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<ct>CONFLICTS AND COMPROMISES? EXPERIENCES OF DOING ANTHROPOLOGY AT THE INTERFACE OF PUBLIC POLICY

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<t1>Over the last 30 years, we have worked in a wide variety of contexts connected with aspects of health, international aid, humanitarian relief and criminal justice, mostly in Africa and the United Kingdom. We have never viewed ourselves as 'applied anthropologists',[1] let alone 'applied qualitative researchers'. We have tried to position ourselves as academic anthropologists who happen to work on certain kinds of issues that require collaboration with international and sometimes national institutions involved in aspects of public policy. Here we comment on issues that can arise in this kind of anthropological research, focusing in particular on dilemmas we have confronted ourselves.

<t>We have undertaken anthropological research in and on policy-related issues from the outset of our careers. Melissa Parker wrote a PhD and published a series of papers that challenged some of the underlying premises informing policy aimed at the control of a tropical disease (Parker 1992, 1993). Her research involved living in villages in the Gezira/Managil irrigation scheme, Sudan for several years. Proficient in Arabic, she investigated the social, behavioural and biological dimensions of infection with *Schistosoma mansoni* among women and children. Support was provided to her by staff from the disease control programme, as well and the Sudanese Ministry of Health, including assistance with the screening of large numbers of adults and children for parasitic infections. In addition, transport and medical supplies were provided by UNICEF, Oxfam, the Japanese government and the UK's ODA (Overseas Development Assistance, now called the Department for International Development).

<t>Tim Allen's route into anthropology was through an aid agency. He had lived in Sudan for several years in the early 1980s, teaching English and History in schools and at Juba University, when Norwegian Church Aid (NCA) offered him support to live in Acholi and Madi villages for a year. He occasionally had to write reports and give talks about the effects of the NCA projects on the local population, and eventually published some of the findings (e.g. Allen 1987). Later, when carrying out PhD fieldwork in Uganda, he had a similar relationship with Médecins Sans Frontières, Lutheran World Federation and the office of the European Union. This provided him with transport and logistical support when needed, and allowed him to study programmes aimed at assisting refugees and returning refugees in northern Uganda from 1987 to 1991 (Allen 1988, 1994, 1996; Allen and Morsink, 1994).

<t>In subsequent years both of us have worked with international agencies and other kinds of institutions involved in public policy. Parker, for example, carried out long-term research in the UK on sexual networks in London, linked to a clinic at a major teaching hospital, while Allen carried out research in Botswana, Uganda and Sudan with funding and support from international agencies including the UK's Department for International Development (DfID), DANIDA (the official Danish development agency), World Vision, Save the Children, USAID (the official US aid agency), UNICEF (the UN agency for children), UNRISD (the UN Research Institute or Social Development) and UNDP (the UN Development Programme). Both of us have also been involved in public heath-related research in East Africa, with support from the WHO (World Health Organization), the Bill and Melinda Gates Foundation and various specialist institutions (such as the Kenyan Medical Research Institute and the Tanzanian National Institute for Medical Research).

<t>Much of our fieldwork would have been impossible without funding and logistical support from organizations involved in public policy, even though we have occasionally been very critical of their activities. Contrary to views that have been voiced by some anthropologists about such collaborations, we have usually been able do our research without any overt attempts to control findings, or to restrict what we have included in publications. That is not to say that there have never been tensions, but when confrontations have arisen, we have managed to weather the storm. Over the years, we have found ways to deal with difficulties before they arise, while also appreciating that there are times when taking the flak can be a helpful role to play.

<h1>Concerns about anthropology and public policy

<t1>Problems associated with the relationship between anthropology and public policy are well known. Historically, there was a great deal of soul-searching within the discipline about the 'colonial encounter'. It was argued that the constraints placed on anthropologists, the kinds of questions they asked and the kinds of insights they offered were all too readily affected by the ways in which they reached the field. Evans-Pritchard even dedicated his great book on Azande witchcraft to Major Larken, the British district official who befriended him and assisted him (Evans-Pritchard 1937). His equally famous work on the Nuer was in large part a response to the Anglo-Egyptian administration's need to know how to pacify them, following a series of violent incidents (Evans-Pritchard 1940). However, if arguments about co-option apply to the past, they apply at least as strongly today. It was international aid agencies that enabled one of us (Allen) to spend time among the Azande in 2006 and among the Nuer in 2009.

