which a Nanumba sniper used to kill Konkomba attackers and the abandoned well which holds a Nanumba mass grave. But the truck drove on and the drumming continued. After Dakpam, we crossed another three villages with separate Konkomba and Nanumba quarters before reaching the environs of Bimbilla, the district and traditional area capital with over 20,000 inhabitants, where no Konkomba have permanently lived since the outbreak of hostilities in 1981.

When the truck reached its destination, the Bimbilla Secondary School football pitch, the Chamba supporters had become quiet. As the match started, Nanumba Nationals scored twice in three minutes, a lead they increased to a final score of seven goals to one. But the players shook hands and parted as if the match had ended in an insignificant draw. The Chamba boys walked to town to buy some snacks, and subsequently their truck left quietly in the night. The next morning the match was the talk of the town in Chamba. By-passers repeatedly stopped at the house of my assistant – the Soccer Heroes’ striker – and teased him with the score, saying “you are just a bush team!”

The point of this seemingly insignificant narrative is partly in what happened – Konkomba and Nanumba men voluntarily played a football match – but especially in what did not happen. Matches in Nanun, as elsewhere, seem to have the capacity to canalise all sorts of social strife and are characterised by biting insults and nasty tackles. Compared to all other matches I watched, that between Bimbilla and Chamba was surprisingly calm and devoid of any provocation. This calm is something I encountered not only in the football match; many informants continuously describe their ‘peace’ as calm or coolness (Konkomba nsudoon; Nanumba sodoo) and not as unity (Konkomba kimòkbaan; Nanumba nangbayinyini). This distinction is important because the main broker of the 1996 peace agreement precisely was suspicious of post-conflict calm. But why was there the need for a peace accord in the first place? In spite of its manifestations, the Konkomba/Nanumba violence is best not interpreted as age-old ‘tribal’ hatred. It was not
until the 1920s, during British colonialism, that Konkomba farmers from the Ghana/Togo borderlands settled in the fertile and sparsely populated Nanumba land hundred kilometres to the south. This was an unplanned family movement, but together they came in such great numbers that within decades, they outnumbered the autochthonous Nanumba perhaps two to one. This demography of Nanun has no equivalent elsewhere in Ghana.

It was, however, not until the 1970s, against the backdrop of ethnic emancipation movements, that Konkomba and Nanumba leaders started blaming each other’s backwardness and claiming the development they felt entitled to. The resulting contestation between Nanumba, who have claimed certain political and economic privileges as the autochthons in Nanun, and Konkomba, who in spite of accepting their settler status demand equal citizenship, exemplifies what is known in political anthropology as the ‘politics of belonging’ (e.g. Geschiere and Nymanjoh 2000). Over time, this controversy fed mutual theories of exploitation: Nanumba found their hospitality taken for granted because Konkomba flourished on their lands without paying respect to their leadership; Konkomba felt denigrated because they felt that they could live in Nanun only by the grace of oppressive Nanumba authorities.

Dialogue and silence in peace-building

Hizkias Assefa, a devout Christian from Ethiopia, was the director of the Kenyan Nairobi Peace Initiative and he has since mediated in various peace processes around the continent. He was invited by a consortium of NGOs to engage with efforts to secure a peaceful resolution to conflict in northern Ghana. In his most influential publication Peace and Reconciliation as a Paradigm (1993), which formed the basis of the peace workshops in Ghana, Assefa argued that the absence of violence is not the same as peace, because the people’s skewing of conflict issues is born from their “misguided perception that by avoiding conflict, it will go away” (Assefa 1996 [1993], 43–44). For Assefa, peace-building always required a transformative ‘reconciliation politics’ to construct “relationships full of energy and differences” (op. cit., 56–57).

This position was different from that of the peace interventions of the Government of Ghana, which comprised constitutional peace-keeping actions such as the declaration of a state of emergency and the
inauguration of a commission of inquiry. Assefa’s mediation defied that of the government but it was no less saturated with the dilemma of transitional justice that dominates current peace studies and that roughly divides scholarship: adherents of the ‘retributive justice’ approach hold that there can be no security let alone peace if perpetrators are at large (e.g. Borneman 2002), while those committed to a ‘restorative justice’ approach fear that prosecuting such perpetrators is counterproductive to peaceful coexistence and instead seek the justice in societal reconstruction through reconciliation and forgiveness (e.g. Lederach 1997, 2005).

