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INTRODUCTION: SPECIAL ISSUE ON THE ETHICS OF ANTHROPOLOGY IN EMERGENCIES

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Introduction

West Africa's Ebola virus epidemic (December 2013 to January 2016) thrust anthropology into the public eye. It is hard to think of a recent moment when anthropology as a profession has had a higher profile. Anthropologists have been active in the Ebola response, both as policy commentators (Sridhar and Clinton 2014; Abramowitz 2014) and frontline responders (Bedford 2015). On the ground, anthropologists worked alongside other public health professionals to trace patient contacts, manage burial practices and guide both the medical responders on the social dimensions of the outbreak and the general population on the behaviours of the virus and its clinicians (Bedford ibid.).

These events have given anthropology coherence in the popular imagination—a public image. Now, when asked the perennial question, 'Just what is it anthropologists *do*?' we can point to the headline news. Media sensationalism aside, the work of anthropologists in the Ebola epidemic has had a practical, visible impact. More than adding flare to the discipline's public credentials, these events have catalysed discussion on the future of the profession itself (Biruk 2015; Leach 2015; Ravelo 2015).

The Ebola response is just one example of anthropology practised in times of acute crisis. There are many actual and potential roles for anthropology in emergencies. Outside the media spotlight, ethnographers and anthropologists can be found in epidemics, mass displacements and conflict zones (Ravelo 2015; Geissler 2013; Harrigan 2011, 2012; Wood 2006); recent debates have focused on potential future engagements for anthropology (Leach 2015). With a higher profile for the discipline and the number of anthropology graduates on the rise, anthropology's engagement with acute crisis seems set to increase.

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Yet engagement brings consequences; when anthropologists enter emergencies they (and their institutions) encounter new risks. Many of the issues that arise are centrally questions of ethics: the 'who, how, where and why' of research in fraught environments. In this introduction we attempt to outline some of these issues and give a short background on anthropology in situations of acute human crises, which we refer to as 'complex emergencies'.[2] Each subsequent article in this special issue examines some aspect of the nature and ethics of fieldwork in complex emergencies.

The materials presented in this issue are the product of a recent workshop on the subject of anthropology in complex emergencies. Entitled *Fieldwork Ethics in Crisis: Practical Considerations for Ethnographic Research in Complex Emergencies*, the authors convened the workshop in the summer of 2015 with the support of the School of Anthropology and Museum Ethnography (SAME) and Wolfson College, both of the University of Oxford. The event brought together anthropologists and emergency practitioners to discuss the past, present and future of anthropological engagement in complex emergencies. While the presenters and their topics formed a diverse group, they were united by field experiences, each presenter having grappled with the ethical complexities of crisis in their own fieldwork. This special issue presents, in expanded form, some of the papers from that day.

A brief introduction to anthropology in emergencies

Anthropology has a history of engagement with crisis and emergency; some of the twentieth century's most prominent anthropologists were concerned with understanding how people deal

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[2] While the term 'complex emergency' most often refers to armed conflict, here we broaden it to encompass public health emergencies and natural disasters, particularly when they unfold in remote, violent or resource-poor environments. As David Keen notes, the term is imperfect, but better than the alternatives: '"complex emergency" draws attention to complexity and embodies a useful degree of vagueness about the nature of a violent conflict' (Keen 2008: 1). While we use the term partly as convenient shorthand, it also draws attention to the social and anthropogenic nature of crisis. Put simply, 'there is no such thing as a natural disaster' (Smith 2006; Squires 2006); the anthropogenic factors that underlie mass casualty disasters, such as over-crowding, under-nutrition, poorly-resourced health systems, inadequate public infrastructure, and dysfunctional or unresponsive governments, are social and political phenomena (Bankoff 2003: 152-5; Smith 2006; Squires 2006). According to Keen (*op cit.*) these social dysfunctions serve a function: inequity, almost by definition, has beneficiaries. A related perspective argues that all human emergencies are underpinned by structural violence (Farmer 2009: 261). Ultimately, the very designation of a set of phenomena as an 'emergency' is itself a social act and a collective invocation to serve moral and political ends (Calhoun 2004: 376-7). Emergencies are, above all, social.