<t>In both cases it was far from straightforward to reach the locations concerned. After arriving in southern Sudan, it was necessary to take two flights on United Nation's planes and then hire a county commissioner's motor boat, at considerable expense, to reach the Nuer population around Fangak. Three years earlier, reaching Zandeland so soon after the signing of a peace agreement in Sudan had proved even more difficult. There had been local clashes between Zande and Dinka soldiers in the Sudan People's Liberation Army, and a rebel force from Uganda, the Lord's Resistance Army (LRA), was based along the nearby border with the Democratic Republic of Congo (DRC). In order to access the region, Allen agreed to carry out research and write a report on HIV/AIDS for World Vision, a Christian-linked international non-governmental

organization (NGO). It is an agency that Allen had taken issue with on a number of occasions, notably over exaggerated estimates of HIV/AIDS rates in northern Uganda as a consequence of LRA activities (Allen 2006a). However, World Vision, unlike many other international NGOs, often works far away from the bigger towns, and sometimes where there is little or no other international presence. It was the only agency working in the places Allen wanted to visit. Equally important, Word Vision was Allen's insurance. The agency would have done everything possible to evacuate him, along with its own staff, if the need had arisen. To arrive at the border area required security clearance from the newly established government of southern Sudan and, starting in Kenya, the journey involved four flights on small planes spread out over several days, followed by an eight-hour car journey. Fieldwork was then facilitated with the use of a motorbike. All of this, as well as accommodation and food, was provided by World Vision.

<t>In these circumstances, supporting agencies can be understandably sensitive about being criticized in published articles and books. Very often, there are pressures placed on the anthropologist to avoid such confrontations, while she or he may also feel appreciative of the care provided by well-meaning aid workers in the field, and feel considerable loyalty towards them. In short, there are problems in 'biting the hand that feeds you' too hard and judging how much rigorous analysis is appropriate is not always straightforward.

<t>Commenting on discussions at the 1983 Association of Social Anthropologists (ASA) Conference, part of which focused on social anthropology and development policy, Michael Redclift observed that attitudes among anthropologists had changed since the ASA Conference in 1973. In the early 1970s, he observed, the prevailing attitude

seemed to be, 'we will only join the development fraternity on our own terms'. Ten years later, he felt that view has been set aside: 'Today, the employment crisis among anthropologists … has triggered a different response. The call seems to be to join the development brigade at almost any cost.' In his view, there were real dangers that this would damage the discipline, as research questions and the weighting of evidence were framed by people with agendas that had little to do with anthropological understandings. 'Must the anthropologist compromise', he asked, 'only to *be* compromised?' (Redclift 1985: 202).

<t>At the time, many academic anthropologists took the view that the answer was 'Yes'. Indeed, both of us have experienced some scepticism over the years from colleagues that it is possible to do credible anthropological research with funding or support from parties with potentially ulterior interests in ascertaining the 'right' kind of results. Allen, for example, was taken aside by a senior anthropologist at Oxford while writing his PhD in the 1980s and told that now he was doing academic anthropology, he should stop working with aid agencies. The strong implication was that it brought social anthropology into disrepute. Similar views were still being expressed by some of his colleagues at the London School of Economics (LSE) several years later.

<t>The expansion of work for anthropologists in policy arenas, particularly in the health sector and international development, and the fact that many anthropology graduates seek work in these areas after they leave university, has not meant that the kind of worries Redclift expressed have abated. On the contrary, they have been elaborated and discussed in more detail. Academic anthropologists like Escobar (1995) and Ferguson (1990), for example, have provided popular critiques of the whole international

development enterprise; and some of those who have worked on policy issues have built careers by explaining the ways in which constraints are imposed and how that makes serious anthropological study largely untenable. One of the best known anthropologists to have espoused the latter perspective is David Mosse. He worked closely with aid organizations for more than a dozen years, but came to the conclusion that putatively scholarly work is almost inevitably co-opted when it is linked to consultancy, or faces a conflict of interest.

<t>Mosse (2004) wrote a book about his work as a social development consultant on a project in India that had been funded by DfID, and found himself in trouble:

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… there was consternation from my consultant colleagues and managers, who felt that I had departed from the received view of the project as an overwhelming success. The point was not that my book… rejected the view of success as being the outcome of good policy and technical design. Instead, it drew attention to the contradictions and compromises of practice … (Mosse 2006)

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<t1>In early 2004, objections were made to Mosse's publisher, to his university research ethics committee, to the academic head of the university, as well as to the Association of Social Anthropologists, 'on the grounds that the book was unfair, biased, contained statements that were defamatory, and would seriously damage the professional reputation of individuals and institutions.' In the event, his university, the ASA and the publisher were either not persuaded or decided that they were not in a position to adjudicate on the

matter. As a result, Mosse was not obliged to make any changes to his text. The case has become well known, at least in British anthropology, largely because of the interesting way Mosse has written about it. To many, it confirms the impossibility of doing proper social anthropology with international development agencies – a perspective Mosse himself has come to largely share.

<t>We are not so pessimistic. It is certainly the case that all kinds of pressures are brought to bear on anthropologists working with international agencies to interpret things in certain ways, and to avoid commenting on certain issues. Indeed, we have confronted the kinds of direct pressure placed on Mosse. In our experience they are not so unusual. But are these pressures actually any worse than constraints anthropologists experience when they work with local or national institutions, or when they discover sensitive things about the people they live with? Anthropologists are always confronting the dilemma of how much to reveal, and what kinds of information might be harmful to informants. It is a problem arising from the intimacies that are an inherent part of our methodology. Anthropologists find out uncomfortable truths, including things that might be considered confidential or even secret. To invert a Swahili proverb: not all secrets are for sharing.