Assefa acknowledged that “reconciliation without addressing the injustice in the situation is indeed a mockery and belittling the suffering of the victim,” but he also feared that accusations and counteraccusations would hinder post-conflict coexistence (1999; 2001, 182). He therefore emphasised the forging of unity and trust, which he found in traditions. In his interpretation, traditional rule or chieftaincy was “still intact and powerful” and “already accessible to citizens” in Ghana (1996, 58–59; 2001, 182). Assefa argued that a handful of ethnic leaders abused chieftaincy for political and economic games (2001, 169; cf. 1996, 53–54). He therefore looked for consensual dialogue among ‘voices of reason’ or ‘bridge-builders’, whom he selected as his workshop participants (2001, 168–169). Assefa hoped that the ‘voices of reason’ from the villages would influence urban elites. Unfortunately, Assefa failed to see that traditions had merged with customary law, which was the product of colonial and post-colonial political processes, and that traditions had never been ‘intact’, not even in the villages.

Why has Assefa’s peace resulted in the calm that it tried to solve; was he correct to interpret calm as conflict avoidance? Post-violence calm has received considerable scholarly attention. A first set of interpretations draws attention to pathology, notably post-traumatic stress disorder and unspeakable trauma (Daniel 1996, 143; Das 2007, 10, 59, 94; Hastrup 2003; Jackson 2002; Malkki 1995, 107–108; Scarry 1985). Another point of view focuses on coping mechanisms of silencing or amnesia (e.g. Borneman 2002, 295). Both strands may be insightful, but my research material drew me to a third approach, which Das (2007, 8) characterised as “withdrawing one’s voice to protect it”. Many people in Nanun spoke of “the lies on the streets”, ‘streets’ referring both to everyone and everywhere, and saw truth as something precious and protected by criteria of gender, age or profession and by location: palaces, court rooms or press conferences.
In an important volume on the anthropology of human rights, Wilson and Mitchell (2003, 5) argued that discourses of law and rights tend to “operate a particular regime of truth” and thereby produce “silences as well as generating and authorizing certain types of speech”. They showed that specific topics are prone to a language of rights between silence and authoritative speech, shaping vocabularies, occasions, speakers and audiences (cf. Hastrup 2003, 319). This theoretical approach is a suitable kick-off for thinking about the processes by which certain topics are anchored in legal formats and occasions deemed safe and constructive.

The dominant form of authoritative ‘speech’ in the Konkomba/Nanumba case was and is that of texts. A popular pidgin English adage in northern Ghana goes that “book no lie” and I found numerous people ‘preparing’ to write a book about the local culture and history. Although I found books to be most highly valued, reports (especially colonial) were also highly esteemed. Books on northern Ghana are often written by anthropologists and the most important anthropologist for north-eastern Ghana is David Tait, who worked in the Konkomba lands in the 1950s. On a trip to the Konkomba ‘homeland’ north of Nanun I experienced the authority of ‘Tait’s book’. Prior to an interview with the elders of Nalongni, one of the villages in which Tait conducted fieldwork, one of the elders refused to have an interview with me because Tait had written down everything there was to know about the village. I insisted on proceeding but the answer to my first question was: “See Tait’s book, page 9”! It is this anthropological representation that turned out to be more challenging in my research than my actual presence in a divided fieldwork setting. I will seek to illustrate this through my research role in a dispute between Konkomba and Nanumba.

_Silence and authoritative speech in Chamba (1996–2002)_

Assefa wanted to reinforce positive traditional cohesive values by “slowly infusing them with modern values of citizenship, participation, and equity”. This resulted in an accord in which certain Nanumba families were privileged to leading chieftaincy positions, while Konkomba would be allowed to freely choose their leaders in the lower levels of Nanumba chieftaincy, provided they had the consent of Nanumba chiefs. In this deal lies the biggest challenge for today’s peace in
Nanun: what if a freely chosen Konkomba chief is not acceptable to the Nanumba chiefs? This became very urgent in Chamba town, where Konkomba constitute the majority of the population, when the Konkomba community leader died in 1996 and the Nanumba chief asked the Konkomba community to choose a new leader to be officially installed as a chief. This case demonstrates how a conflict issue between Konkomba and Nanumba continuously defied dialogue and instead produced a silence-generating legalistic discourse.