with social upheaval, particularly the massive and widespread social change wrought by the colonial encounter (Malinowski 1926; Turner 1972 [1957]; Douglas 2002 [1966]). Until relatively recently, however, field-level ethnographic accounts of mass emergencies have been rare. Mid-century, Firth (1959) and Spillius (1957a; 1957b) wrote with scientific clarity on famine in Tikopia, while Turnbull (1972) managed a somewhat less lucid account of displacement and disaster in 1960s Uganda. These encounters were largely accidental; the anthropologists arrived, coincidentally or unawares, as a potential catastrophe unfolded in their fieldwork communities. This happenstance form of study would change in subsequent decades. Following the pioneering work of anthropologists like Barbara Harrell-Bond (1986), Alex de Waal (2005 [1989]), Liisa Malkki (1995; 1996) and Sharon Hutchinson (1996), more ethnographers actively began to seek out and engage with issues of war, disaster and forced displacement as core research topics. There is now a significant body of work on the anthropology of violence and complex emergencies (among many others, see volumes by Nordstrom [1997; 2004]; Englund [2002]; Hammond [2004]; Richards [2005]; Ellis [2007]).

These new engagements brought with them new ethical dilemmas, or more precisely, a renewed focus on old ones. In complex emergencies the divide between subject and observer can be extreme, thus making the methodological questions stark. Just how is one to be a participant observer of disaster? Is it ethical to conduct ethnographic research during war or famine? Can one be a neutral observer while others suffer and die? And if one takes the opposite tack—to actively intervene, more participant than observer—what happens to one's ethical and epistemological stance? (de Waal 2005 [1989]: 2-4; O'Neill 2001:225-9). These questions have no conclusive answer; their elements and considerations are slightly different for every study. One can read about how anthropologists negotiated these questions in the past (Spillius 1957a: 3-27; 1957b: 113-24; O'Neill ibid.; James, this issue), while the ethics review process can prepare the anthropologist for the dilemmas they will face (Ford et al. 2009; Lowton, this issue), but ultimately the trickiest conundrums are often negotiated on the wing (Felix da Costa, this issue).

Thus, despite excitement over anthropology's constructive role in the Ebola response, the discipline's participation in complex emergencies is not without controversy. By way of comparison, consider another public moment for anthropologists: the outcry and debate—at its

fiercest about five years ago—surrounding military anthropology, particularly the US military's implementation of the Human Terrain System in Afghanistan and Iraq, whereby anthropologists are embedded within military units in pursuit of counter-insurgency objectives. For many anthropologists this practice is an inexcusable compromise. Critics argue that, not only does the practice implicate anthropology in extreme forms of physical violence and support an essentialist political rhetoric, but also, through its association with counter-insurgency and intelligence-gathering, it endangers individual fieldworkers and poisons the discipline for the long term. The counter-argument states, simply, that a bit of anthropological knowledge can go a long way—a moderating influence that might enable anthropologists to mitigate the most egregious collateral casualties (for more on the debate, see Forte 2010; Bristol and Jones 2007; González 2007: 14-15; Lucas 2009: 5-9; Price and Sahlins 2013; also see Kunnath 2013 on the ethics of anthropologists 'taking sides' in armed conflict).

It is informative to compare the furore over military anthropology with the present enthusiasm for the discipline's role in the Ebola response. At a quick glance, the comparison between the armed forces and public health might appear disingenuous. There are clear differences in motives: one studies and practises systematic lethal violence, while the other fosters life. Yet there are key historical and organizational similarities in methods and practice. The role of the military in the institutionalization of medicine and public health is well established (Foucault 2012 [1963]: 69, 80-2; Collier and Lakoff, 2008: 7-8, 13-15). Public health campaigns follow a military logic in their vocabulary, organization, planning and execution. This is particularly true in outbreak situations, where the aim is to delineate and eradicate a pathogenic threat. Since population health is a major factor in geopolitical stability, the military has been a prime mover in many public health campaigns, including the current Ebola response (Martin 2012: 24-5; Lakoff et al. 2015). For good reason, therefore, military analogies are perhaps the dominant metaphors of medicine (Sontag 1990: 97-9) and—at a time when enemy ideologies are a cancer and surgical strikes target terror cells—military campaigns make use of the language of healing medicine.