<t>It is also important to note that while some DfID employees may have felt betrayed by Mosse's book, senior staff we have spoken to had not heard about the case until we discussed it with them. Their view was that to oppose publication in the manner attempted could only have been counterproductive. They asked questions about who had actually raised objections within DfID, because it had never come up for internal discussion as far as they were aware. It is possible that they were not telling the truth, but

they did seem genuinely surprised by our account of what happened. In our experience, the view inside DfID is that critical research on development issues, some of which is directly funded by DfID on a 'hands off' basis in university departments, raises the bar on the quality of UK aid. Perhaps that point is debatable, but what is interesting is that institutionally the idea of trying to muzzle critical analysis is viewed as inappropriate. It should also be mentioned that many staff within DfID have themselves taken degrees in social anthropology, including the current director general for policy and global issues.

<t>Overall, we have found, as Mosse himself did in the end, that when overt pressures are applied, it is possible to deal with them. In addition, we have discovered that writing as honestly as possible about what we have observed, and backing up critical comments with robust evidence, is respected, even if it is not liked. Years ago the anthropologist Barbara Harrell-Bond told Allen to accept support from anyone if they allow you to do research, but never agree not to publish what you find out. It was good advice. There are, as Mosse notes, clauses in contracts about funding institutions having intellectual property rights. But they are vague, and very difficult to implement. In most instances we have found it is possible to strike out those sections, or to make an arrangement whereby material is leased to the donors, but copyright is retained by us. There have been the occasional unpleasant moments, but whenever efforts have been made to use a contract or any other measure to restrict what we write, we have always been able to resist if we have decided it is the right thing to do. We have never been forced to remain silent, even if we have sometimes decided to bide our time before releasing our findings. In the following sections we provide a few specific examples of

the kind of work we have been doing, mainly related to health issues, and comment on how conflicts and compromises have been worked out.

<h1>Divergent approaches to evidence

<t1>One of the most important issues confronting anthropologists working at the interface of public policy relates to divergent disciplinary understandings of evidence, both in terms of what constitutes reliable information and the way in which dissemination occurs. Many policy-orientated research projects tend to emphasize 'scientific' methods, foregrounding quantitative approaches that appear to replicate natural science research. This is a reason why economics is often viewed as the most important social science discipline. It also means that studies published by the World Bank, for example, will often refer to econometric papers, or will have an appendix describing regressions used in the analysis. Often policy professionals themselves do not fully comprehend the methods used in these contributions, but they are perceived as lending credibility to the results and conclusions. More generally, there is a prevalent view among policy makers that evidence means data, and that data means numbers.

<t>In this context, social anthropologists can find themselves at a disadvantage. Their findings generally lack a quantitative dimension, and can be set aside as interesting but anecdotal. For this reason, it is important for those working on public policy to equip themselves with a basic understanding of quantitative techniques, so that it is possible to discern the strengths and weaknesses of what they are reading. Often such a familiarity with quantitative methods, and avoidance of defensive dismissals of numerical data, opens up useful alliances. There are many academic economists, demographers,

epidemiologists and clinical scientists who are just as appalled as anthropologists by the quality of analysis that appears in policy documents. It is helpful to be able to talk to them in a language they can comprehend.

<t>In the health sector, anthropologists work alongside colleagues from a variety of disciplinary backgrounds in both the biological and social sciences; and they often find themselves cast as 'the handmaiden of biomedicine'. In these circumstances, research protocols may preclude open-ended investigations. Underlying concepts and categories informing beliefs and behaviour may be taken for granted, and the notion that they should be identified and scrutinized strongly discouraged. The investigation of health and sickness is typically divorced from the study of political economy and history, let alone local conceptions of suffering and therapy. Reductive styles of reasoning render wide-ranging, non-quantifiable explorations interesting, but 'anecdotal'. Anthropologists who do more than address predefined questions such as 'What are the social and cultural factors that impede the willingness or ability of patients to comply with particular drug regimes?' or 'What are the social and cultural barriers that need to be overcome to ensure the smooth running of a vaccination campaign?' are likely to be viewed by at least some of those they work with as difficult, foolish or possibly threatening. They may well find their insights dismissed as 'not proper science'.

<t>As a result, many so-called 'applied medical anthropologists' end up doing 'knowledge, attitudes and practice surveys', 'focused ethnographic research', 'process monitoring' or 'participatory appraisal'. This is usually undertaken in a relatively short time span, sometimes over a few weeks or even days. Such techniques can produce useful findings, when done with skill. But most epidemiologists, clinicians and public health

practitioners have no formal training in anthropology and, not uncommonly, equate this way of working with what anthropologists should do. It can be hard to suggest otherwise. The problem is compounded by the fact that some 'applied medical anthropologists' are comfortable with a range of qualitative methods other than participant observation, and basically accept the premises and approaches of biomedicine as a starting point for their work (see, for example, Pelto and Pelto, 2008 [1978]). Eager to work with epidemiologists and clinical professionals, they rarely challenge biomedical understandings of evidence, whereby data are treated as objective 'facts' that have analytical power when they form a significantly distinctive pattern. Compliance with biomedical approaches to evidence has led, on occasion, to tense verbal exchanges between such 'applied medical anthropologists' and other anthropologists, who maintain that a distance from biomedical paradigms and research grounded in comparative ethnography are crucial components of the discipline.