Although the dispute emerged in 1996, the story of Chamba headmen actually started in 1979, when the Konkomba Youth Association (KOYA) told their local activists in Nanun to choose their own leaders. One Bijiba became the Konkomba headman for Chamba. Throughout 1980, the local Nanumba chief called Bijiba to step down, until the 1981 violence broke out. The Nanumba community in Chamba fled to Bimbilla and when some of them returned, they brought a new chief who feared the Konkomba in town and hence tolerated them.

When Bijiba died on 28 April 1996, his eldest son Nyilyar acted on his father’s behalf until the funeral was performed; his experience and young age made him popular. By then, there were no Nanumba in town, since all were displaced by the 1994–1995 violence. But when Nyilyar performed the funeral of his father two years later, some Nanumba had returned and there was also a new Nanumba chief (Salifu Alhassan). This chief announced to Nyilyar that he wanted to formally install a Konkomba leader as a medium between him and the Konkomba communities in town. Nyilyar said that he was not interested in such a title and that he wanted to become a farmer after the funeral was over.

Immediately after his father’s funeral, Nyilyar organised a council of elders from the main Konkomba communities in town and informed them about his father’s last words. Two other contestants hoped that their names would fall, but surprisingly, Bijiba’s last wish was that Konkomba leadership in Chamba be given to the family of the first Konkomba settler in Chamba (Fiindi). Fiindi had died, but his eldest son was present at the meeting and he said that he was grateful for the offer but also unprepared. He therefore asked Nyilyar to become the leader himself, so that he could learn from him. The council of elders supported Nyilyar’s candidacy and Nyilyar, who had preferred to be a farmer, agreed. The elders and Nyilyar visited the Chamba chief that day and he said that he recognized Nyilyar as the Konkomba elder.
However, the main contestant (Biligban) complained to the chief that a son could not succeed his father as chief. The chief agreed but he was also afraid to meddle in these Konkomba affairs and sent Biligban away to find an amicable solution. But the case was increasingly between the supporters of these candidates and tensions rose over that year to such an extent that in 1999, the District Security Council (DISEC) suspended the installation of a Konkomba chief in Chamba. The Chamba chief was under stress: Biligban followed all the procedures for becoming a sub-chief and paid him well with yam money, but denying the popular Konkomba support for Nyilyar, including that of Fiindi’s family, could be a dangerous decision for him personally and the small Nanumba community in Chamba.

There was a status quo for two years, until DISEC came to Chamba to push the contestants for finding an amicable solution in May 2001. Nyilyar organised a meeting of family elders at which, in the absence of Biligban, he was once again elected. Nyilyar presented himself to the chief who, overwhelmed by the sheer numbers of elders, accepted Nyilyar’s election. Biligban immediately called the help of a Konkomba lawyer based in southern Ghana and three days later, he summoned Nyilyar to the Nanumba Traditional Council (popularly called kòtini, court), since chieftaincy matters cannot be handled by civil courts.

Biligban was illiterate but in a statement of claim bearing his thumbprint, he claimed that Nyilyar’s election was illegal, untraditional and dangerous because it was done “without the approval and blessings of the Chamba Naa [chief]”.

After Nyilyar, who was also illiterate, gave a written statement of defence to the Council, written by his cousin, with thumbprints of most clan elders in Chamba, the Council installed a Judicial Committee of
three chiefs, all but one illiterate as well. The Committee never reached a conclusion, because its chairman died early 2002 and his death led to an escalation of a serious intra-Nanumba dispute.

The chief forced a breakthrough in the stalemate. In August 2002, heavily pressurised by the Traditional Council, he installed Biligban, while armoured cars patrolled Chamba and a military detachment had surrounded town for the chief’s safety. Nevertheless, a troop of Konkomba youngsters with sticks and guns headed for the chief’s palace. It was mainly because of the authority of the Youth Chairman that fighting was averted. That very week, this Youth Chairman, who was illiterate, reported the events to the Konkomba Youth Association (KOYA),6 which sent a delegation to Chamba to investigate the issue. After that, a KOYA peer gave a press conference, referring to clause two of the Kumasi Accord and interpreting it in the following way:

KOYA is against imposition of leaders of any kind at any level on our members. KOYA believes in democratic [tenets], and as such we cherish a situation whereby our members freely choose their leaders as Ghanaians.7

From here, the local case was rapidly brought to an ethnic level. On 16 September, KOYA wrote a memo on the Chamba issue, accusing the Nanumba Traditional Council of “divide and rule tactics” and of enforcing a chief on the Konkomba community by force.8 Three weeks later, the Nanumba Youth Association (NAYA) responded to KOYA, firmly rejecting the critique on the undemocratic Nanumba installation customs, also by adhering clause two, albeit through a different lens:9

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6 Yaw Waja Peter (General Secretary KOYA) to The Regional Minister (16-09-2002) ‘Simmering Tension between Two Konkomba Factions at Chamba in the Nanumba District’. The KOYA delegation was made up of the Saboba District Chief Executive Charles Bintin, the chief of Nnaalök village in Kikpakpaan (Unalorbôr Yamba Yajool) and several KOYA executives.