There are also clear commonalities in how anthropology is perceived and represented in military and public health circles. What both approaches share is a reification of 'the

anthropological' as a specific category (or terrain) of knowledge, with the anthropologist as a custodian or gatekeeper of knowledge. At times, this can translate into a drive to operationalize anthropology, to put 'the social' on a footing amenable to technocratic intervention and to package and communicate 'culture' in a format that is easily understood by people from very different social, educational and professional backgrounds (Lucas 2009: 5-6).[3] Given anthropology's colonial legacy, many anthropologists will approach these institutional attempts to operationalize anthropology, whether in the name of arms or of health, with suspicion (Asad 1991:314-315); the same structures, networks and technologies that make it possible to project military force around the globe also make it possible to mount a public health campaign to combat Ebola (Martin 2012: 25-6).

This highlights a key dilemma. Most anthropologists want their work to benefit others; they often feel they have something to offer in situations of acute need. Their subject matter or regional expertise may give them special insight into a local crisis dynamic, and they are often ready to offer their services (Abramowitz 2014). But whom are they to serve?

Most anthropologists want their work to benefit the people and communities they live among; they may view themselves as spokespersons and advocates for people who are otherwise under-represented in research and policy (Scheper-Hughes 1995: 411; Marcus 2010: 371). Yet in a public health emergency or other complex crisis, community-based advocacy may prove untenable or ineffectual (Felix da Costa, this issue). Anthropologists may wish to speak—and work—for the weak, but the crisis response mechanisms belong to the powerful (Revet 2013: 48, 50-1; McKay, this issue).[4] The fear of misuse or instrumentalization of research might be

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[3] For an example of this tendency in health fields, see Abramowitz et al. 2015. Although the editors, not the authors, likely chose the title's reference to 'social science intelligence,' it illustrates how anthropological knowledge may be conceptualized by and for other professions.

[4] The most influential contemporary anthropological debate on the subject characterizes complex emergency as a 'state of exception' where the power to impose legal and moral interpretations, to re-shape political and economic norms—often with unintended consequences—belongs to those who control the response. Complex emergencies amplify power differentials and vulnerabilities. In a state of emergency, the laws, rules and norms that govern everyday life may be suspended (see Agamben 1998; Redfield 2005: 329-330). Those who were weak before are made weaker, while the old power structures persist and often reassert themselves with force (Fassin 2011: 181-2; Ticktin 2014: 278). In crisis, action itself imparts a moral right of interpretation and imposition. This, naturally, means powerful governments and international actors most often dominate the moral, political and physical sphere in the wake of crisis. See Ticktin 2014 for a review of the anthropological literature on states of exception in complex emergencies.

particularly strong for anthropologists, whose work is based upon acceptance, trust and intimacy (Lane, this issue).

Fieldwork ethics in crisis

With these tensions in mind, the authors convened the workshop on *Fieldwork Ethics in Crisis* mentioned earlier. In light of the seemingly growing numbers of anthropologists working in unstable contexts, the aim of the workshop was to bring together aid practitioners and academics to map the current state of practice, outline key dilemmas and draw out recommendations for future action. The workshop asked a single question: how to conduct ethical, rigorous, independent civilian ethnographic research in wartime and disaster?

The workshop's central question split into several sub-questions:

(1) What, if anything, is new or distinctive about ethnographic research in and of emergency interventions and relief work?

(2) What are the experiences of ethnographers in these situations, past and present?

(3) What, if anything, makes the ethics of working in complex emergencies different from the ethics of working in non-conflict zones?

(4) What are considered acceptable degrees of risk, for both research participants and anthropologists? Do anthropologists and participants face more danger now than they did in past?

(5) How do new technologies and institutional configurations change research methods and ethics in complex emergencies?

Workshop participants were given these questions in advance and asked to present on one or more of these themes. Some of the presentations from the day are reprinted here, in a revised and expanded form.

In the opening submission, Wendy James recounts her fifty-year engagement with the peoples and crises of Sudan, South Sudan and Ethiopia. The Blue Nile region (where the borders of these three states meet) forms the backdrop for much of her reflection. James relates some of her struggles with officialdom—demands from bureaucrats and security services for supervision

and input where there was neither understanding nor particular interest in her research—and her subtle manoeuvres that enabled her to work, for the most part, as she wished. One is left with the impression that the more things change, the more they stay the same: James confronted many of the institutional attitudes and hurdles that young ethnographers face today. Yet she worries about a new institutional factor, that current research agendas, particularly those driven by urgent response, miss out on what is perhaps the preeminent strength of an anthropological approach to complex emergency: continuity through time and space.