<t>All of this is not to suggest that anthropologists who carry out conventional participant observation fieldwork and who treat biomedical policy and practice as an aspect of their fieldwork are not able to make useful practical contributions to contemporary public health issues (see, for example, Davison et al. 1991; Fairhead and Leach 2006, 2007;   Harper 2006; MacGregor 2006; Shaw 2009 Whyte et al. 2004). It is important to emphasize, however, that a key aspect of the influence that critically engaged anthropologists can have on public policy is that the status of evidence is not simply set aside. To be recognized as important rather than 'interesting', anthropologists have to take into account, and engage with, the ways in which evidence is generally constructed by biomedically orientated researchers and practitioners. It can lead to some

challenging encounters! The tensions may be illustrated with reference to our own research on schistosomiasis in East Africa (Parker et al. 2008; Parker and Allen 2011; Allen and Parker 2011).

<t>Work carried out by epidemiologists and parasitologists, as well as some applied qualitative researchers, suggested that treatment programmes were going well. Kabatereine et al. (2007), for example, followed a cohort of children over a period of two years in north-western Uganda. These children were treated every year with two drugs, praziquantel and albendazole, as part of a national programme to control schistosomiasis (bilharziasis) and soil-transmitted helminthiases (parasitic intestinal worms). The selected children were monitored before treatment and at six-monthly intervals after each annual round of treatment. Key biomedical indicators included noting whether the children were positive or negative for these parasitic infections and, if they were infected, their intensity of infection and the presence or absence of clinical signs of infection such as hepatomegaly and splenomegaly. Additional epidemiological research was undertaken by Brooker et al (2005). This was a cross-sectional study monitoring changing patterns of infection among school children in Uganda over time. Both studies suggested that endeavours to control neglected tropical diseases by distributing drugs to school children, free of charge, were having a positive impact as there was a marked reduction in prevalence and clinical indicators of morbidity. In addition to this biomedically orientated research, Fleming et al (2009) undertook process monitoring to explore adult responses to the mass distribution of drugs at selected sites in north-western and south-eastern Uganda. This paper also presented a positive assessment of the National Control Programme for the control of schistosomiasis and soil-transmitted helminthiases, albeit

from a different perspective; and argued that there was growing demand for treatment among adults and children.

<t>By contrast, ethnographic research undertaken by Parker et al. (2008) on local responses to the national integrated control programme in north-western Uganda suggested that the picture was much more complicated than that conveyed by the above studies. It highlighted the point that it is hazardous to assume that targeted populations will necessarily understand or agree with the rationale for the free distribution of drugs to adults and children, and demonstrated that treatment was being actively resisted in some places. While there was no doubt that large numbers of children were swallowing tablets under the authoritative gaze of their teachers, there was every indication to suggest that adults did not feel similarly compelled to take the medicine and, in some cases, forbade their children to take it too.

<t>Further research undertaken in north-western and south-eastern Uganda (Parker and Allen, 2011) highlighted mistaken assumptions guiding the delivery of drugs, and raised the question of how much weight should be given to different kinds of evidence. Kabatereine et al.'s research (2007), for example, assessed the impact of treatment on a cohort of primary school children. Our own local-level research suggested, however, that it was misleading to extrapolate findings from this study to make general observations about the effectiveness of the national control programme for several reasons. First, the number of children successfully completing primary school in under-resourced, rural areas is small, so the number of school-aged children receiving multiple rounds of treatment is likely to be small too. Second, cohort studies are necessarily biased towards children from families of long-term, stable residency and relative wealth. In

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north-western Uganda, however, mobility is a feature of daily life – a point underlined by the large numbers of children that dropped out of the study over time. Third, local research revealed that hundreds of Congolese and Sudanese people regularly move across borders to this part of Uganda. As yet, there are no national control programmes for the treatment of tropical diseases in these neighbouring countries and our own parasitological investigations revealed that more than 95% of migrants in the study area were positive for *S.mansoni* and/or soil-transmitted helminthiases. Almost certainly, these patterns of migration contributed to the high rates of reinfection (ranging from 54% to 84%) recorded among school children at selected schools in the district.