7 Wujangi, Kenneth (KOYA) (2002) ‘Speech of the National President of the KOYA at a Meeting of Konkomba Chiefs on Sunday 8th September, 2002 in Saboba’.

8 Beyi, Tagal (Youth Chairman) to The Regional Minister (31-08-2002) ‘Chamba Konkomba Community Situational Report’; On 20 September, the Chamba assembly member John Kidisil wrote to the DCE for intervention; NDA/P/28/Vol.2/21 Assemblyman John L. Kidisil to The District Chief Executive (20-09-2002) ‘Report from Chamba’.

9 Mohammed Yahaya A. (General Secretary NAYA) to The President KOYA (06-10-2002) ‘Protest against KOYA Activities in Nanung’.
Against this background it would indeed be very unfortunate for KOYA to interpret this to mean that Konkombas can just pick a leader bring him to a Nanumba traditional ruler and sort of command him to give his blessings without due regard to the normal and established channels or procedures Nanumbas equally go through to seek titles from our overloads [sic].

The Nanumba petitioners added that they would “not compromise with the novelty KOYA wants to introduce into the traditions and customs of Nanung” and embedded their legal position in a moral stance that their “hospitality is being taken for granted”.

The tone of both letters was inflammatory but a dramatic turn for the good came in November 2002 with a KOYA letter to the Regional Security Council (REGSEC), asking for a meeting between Konkomba and Nanumba Youth. As the latter agreed, the meeting took place on 17 December 2002 in the provincial capital Tamale.\(^\text{10}\) Although no decisions were taken that day, both parties were dedicated to finding a peaceful way out of the Chamba impasse. However, the tensions over the mentioned intra-Nanumba chieftaincy crisis overshadowed the case until early 2005, when a peace-building programme from the Catholic Relief Services (CRS) gave new impetus to resolving the dispute.

Towards a new escalation and authoritative speech (2002–2007)

Although CRS had been a prominent member of the NGO Consortium, its peace-building activities got new impetus in 2004 with the establishment of the Centre for Conflict Transformation and Peace Studies (CECOTAPS). In February 2005, CECOTAPS organised a meeting in Bimbilla for opinion leaders from all parts of society and asked them to list the biggest peace problems in the district. The Chamba chieftaincy dispute topped this list and was selected for peace-building workshops. In June, the local nun ‘Sister’ Melanie Amikiya and the director of CRS, Aidan Saabie Naah, organised separate meetings with the three main actors – the chief, Biligban and Nyilyar – in Chamba.

The CRS director infused these workshops with a paradigmatic approach, since he had received training in peace mediation from John-Paul Lederach. Aidan strongly believed in society’s self-organising

\(^{10}\) Abas, Charles (Regional Co-ordinating Director) to NAYA, KOYA (09-12-2002) ‘Summons to Meetings with REGSEC’.
mechanisms to transform conflicts to a peace culture. His treatment of
the Chamba case as a ‘stepping stone’ for Konkomba/Nanumba prob-
lems at large reflected Lederach’s paradigm that conflicts are ‘nested’
in wider problematic contexts (1997).

I was able to observe the meeting between CECOTAPS and Nyilyar
with twenty of his entourage. The workshop, which lasted for three
hours, took place in the mission garden. Nyilyar’s cousin, a former
District Assembly member, was asked to translate. Participation in the
workshop was voluntary but Nyilyar was offered “drinking money”.
The workshops were marked by improvisation, no records were taken
and both the organisers and the participants were nervous.