In a related vein, Diana Felix da Costa offers a personal account of how she navigated ethical dilemmas as a doctoral student among the Murle of South Sudan. She relates the shifting fortunes of her research as hostilities engulfed the region and eventually overtook her research site at Boma. In her reflections, Felix da Costa emphasizes the 'accidental' and 'improvisational' nature of anthropology done in wartime. She argues that this makes for flexible and adaptive research, though the ethics and outcomes of such an approach may not always conform to institutional expectations of rigour. As such, the tension between subjectivity and objectivity in anthropology becomes more visible when research is conducted under duress.

Karen Lowton advances a perspective from the other side of the desk. Writing as a member of a university ethics review board (ERB), Lowton outlines how an ERB approaches high-risk research. Lowton argues that the ethics review process should be collaborative, not adversarial; the review allows the researcher to think through both ethical and practical considerations, in most cases making a more robust research plan. She concludes her piece with a list of key considerations for research in emergencies, gleaned from her experience as both a social scientist and an ERB member.

Melyn McKay and Alissa de Charbonnel argue that not only research institutions but also aid agencies should engage with these ethical guidelines. Positioning the aid economy in the field of ethics at large, the two authors explore how 'Big D' development has misapplied anthropology's disciplinary focus on the 'local'. They argue that the drive for community-informed development has objectified both local knowledge and local researchers. The rise of evidence-based programming, value for money (VfM) principles and 'remote' aid management contribute to the increasing, and competing, demands upon researchers. Ultimately, they write,

this subcontracts risk in the research apparatus to its most vulnerable actors – local researchers themselves.

While there is historical continuity to the dilemmas that confront anthropologists working in complex emergencies, there are emergent ethical dilemmas too. As Justin E. Lane demonstrates, the rise of digital technologies—in particular big data applications—raises new concerns for anthropological practice, especially for anthropologists working in insecure environments, where, in the wrong hands, some data can get people killed. Here, the emergent ethical dilemma coalesces in 'The Cloud'. As data management takes on new gravity, Lane makes a strong case for anthropologists to be better prepared to manage data security in the field.

The workshop presentations on the day—and in this issue—share certain common concerns. Perhaps the central concern is an awareness of the heightened moral, physical and emotional stakes that can exist between individuals and institutions in times of crisis. Ethnographers have a duty of care towards themselves, their informants, host populations and the discipline of anthropology. They must also be respectful and responsive to the needs of institutions such as the university, the host government and a variety of governmental and non-governmental responders. Consideration of how to navigate between these multiple obligations was a feature of every presentation.

Participants concurred that there is little published in the way of guidance for anthropologists setting out to work in extreme environments. Similarly, institutional guidance is limited or poorly mapped. Institutions, whether university departments, ERBs or aid agencies, are caught between obligations to their individual members and to the body corporate; they appear to struggle to provide adequate guidance and support in every instance. As a result, the workshop came together to recommend a further conversation on the rights and responsibilities of anthropologists working in crisis situations: how individuals and institutions can work together to better facilitate safe and ethical research.

Most of the responsibility for self-protection and integrity of research is borne by the individual researcher. It has always been this way in anthropology and should remain so—no institution, checklist or process can abrogate individual responsibility. However, the role and potential for institutional ethics review boards to support that process may be underestimated.

Participants concurred that a well-functioning ERB can strengthen the quality of planning, facilitate critical reflection and identify potential resources and partners.

Participants in the workshop identified particular institutional mechanisms that may be under-prepared to address questions arising from research conducted in conflict zones. The conclusions of our workshop echo an earlier special issue of *JASO* on the subject of sexual harassment in the field. In their introduction to that issue, editors Imogen Clark and Andrea Grant highlight the need to incorporate more danger-awareness and avoidance skills into pre-fieldwork training for student anthropologists, as well as the need for greater peer support upon return (Clark and Grant 2015: 4-6, 11). While Clark and Grant voiced these concerns with particular reference to young ethnographers confronted with changed sexual and gender norms, related concerns can be extended to all anthropologists, student or not, working in or near complex emergencies.