<t>Not surprisingly, reactions to our findings have been very mixed. The development of the Ugandan national programme for the Control of Schistosomiasis and Soil-Transmitted Helminthiases would not have happened without the financial support of the Bill and Melinda Gates Foundation, contributions from drug companies and donations of aid from the United States and other countries. This means that most of those involved in the treatment programme, from the head of vector control in the Ministry of Health to the medical researchers and drug distributors, operate with soft funding. This creates a difficult situation: How can the head of a programme, receiving millions of pounds from a philanthropic organization to undertake a piece of work, publicly say, 'This programme has run into difficulties'? Why would an international agency or philanthropic organization renew a programme if it is not working properly? There are enormous pressures to 'prove' that money is being spent wisely. For all the critical comments and private reflections, it is not even possible for a Minister of Health

or a programme manager to say that 'I do not know if the programme is working well or not, because the available information is too partial'.

<t>With respect to our research on neglected tropical diseases in Uganda, it has led to an array of responses. These include contacting informants in the field to tell them it is their duty as citizens of their country to say positive things about free treatment; requests by personnel implementing the programme for us to present our information selectively; and heated exchanges conveying a sense that we are disloyal and risk undermining the future of free treatment for debilitating diseases. However, there have been other occasions when we have discussed the strengths and weaknesses of our findings with biomedical practitioners and researchers with the common purpose of improving treatment and monitoring. This has not, as yet, led to material changes in the running of the national control programme, but the desire is clearly now there to respond to the difficulties identified. Our findings are in the process of being published and disseminated, not least by staff from the World Health Organization, where some tropical disease specialists have become as concerned as us about current trends in mass drug administration.

<t>This also applies to our research on tropical diseases elsewhere in East Africa, which if anything proved even more controversial. A layer of complexity was added by the fact that we had a PhD student doing fieldwork on tropical diseases in Tanzania at the time that one of our reports landed on the desk of a key funding institution. The report highlighted fundamental problems, including widespread over-reporting of drug take up. The director of the programme told us that he found the executive summary so depressing he could not bring himself to read the rest of the document. He did not suggest that

anything we had written was inaccurate, but he was reluctant to circulate the report, and keen that it was not shared with the Tanzanian Ministry of Health. He was concerned that senior figures might react by deciding to work with alternative agencies in the future. With an array of implementing agencies to choose from, it was not an unreasonable concern. We were also asked not to publish our findings, because there were concerns that the information would jeopardize future provision of free medicines by drug companies.

<t>Taking these factors into account, as well as the vulnerability of our PhD student if we rocked the boat too much, we decided to delay publication. Almost two years later, and having gathered more information (including details about riots that took place during some drug distributions), we provided advance notice that we would now publish. The idea was not greeted with enthusiasm, but we responded by saying that considerable time had passed and discussions to alter the structure of the national programme concerned were already at an advanced stage. We have thought carefully about our audience and have chosen to avoid conventional medical journals, because the reviewing process tends to mute critical voices and discourage the publication of articles with negative results (Allen and Parker 2011; Parker and Allen 2011).

<t>While some policy makers and biomedical researchers welcome our contribution, others do not. On the one hand, the World Health Organization recently highlighted our findings by placing a direct link to one of our articles on their Tropical Disease Research website (http://apps.who.int/tdr/svc/news-events/news/social-science-ntd.). On the other hand, a researcher based at a medical school involved in monitoring one of the treatment programmes responded to the same publication in negative terms. To

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quote from a circulated email: 'People who have read this [paper] say "Are they for you or against you?" On balance I have to say it reads like against. Which considering all the support you have received is … disappointing….'. It is possible that opportunities for us to do further fieldwork on neglected tropical diseases will be restricted, but we are optimistic that things will not work out like that.

<h1>Ownership and dissemination of findings

<t1>Tensions over research findings can sometimes generate more overt confrontation, especially when it comes to the 'ownership' of data. Anthropologists are far from unique in having to confront these difficulties. Well-known economists, including Joseph Stiglitz and William Easterly, have been compelled to leave the World Bank, in part because they used World Bank data to raise critical points about the institution's policies in their publications. Thus, the following example may be extreme, but it is far from exceptional.

<t>Parker undertook several years' fieldwork on sexual networks in the United Kingdom. This fieldwork was part of a multi-disciplinary research project involving collaboration with epidemiologists, mathematical modellers and clinicians. Based at an NHS-funded clinic for sexually transmitted infections, the research tried to map out sexual networks in a multi-cultural urban setting by tracking gonorrhoea as it spread from one person to another. Within six months, it became apparent that it was going to be little short of impossible for Parker or any member of staff from the clinic to move much beyond the collection of a series of individual accounts of recent sexual contacts (which are usually referred to as egocentric networks by mathematicians, epidemiologists and sociologists). The reason was simple: almost no-one attending the clinic for the treatment

of gonorrhoea was willing to impart sufficient information to enable their sexual contacts to be contacted through the existing system of partner notification, and few patients could be persuaded to bring their contacts into the clinic to be screened. At this juncture, relations between staff on the project became fraught. It was hard to see how an interim report, let alone a final report, to the funding agency could say anything of much interest about sexual networks. On what basis could the salaries of the research team be justified when the project was generating minimal data of academic or practical interest?