Between opening and closing prayers, at least four phases passed to
empower the participants to dialogue. First, upon Sister Melanie ask-
ing, Nyilyar explained that traditional problem solving requires that
elders “sit together”. Melanie thereupon replied that the chieftaincy
dispute had to be addressed in this way. Second, Sister Melanie argued
that the root causes of the conflict had to be addressed; she showed
a small tree and explained that if the roots were not healthy it would
not bear fruits. Third, after that, Sister Melanie took a spreadsheet and
asked the participants to identify the actors involved in the dispute and
then evaluate the relations between these actors. Worth noting is the
relationship with the chief, which was described as “very bad, because
he is corrupt”. Aidan asked the participants to think about changing
the relations that they had just evaluated. A participant wanted to start
greeting Biligban and chief supporters, and Nyilyar said that he was
prepared to meet the chief or Biligban. Aidan told the participants to
clap for these promises. Finally, Aidan emphasised that CECOTAPS
could only play a facilitating role but that the solution should come
from the participants themselves. He announced a joint follow up
workshop for the three parties and asked the participants to choose a
representation of ten people including three youth and two women.

The announced joint workshop of three factions made up of elders,
youth and women took place from 19–21 December 2005, on neutral
grounds, in Sunson village near Yendi. Aidan had deliberately chosen a
location with only one hall but the first night most participants refused
to sleep together and many took their mattresses to the veranda.

The next morning Aidan organised role-plays that led to the easing
of tensions. After lunch, Aidan asked each faction to desist from accu-
sations and rather to phrase their core fear. Nanumba felt threatened
as a minority while both Konkomba factions feared that Nanumba
wanted to divide and rule them. Aidan told the whole group not to be afraid. The outcome of this day was that the groups agreed to maintain the current status quo and exercise patience. That night, almost every participant allegedly slept inside the hall.

The following day, the two Konkomba groups agreed that they would “sit together” and find a solution, while the Nanumba delegation promised to completely resign from it and accept any outcome, provided it was mediated by the son of the first Konkomba in town. Aidan put the agreements on paper in a declaration, and under the auspices of the bishop of Yendi, the participants put their thumbprints under it. Having achieved this solution, the facilitators said that the time had now come to talk about development issues. Back in Chamba, the chief called the son of the Konkomba first-comer Nmabini to his palace and indeed delegated his sovereignty to him. A few days later Nmabini organised a meeting between Nyilyar and Biligban, but the latter did not accept Nmabini’s authority and refused to attend to the meeting. Momentum faded and in spite of repeatedly receiving the blame from the Chamba chief, Nmabini conceded.

However, this status quo was eclipsed by another outcome of the Sunson meeting, namely the CECOTAPS shift towards development issues in order to take away the seedbed for conflict. Although it was initially successful, this technocratic focus on development eventually boomeranged back to the chieftaincy dispute. In May 2006, namely, Sister Melanie invited women from the three disputing parties to Sunson to “prepare a brighter future for our children”. Sister Melanie found the women more cooperative than the men. They agreed to abstain from gossiping in the presence of their children and founded a Chamba women’s group. Back in Chamba, they marked their agreement with a parade and they thereupon set up a women’s group led by the local catechist.

The Chamba women’s group developed beyond expectation, enrolling more than one hundred women within the first six months. In the meetings, women shied away from issues of conflict and chieftaincy, but they did talk about the need for, e.g., a micro credit programme. It was, however, in one of those meetings that the seeds for a near-escalation of the Chamba dispute were sown. In the 21 October 2006

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meeting, a Konkomba woman raised the issue of the measurement of foodstuffs in Chamba market: traders monopolised the measuring of foodstuffs, whether for purchasing or for selling and they always took a share for themselves. Chamba women, who usually sold their husband’s produce in the market, Nanumba included, saw it as a kind of cheating and they called for the right to measure their own produce.

When the chief heard about this, he was enraged and he wrote a letter to DISEC, informing them about a Konkomba ‘by-law’ and announcing a three-week suspension of the market. DISEC responded to the letter by visiting Chamba and supporting the chief. It was surprising to witness how a case between farmers and traders became ethnic; although Nanumba women initially backed the uprising, they fell silent when their chief imposed a boycott. On Friday 27 October, Nanumba and Dagomba traders from Bimbilla and Tamale boycotted the market of Chamba and set up roadblocks to prevent traders from other parts of Ghana from attending the market. Most Konkomba refused the boycott.