It is clear that more attention to individual welfare is needed, but this can be difficult to implement in an institutional setting, where solutions are ultimately administrative. It seems likely that any increase in welfare support for fieldworkers will be accompanied by increased scrutiny of itineraries and research plans. An over-protective institution can potentially add additional research administration or even discourage potentially valuable research without having a corresponding effect on welfare (Haggerty 2004: 392-4). Thus institutional administrators must balance individual and corporate needs, duties and risks. Our workshop highlighted that institutions cannot afford to allow corporate risk management (and the tendency to stifle research in favour of playing it safe) to predominate (Lowton, this issue). If they do, the gap between researchers and their subjects will become even greater (James, this issue). The borders between research autonomy, support and accountability are difficult lines that researchers and institutional officials must establish together.

As the articles in this issue make clear, planning, preparation and the institutional review process can be essential to success. Yet in unpredictable environments, much is down to the individual researcher. Ideally, the institution will equip each researcher with a robust toolkit of ethical guidelines and practical methods. But no matter how well prepared, the process of fieldwork is, by nature, organic and at times improvisational (Felix da Costa, this issue). The

anthropologist's practice makes the field—and this field is a space that must be negotiated daily. In exploring these issues in more detail, the *Ethics in Crisis* workshop, and this special issue of JASO, hope to help equip anthropologists in their daily practice.

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THE INNOCENCE OF FIELDWORK LOST IN THE SHIFTING LANDSCAPES OF WAR:
A CASE STUDY FROM THE UPPER BLUE NILE (1965-2015)

WENDY JAMES[1]

Introduction

There is a big difference between the highly specific nature of modern aid projects – in time, space, objectives, methods, funding, organized team-work and reporting back duties – and what we like to think of as the more creative, personal nature of academic research in the social sciences, especially ethnographic fieldwork. What has recently become known as 'classic' fieldwork never seems to have been boxed in by questionnaires on method and timing answered in advance. Rather, it was supposed to remain exploratory, and open to the unexpected. The 'ethical controls' of today's bureaucratic paperwork rarely prepares the researcher for the independent judgement that he or she may have to exercise in the field, especially in the more politically and socially turbulent regions of world today. I was staggered when I recently looked up the current tangle of procedures that Oxford's own Central University Research Ethics Committee requires researchers to go through (*https://www.admin.ox.ac.uk/curec*, last accessed 31 October 2015), quite apart from those of the academic and other funding bodies, NGOs and political gatekeepers they will face if they plan to work overseas.

Perhaps the root questions of most ethics review boards etc. arise from the medical need for potential treatments to be tried out and thus experiments to be done on living human beings. Many questions assume that the researcher will be dealing with individuals in a structured situation (perhaps a focus group). One that struck me on the CUREC forms was 'Will you be working with children?' But we do not study individuals or categories of persons as such; we study fields of social relations, interactions and live opinions. You cannot exclude children from your study, nor can you draw clear boundaries, or screen out linguistic differences, disagreements and inequalities, as they shape, together, the social world you are investigating. Moreover, that world will be changing over time, and there are limits to what can be understood about it on the basis of one limited research trip. Repeated visits are always to be sought if possible, providing

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perspectives on the seasons, at least, and changing conditions over the years, through soliciting memories, but also reading historical sources, and thus adding to what you can discover of the life of your community's grandparents. Fieldwork should never be restricted in space: you should ideally visit places where your informants say they used to live, or where their children have now moved. Nor should it artificially restricted in terms of language: you should aim for meaningful conversation in the context of one or more relevant languages yourself if you can manage it, but otherwise through a range of local interpreters. You should not be satisfied just gathering information from an anonymous focus group according to pre-set questionnaires, formulated in the language of the research project itself. As far as you can, you should actually be getting to know people, as they should be getting to know you.

I have never worked as a direct employee of any government scheme or aid agency in order to provide them with specific information for a defined project, but concentrated rather on publishing the fruits of my original, 'innocent' efforts to add to the ethnographic record on little-known minorities in the Upper Blue Nile region borderlands linking Sudan and Ethiopia (the main examples being James 1979, 1986, 1988) or to explain my own approach to comparative anthropology in general (2003). My sources of funding have been almost entirely through universities where I was fairly free to operate as an independent researcher, or through the former UK government-funded Social Science Research Council (SSRC), later morphing into the present Economic and Social Research Council (ESRC), which has come to exercise much more intensive financial, methodological and 'ethical' scrutiny over research and researchers. The national or gender identity, social experience, theoretical grounding, language abilities and existing local knowledge of the researcher tended to be recognized in the old days as relevant to whatever their research might achieve. They were expected to be able to make sensible political or 'ethical' decisions as and when the local circumstances might change. As my own experiences have shown, one may find that sometimes it is helpful to be seen as an authority, or an NGO figure, and basically friendly to people, whether Muslims or Christians (though in some cases I had to insist I was definitely NOT a missionary!) Despite even today's vetting and oversight by funding and ethics boards, individuals and research teams will always have to exercise their own judgement in the production and use of research findings. In the troubled times of uncertainty over where authority might properly lie, let alone in regions dominated by actual currents of conflict and war, the researcher may need to draw on considerable reserves of good sense, diplomatic skill and a

view of future outcomes beyond those of the ethics questionnaires that have already been answered.