<t>Meetings followed meetings and various suggestions were made about how to amend the study design. These included visiting known public 'cruising' sites late at night, and asking those soliciting or offering sex if a member of the research team could return to their homes to chat about their sexual contacts. Parker, unconvinced that such suggestions would work any better than the previous ones, suggested that it might be easier to map out sexual networks through the study of HIV/AIDS rather than the study of gonorrhoea. Preliminary research suggested that those who were HIV positive were more talkative and trusting of staff associated with the clinic than those free from this infection, but infected with other sexually transmitted infections. However, there was no support for developing the study in this way. Concerned that the other proposed amendments to the study design would yield nothing, Parker decided to pursue the work on sexual networks and HIV transmission independently.

<t>It was not until the end of her contract that staff working on the project realized how little useful data on sexual networks and gonorrhoea had been collected by anyone on the team. At this point, the head of the clinic called a meeting. Aware that the funding agency would require a detailed report demonstrating that money had been well

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spent, he insisted that the clinic should take possession of all the data that Parker had collected on sexual networks and HIV transmission. She was told to bring all the data she had gathered to the clinic and hand it over. The data would then be available for others to analyse. For Parker this was unacceptable. She had trekked around London for the best part of two years, often late at night and during weekends, following leads that would enable her to map out sexual networks. No-one else at the clinic had been involved in the fieldwork, or had any idea who was connected to whom. Indeed, clinic staff had gone out of their way to ensure that no research on HIV networks had taken place in the clinic unless it was directly linked to the gonorrhoea network study. Quite apart from what she perceived to be an injustice towards herself, she thought that it would have been a travesty to leave others with the task of interpreting her material, and a betrayal of the relationships she had built up with her informants.

<t>When Parker refused to comply, she was told that legal action would be taken against her and that senior staff at the hospital would do everything possible to ensure that she never obtained another academic job. Shocked and a bit frightened, she considered capitulating. However, she took advice from other anthropologists, including a senior anthropologist at London University, who took the view that threats of this kind were immoral – exploiting the position of an anthropologist whose contract had just ended. Parker was immediately offered an honorary research fellowship and institutional support. She was also advised to contact the human resources division of the teaching hospital concerned, and to her relief was told that the incidents in the clinic amounted to institutional bullying. There were doubtless then some frank internal exchanges, and the case was dropped. Parker proceeded to publish her fieldwork (Parker 1999, 2001, 2006).

<t>In common with Mosse's research involving DfID in India, the above is an example of how confrontational events can become. They can sometimes prove insurmountable. There have been cases, for example, where PhD students have effectively had their data taken away from them or been forced to terminate their research. However, in most cases we know of, the more extreme the efforts to control or muzzle research, the easier it has been to resist them, even if it has not felt like that at the time. It is not unusual for those exerting their authority to overplay their hand. They assume that the researcher will be easily intimidated. Standing firm and seeking professional support often does the trick.

<t>If it does not, there is another method that can be used too. It is possible to 'leak' the results. This has become very effective with the expansion of the Internet and it has made arguments about controlling information somewhat redundant. Even if a contract has been signed stating that a funding agency 'owns' the research findings, there is very little that international or national institutions can do about it. There are problems with the ways in which information is accessed and uncritically deployed from Internet sources, but the Internet certainly empowers researchers if they choose to use it. The following example illustrates this point.

<t>In 2005, Allen was funded and supported by the UK section of an international NGO to carry out research in the war zone of northern Uganda. Funding and logistical support from the agency enabled him to spend time in internal displacement camps, some of which were not easy to reach from the main towns due to land-mines, attacks by the LRA and curfews imposed by the Ugandan army. The focus of the work was on the implications of the intervention by the International Criminal Court (ICC) for

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children, both those in the camps and those who had been abducted by the LRA. The fieldwork was quite difficult, and on occasion very disturbing. It included spending nights in places where terrible atrocities had been perpetrated, and where fighting was ongoing. Amongst other things, the research revealed that the emphasis on traditional forms of healing as a means of dealing with the LRA was overstated, as were assertions that the mass of the local population were vehemently opposed to international criminal justice mechanisms. Another issue highlighted was that many of those who had been abducted by the LRA were adults, rather than children. Indeed the majority of those taken were probably over the age of 18, and very many of those being supported by aid agencies after escaping or being captured were adults too, even if they were being assisted in reception centres that were funded to deal with returned children.

<t>The findings proved challenging to the NGO, because they countered perspectives that had been taken as given. Presentations were made in northern Uganda and Kampala, and the NGO also organized a meeting in Oslo to discuss the draft report at which all the European sections of its network were present. Following this, a meeting was organized in The Hague with the ICC to discuss the situation and present in detail the NGO's concerns about possible threats to children. One particularly significant aspect of that meeting was that it was possible to demonstrate to the ICC that the protection then being offered to those interviewed by the prosecutor's team was inadequate. Several individuals who had spoken to ICC investigators had been easily located and interviewed by Allen and NGO staff. Their anonymity could not be guaranteed.