A dramatic event further escalated the market situation in Chamba. On 7 December, on the eve of the first market day since the three-week boycott, the chief died. The next day, as the market was suspended, the corpse was buried in the palace and in the subsequent week, a series of funeral ceremonies were organised in the Chamba palace. The tensions resurfaced after the funeral and reached a boiling point ten days later. On Friday 22 December, as Chamba had its first significant market in almost two months, Nanumba traders returning from Chamba market spread the rumour in Bimbilla that they had seen weapons in that market. Within a day, rumours were buzzing around that Konkomba were preparing to step into the power vacuum of Chamba. In this altered form, these rumours spread back from Bimbilla to Chamba. So when two days later, on Christmas Eve, a shed burnt down coincidentally in Chamba, most Nanumba in town fled to Bimbilla overnight. The next morning, police enforcements were sent to Chamba, where Christmas was celebrated somewhat soberly.

While that very evening, Nanumba refugees started to return home, news of a potential new conflict between Konkomba and Nanumba spread like wildfire, making headlines in national media. The three

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12 Chamba Naa Salifu Alhassan to The DCE (23-10-2006) ‘Halting of Commercial Activities’.
weeks after Christmas were marked by a lot of meetings and activities, mostly outside of Chamba town. Again, as in 2002, there was a parallel development of local silence and national authoritative speech.

Immediately after Christmas, the Regional Security Committee of top military and police executives came to Bimbilla for an emergency meeting at the District Assembly hall. The tensions provoked such an active role of government representatives, because the national government did not want the upcoming ‘Golden Jubilee’ or Ghana’s 50th Independence Day to be overshadowed by ethnic conflict. Although the meeting was held in the District Assembly hall, there were very few local government representatives among the more than one hundred ‘chiefs and opinion leaders’. More than two hundred young Nanumba, and some Konkomba from Chamba too, followed the meeting through loudspeakers outside the hall.

The meeting was opened by the two Nanumba District Chief Executives. While Saeed (the Nanumba North DCE and a Nanumba) said that the District Security Committee was still investigating the rumours, Ogajah (the Nanumba South DCE and a Konkomba) stated that, after a ‘fact-finding mission’, he knew that the rumours were simply not true. Whereas the latter implicitly regarded rumour-mongering itself as a source of insecurity, Saeed seemed to take the rumours at face value, assuming that there is no smoke without fire, and described Chamba as the source of danger.

The Regional Minister then spoke for more than half an hour. He hardly spoke about Chamba but he made two related points about the links between development and security. Because money wasted on peace-keeping could otherwise be spent on development, opinion leaders should advise their communities to abstain from violence. Second, even though they had just had “a terrible Christmas”, “the soldiers are here for you”. He prayed that he would never have to use military force on the district but he would not hesitate to do so if people took the law in their own hands. In the ensuing open forum, the latent distinction between both DCEs was repeated in the words of a former (Konkomba) MP who asked the chiefs to send all rumour mongers to the security agencies, and in those of two NAYA peers who said that Chamba was “a very unsafe town”.

Outside the hall, there were similar thoughts about the origins of insecurity. However, Konkomba suspicions that Nanumba rumour-mongers wanted to provoke them and Nanumba fears that Konkomba
wanted to seize the land of Chamba were eclipsed by a mutual fear of an independent force of violence. Those good at counting saw that Nanun, which was on the eve of 2007, was bedevilled by a thirteen-year cycle of violence, after 1981 and 1994. Rumour was the main expression of this force. Although rumour is a very strong public expression in any case (Ellis 1989), in conflict situations it is often the only source of information (Robben and Nordstrom 1995, 15). De Boeck (2008) argued that rumours straddle publicity and privacy as the “awkward intimacy of a public secrecy” (cf. Das 2007), hence triggering the need to validate or falsify them. As news spread around Bimbilla or Chamba, my interlocutors always first asked the messenger: “Who told you?” It struck me that during the tensions, few Nanumba went to Chamba to see if the Konkomba there were indeed preparing for war, and those who did mistrusted the town’s normality; few Konkomba went to Bimbilla to say that they were not.

Many people I spoke to were disappointed in the Regional Minister who had only tried to empower the heavily divided chiefs and elders, without addressing the Chamba issue. There was, however, one group of people who felt responsible for averting further tensions: students, who tried to stop this silence and mistrust. Because it was Christmas break, most Konkomba and Nanumba college students were with their families. During college breaks, students usually engage in voluntary clean up exercises and health education programmes. While the Konkomba-Bassari Students Union (KOBASU) holds its annual congress during the summer break, the Nanumba Students Union has its congress (‘the homecoming week’) during Christmas break. During this week, students organise sports matches, quizzes, movie shows and a durbar for educated opinion leaders.

Responding to the tensions, NASU leaders contacted KOBASU students from Chamba to play a football match with them in Chamba to give a message of peace, but the Police Superintendent forbade the match from being played, because he feared it would exacerbate tensions. Despite this disappointment, NASU and KOBASU leaders agreed to meet each other on 29 December in Bimbilla to discuss their possible role in reducing the tensions. At a closed-door meeting (to which I was allowed), the students openly spoke (in English) about their fears of another conflict (“If I hear a gunshot I will shit in my pants”) and their doubts about the position they were in. They felt a great responsibility to unite NASU and KOBASU in a Nanun Students
Union. In fact, these students came under serious stress and many of them were to arrive back on campus two weeks late. This activism of students was a clear sign of the loss of moral authority of the Youth Associations, of which students were junior members. NASU stimulated the KOBASU leaders to organise a conference in Chamba to convince everyone that this town was safe.

But before that, on 30 December, Nanumba students held their annual homecoming durbar, which had the surprisingly topical theme ‘Peaceful coexistence: a tool for quality education’. The meeting was more informal than the REGSEC meeting and the whole programme was in English. After an hour of Nanumba dancing performances, various local administrators and NGO executives addressed the audience of students, challenging them to tolerate each other and to end the rumours. On 3 January, KOBASU organised a peace meeting in Chamba, with hundreds of attendants, from family elders and students to young women. Invited speakers were mostly the same as in the previous Bimbilla meeting. The KOBASU president gave a speech in which he said that Konkomba had been misrepresented in the REGSEC meeting. He summed up the Konkomba grievances in Chamba, which included cheating on Chamba market. He also stated that Nanumba should keep their hands out of the installation of a Konkomba sub-chief. On 7 January, NASU reacted to the KOBASU paper, asking for District Assembly resolution of the market issue, but reacting to the chieftaincy issue that “some Konkombas have failed to give the Nanumba chief the due recognition”.

Despite these meetings, police at a Bimbilla roadblock intercepted guns and ammunition in a Konkomba truck on 9 January, after which the District Chief Executive told the national press that the war had already started, causing the small Konkomba community in Bimbilla to flee. The increasing tensions led NAYA and KOYA executives to come from Accra to Bimbilla. On 14 January, they held a public meeting together with the local and national politicians and administrators who had attended to the students meetings. Although Christmas break was over, most student leaders were still in Nanun and attended

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14 ‘Speech Delivered by the Nanumba Students Union and the Concerned Citizens Association of Nanung on the Occasion of a Peace Forum for the People of Nanung Held on Sunday 7th January 2007 at the Bimbilla Secondary School Grounds’.
this meeting. While state representatives warned the youth leaders that Government would not allow conflict to start, the latter replied that they were only interested in the development of their people. After the public meeting, the speakers held a closed-door meeting in a Bimbilla restaurant where the local administration provided the youth leaders with a vehicle to go round Nanun and preach peace. When this peace educating tour actually took place on 28 January, tensions had almost completely subsided. Nanun was ‘calm’ again, a calm that I have found difficult to evaluate.

Assefa was probably correct when he argued that peace should exceed the absence of violence. In this case, calm is not the opposite of conflict but one its manifestations. Considering the violent past of Nanun, issues of contention are not just talked about ‘on the streets’, but they take different forms: silence, rumour or authoritative speech or petitioning. Assefa hoped to build unity among the communities partly by bypassing apparently divisive youth leaders. While this approach was successful in the 1995–1996 peace workshops, Assefa overestimated the solidarity of the masses. Konkomba and Nanumba ethnic emancipation movements not only produced external inter-ethnic enmities but also generated internal moral contentions, e.g. between chiefs, youth, students and women (cf. Lonsdale 1992). It is precisely this ‘modernising’ context of uncertainty in which the calm but insecure ‘peace’ of Nanun has to be studied; a ‘peace’ in which football players do not dare to tackle. Nanun is probably getting calmer and less secure. But whether such a peace cannot last is another question.

Witnessing and testifying

The above case study clearly showed how a repertoire of issues produces silences, rumours and conspiracy theories and almost violence, and how it involved various ‘opinion leaders’ along the way. First, Appadurai (1998) has shown how violence can be a mechanism of recreating certainty in times of rumour and conspiracy. Fortunately, no such violence erupted in 2002 or 2007. Second, I found that most of my interlocutors believed in a truth beyond contingencies, or as one of them aptly stated: “The truth is the truth and lies are lies”. However, as seen above, “the lies are on the street” and truth is confined to specific mouths, ears and occasions. But I also found that authoritative speech is often contested between and among elders, youth, chiefs,
politicians et cetera. Information can be simultaneously evaluated as truth, rumour or conspiracy, depending on who gives the information. While there was no violent clarification of the rumours, the rumours did produce a silence-generating legal discourse centring on authoritative statements.