<t>A few weeks later, the final report was finished, and a launch was planned in Kampala. However, a couple of days before flying to Uganda, Allen was informed that

the NGO had decided not to have a launch after all, but to keep the report for internal consumption. This came as a shock; several NGO staff had been very positive about the study and were keen to shift away from the approach that had been in place for several years. However, it seems that not all the members of the agency's network were pleased with the findings. Although the specific reasons why it had been decided to keep the report secret were never outlined in any detail, discussions with sympathetic NGO staff suggested that there were concerns about making public the facts that so many adults had been abducted, and that there was less enthusiasm for so-called traditional reconciliation practices than had been supposed. Funding had been raised on the perception that the war was being waged by traumatized and innocent children, and some sections of the NGO had helped promote the view that reintegration of those who had spent time with the LRA, including perpetrators of very violent acts, should be based on local notions of forgiveness. To suggest that so many adults had been abducted by the LRA, and also that the ICC might have a positive role to play in dealing with the situation, was therefore viewed as a potential threat.

<t>Not surprisingly, Allen was unwilling to accept the shelving of the report. The contract that had been signed was vague about ownership of data, and nothing had been agreed about what would happen to the final report. This was pointed out to the NGO, as was the fact that a draft of the report had already been circulated to the ICC and other interested parties in email attachments. It was, in effect, already in the public arena. In the face of these points, the NGO accepted that the final report could be circulated, but wanted reference to the NGO having commissioned and supported the work removed. Subsequently, the report was expanded into a book (Allen 2006b) and, by the time it was

published, issues had moved on in Uganda. What had appeared to be so controversial in 2005 was more widely accepted. The NGO then complained to Allen that the agency had not been explicitly mentioned in the Acknowledgements! They will have to wait for the second edition.

<t>From Allen's point of view, there was an important lesson to learn here. It is usually a good idea to circulate drafts of a report as widely as possible by email, and to post a near-final version on the Internet as quickly as possible. Once a piece of work is available, there is little point in trying to hide it. The horse has already left the stable. The response is normally for the agency concerned to post an official version almost immediately. More recent examples of this approach are studies Allen has been involved with in northern Uganda, funded by UNICEF and USAID, and in southern Sudan, funded by DfID and PACT Sudan (Allen and Schomerus 2005; Schomerus and Allen 2010). In both these cases, as with that of the ICC report for the international NGO, findings contradicted received wisdoms. But, while some staff within the relevant agencies were concerned or even hostile to points made, many others were very keen to have points placed in the public arena that they would find hard to make themselves.

<t>In some ways it is harder to deal with the more subtle disagreements about the use of evidence. Aid agencies, government departments and medical institutions have nominally pervasive and strict ethical procedures for research. These are partly meant to protect those who might be exploited or harmed, such as patients, children, the elderly and those divulging sensitive information. However, as has often been noted, ethical clearance and ethical arguments are also used to protect the funding institutions and its personnel. In practice this can mean that there is a kind of censorship of certain

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arguments or findings, especially where they challenge or contradict established protocols. Sometimes it is not clear when ethical guidelines are really about ethics, and when they are about maintaining the kinds of consensus that anthropological research frequently subverts.

<t>To give one example, when Allen submitted his report on HIV/AIDS among the Azande to World Vision and the Southern Sudan Ministry of Health in 2006, there were many points in it contradicting assumptions, including the view that disease prevalence was very high in the study area. This provoked quite a bit of debate, but Allen was asked to make only one significant change before the report was published. He was asked to remove detailed reference to an HIV-positive man who sat in the market place and discussed his disease with anyone who would listen. This placed Allen in an awkward ethical position. On the one hand, the case for preserving anonymity was being made on ethical grounds, but the man in question had spent hours discussing his condition with the researchers, and had insisted on photographs being taken of his body so that these could be shown to a wide audience. He was also very keen that his name be recorded too, as a kind of testimonial and in the hope that funds would become available to set up a segregated community for HIV-positive people. In the end, Allen agreed to the name of the individual being removed from the report, as well as his photographs. Where the photograph was meant to appear, a caption explains why it was not included. Thus, an attempt was made to keep the thrust of the analysis and to do justice to the information collected, while respecting the concerns of the funders. Later, when the academic paper was ready for publication, a further discussion of the issue occurred with the journal's editor. His view was that it was unethical not to include the details that the HIV-positive

man wanted to have recorded. So his photographs were included, although Allen decided not to have them reproduced as clearly as they might have been, and not to include the individual's name next to them, although at that point he could have done so. Perhaps it was still a compromise, but it was entirely Allen's decision (Allen 2007).

<t>More generally, there are times when the concerns that those implementing policies have about releasing information do not fall under clear ethical guidelines, but nevertheless may seem ethically reasonable. This is particularly the case when releasing findings might have implications for someone in particular, even if that person is not really someone who might normally be considered vulnerable. This was an issue with Mosse's work on DfID. Was it ethical for him to reveal so much about off-the-record and informal discussions relating to easily identifiable individuals? Many anthropologists go out of their way to disguise the identities of those they write about, often making up the names of people and places. Should this equally apply to work at the interface of public policy? Again it can be hard to decide where ethical or moral considerations should apply. Were those DfID staff who were angry about Mosse's book right to complain about him acting unethically? Above we have mentioned a 'teaching hospital' and an 'international NGO'. Would it be better − and also ethical − to be less coy and more explicit?