The obsession with texts and written evidence was epistemologically interesting but also methodologically challenging. I was worried that my interlocutors would try to ‘seduce’ me to consensus (Robben 1995), that they would interpret my encouraging ‘silent’ and ‘uh-huh’ probes (Bernard 1995, 215, 217) as consent, and that my pursuit of research neutrality was subverted by my empathy towards the people I worked with (Sluka and Robben 2007, 22–23; Nordstrom and Robben 1995; Scheper Hughes 1992). While this dilemma may seem to be a point of departure for contemplating ethical codes for ethnographic fieldwork, my fieldwork experience defied clear-cut guidelines, because the tension between the pursuit of scientific objectivity and empathy towards the people in the field lies at the very basis of ethnographic methodology (Meskell and Pels 1999; Pels 1999). As far as I could register, these were needless worries. Expecting my neutrality to be compromised during such events, I was surprised to find it actually imposed on me. Whenever there were tensions, my interlocutors invited me to ask all the parties involved about their perspectives, so that my book would be better.

But when my interlocutors said “I can’t wait to read your book”, this flattery engendered doubts in me about my expertise (Ferguson 1999; cf. Clifford and Marcus 1986), disappointment about my representation of the complexities I sought to describe (Das 2007, 2; Robben and Nordstrom 1995, 15) and nervousness about what local leadership would use my findings for (Brettell 1993; Caplan 2003, 23 ff.; Scheper-Hughes 2000). While most people I worked with thought that my research would reveal ‘the facts’, and coupled my witnessing to their testifying, my work was about dismantling such ontological aspirations and trying to represent the ways in which facts fractured. Eltringham’s work on the methodological and ethical challenges of representing the Rwandan genocide helped me with this approach. He proposed to give voice to confusion rather than a sanitised meta-narrative:

Conflict is ultimately about disagreement. To properly understand conflict we must give voice to these disagreements and demonstrate how they are articulated. From such a perspective, disagreements about the nature and ‘truth’ of conflicts have less to do with the sanitised, objective
and inevitable progression of ‘facts’, than they do with an informed engagement with the confused and confusing words of discursive strategies, partial ‘truths’ and conflicting subjectivities (Eltringham 2003, 109).

How to represent confusion without being confused yourself? Like Ferguson in a Copperbelt in crisis, I found that my interlocutors’ messy answers inflicted confusion on me, resulting in “a situation where ‘the natives’ as well as the ethnographer lack a good understanding of what is going on around them” (Ferguson 1999, 19).

Analyses of the events I witnessed should not therefore be mistaken for testimonies. My epistemological, methodological and ethical position merged in an attempt to give voice to the inherent disagreements that I recorded. But what if empathy and objectivity coalesce in the interlocutors’ expectations? People in the field came to know me as that man who wanted to learn the culture and history of Nanun and who, to that end, went round listening to all sides. Many of them expected me to postpone my testimony to my ‘book’.

Conclusion

The growth of local readership promises to become an increasingly topical research theme, which requires the analysis of local expectations and the researcher’s choices and doubts rather than ethical codification. In this chapter I have tried to demonstrate how in post-violence Konkomba/Nanumba coexistence, dialogue about conflict issues was continuously subverted by a communication strategy of silence and diverted to authoritative speech. We have seen these strategies in both the 2002 and 2006/2007 tensions in the Chamba dispute. I had hoped to listen in on local dialogue but the prevailing legalistic discourse and the pervasive silence that was its corollary combined to render my research position problematic. The great challenge was actually how to live up to my interlocutors’ expectations of my future ‘book’ which they hoped would reveal the truth.

Like Ferguson, I found it difficult to make authoritative statements about such a complex fieldwork setting and, like Eltringham, I have tried to give voice to the multitude of legalistic processes, such as authoritative speech, rather than trying to produce a sanitized truth as an authority myself. This was as much an epistemological and methodological as an ethical choice. However, as I showed, this choice was imbued with a fear that I would disappoint my interlocutors. Whether or not their expectation will turn into disappointment is yet to be seen.
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