<t>There are no straightforward answers. Decisions have to be made in each case based on consideration of what is appropriate, but it is important to recognize that what may seem the correct approach to anthropologists may seem unacceptable to others. Indeed, anthropologists working in multi-disciplinary and policy settings are very likely to find some people who will find things that anthropologists take for granted very

troubling, including the way in which anthropologists publish single-authored books and articles. They will on occasion, find themselves being asked how it can be 'ethical' not to recognize the contributions of those that have supervised or facilitated their research, and the local collaborators and assistants who have helped them in the field. In biomedical research, for example, it is the norm to do so, and an aspect of the way that the credibility of a publication is assessed.

<h1>Conclusion

<t1>It will be clear from the examples we have given that the two of us are now in a relatively privileged position. We both have permanent positions in academic departments. We do not have to raise our own salaries from soft funding, and we are not responsible for sustaining the income of many research-funded colleagues. Nowadays, if we were to confront serious problems, in the way that David Mosse did, we would expect our universities to back us up, just as his did. If we decide that we do not like working with a particular funder, or if contract arrangements are too demanding, we can simply say no. Our careers do not depend on 'fitting in' all the time. Others are not so fortunate, and in the early stages of our careers, neither were we.

<t>Those most vulnerable to being compromised by researching on issues concerned with public policy are those whose livelihoods depend on keeping their donors content. That is not just the case for anthropologists. It also applies, for example, to medical researchers of all kinds. From our own experiences, epidemiologists and parasitologists whose research is expensive and largely soft-funded encounter considerable pressures to produce positive results. Amongst anthropologists, those most

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vulnerable are likely to have to raise their own salaries from grants and consultancies and are not based in academic departments. We know of several situations in which such researchers have felt that they have had to keep silent, or have been forced to abandon their research when it reveals ongoing problems too starkly. So we are certainly not saying that anthropologists are immune from being compromised. Work at the interface of public policy will always involve dealing with attempts to sustain consensus views. But are anthropologists more open to manipulation than scholars working within other disciplines? We have not found that to be the case. We end with four key ways to avoid it.

<t>First, for anthropologists working on policy issues it is really important to ensure that insights are explicitly supported by robust evidence, especially when those insights are threatening to entrenched interests. This will often mean triangulating findings with other available evidence and, wherever possible, numerating results. How many people have been spoken to? What percentage acted in a certain way? These are not questions that anthropologists always ask, but if you do not want your analysis called 'anecdotal', then you need to demonstrate that insights are representative. If your findings are strong enough, in the end that will be recognized, even by those who initially seem antagonistic.

<t>Second, try to make sure you have backup for when things become difficult. If you are not in a university department, check the wording of contracts and negotiate the removal of things that are overtly controlling, or perhaps just don't sign until the work is finished. In our experience it is surprisingly common for contracts only to be agreed after research is underway or has already been completed. Often it is not necessary to have a

signed contract to be insured during fieldwork, or to receive funds. Look into these things, and take advice. Link yourself with an academic department if at all possible as a visiting researcher or fellow, and find out what support might be available. If you end up being bullied, as happened to Parker when she was attached to a teaching hospital, do what she did. Contact professional bodies and senior academic anthropologists. Do not just roll over. Almost certainly, you are in a stronger position than you think you are.

Third, do not let rules about dissemination, including restrictive ethical protocols, lead to a situation in which you are effectively pressurized to act unethically. You will generally be a good position to know what is the most appropriate course of action, or sometimes the least bad one, for those who have allowed you into their lives. That is a responsibility, and it cannot just be set aside because of rules generated primarily to protect funding bodies or avoid controversy.

<t>Fourth, if you want to be a critically rigorous and independent anthropologist who works on public policy issues, then you must always seek to make your findings public. If you do not, you are likely to end up as a qualitative researcher investigating a range of pre-set questions according to some sort of established formula – such as a 'Knowledge, Attitudes and Practice' survey. Ideally, publication means peer-reviewed journals and books, but sometimes it means using other mechanisms, such as news media or the Internet. Occasionally, there will be things that must be kept confidential, but if you do not place most of what you discover in the public arena, then you are inevitably more vulnerable to being compromised. There are many researchers who for a variety of reasons take the latter route. While you may find some such individuals encouraging of what you do, because you can make points that they feel they cannot, you are likely to

make others very uncomfortable. You may well find them the most hostile of all to what you are doing, precisely because you remind them of the choice they have made.

<h1>Note

<NOTE>1 The term 'applied anthropology' is commonly used to refer to qualitative research and policy work, influenced by academic social anthropology. It is normally commissioned by policy-related institutions on a consultancy basis, and is particularly common in the health sector and 'grassroots' international development programmes.</NOTE>

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