1. From Oxford to Khartoum, 1960s

During my time as a graduate student at Oxford's Institute of Social Anthropology (later 'Social and Cultural Anthropology'), competition for research funds was growing, and various specifically 'colonial' sources of funding were disappearing. Several of us going through the old pattern of a Diploma and B.Litt. (now M.Litt.) decided to seek jobs in the country where we hoped to do our research for the D.Phil. as a way of getting started. This is basically how I found myself, by 1964, teaching in the University of Khartoum. The Department of Social Anthropology and Sociology there (founded in 1958, two years after the country's independence) was keen to appoint young lecturers and give them access to its own research funds from the Ford Foundation. But before that happened, I had a steep learning experience.

The Sudanese authorities, and people generally, were respectful of the university and its activities. In the case of our department, this was partly because Ian Cunnison, as the first professor, made sure that plenty of 'applied' topics were included in the teaching, along with classic writings by the Seligmans, Evans-Pritchard and so on. I remember that we had two core courses on 'Social Problems of the Sudan': the first on rural, the second on urban issues.

My very first opportunity for a taste of research came when our department was invited by the Ministry of Health to send a team to investigate an apparent breakdown in mental health in the slums around the city of Port Sudan. Having just arrived, I was the only staff member free to take this on. We put together a group of students, and a senior colleague agreed to join us for the first few days to introduce us to the Town Council officials and get us started. We divided ourselves up into pairs, took the train and devised a plan for covering representative areas of the two main shanty towns. Because of political upheavals in October of that year our trip had to be cut short, but we returned in the early months of 1965 to complete the study. The officials had told us that the main problem was drought in the nearby Red Sea Hills, with pastoralists losing their cattle and therefore squatting in the town, where they began to suffer. Yet, our findings turned out to be quite different; in the shanty town behind the slaughterhouse lived mainly temporary or seasonal labourers who made good money in the docks and took it home to the Nuba Hills or other rural areas, even in the south of the country. In the wide open spaces of the second shanty town, we

again found relatively comfortable people, many of whom had moved out of the city centre because of overcrowding. This settlement was known as Dar el Salaam (Haven of Peace). When we presented our findings to the Town Council officials, however, they denied that anybody lived there at all. Pointing to a map of the city, they said, 'But nobody is living there! It is scheduled as a First Class development.' We had to report that we had estimated the population at 2,000 and that none were pastoralists from the hills; the text was included in our departmental journal, attracted some interest and later reprinted (James 1969). This whole exercise could perhaps be seen as a modest example of what used to be known in the colonial period as 'applied anthropology'. In the old days, anthropology came to be seen as a handmaiden of colonialism. But even then, this sometimes masked the way anthropology could critically analyse – and perhaps even call into question – colonial rule and administration (James 1973). At least we had been free to design our own approach, and methods, and write up what we hoped would be a helpful report, even though it was pretty critical of the relevant authorities (and far from what our sponsors had expected).

2. Planning ethnographic exploration in the Sudan

At that time, the options for personal research funded by the department's Ford grant were fairly clear. Applications were considered by a number of committees within the university, but I do not remember going through any difficult interviews. Key discussions were mainly a matter of *where you planned to go*, as some areas were sensitive; for example, because of the escalating conflict in the south, none of us could have gone there. I seriously looked into the possibility of the Nuba Hills, an area that boasted the complexity and diversity of two or three dozen different indigenous languages, but Ian Cunnison dissuaded me from that idea – and in any case we were being joined by a new colleague already committed to doing research there, Jim Faris. I eventually focused on the possibilities of the hilly country of the Upper Blue Nile province and started exploring the archives in the National Records Office, which I found fascinating. The archivist explained quite straightforwardly that I could read anything on the Blue Nile except files on slavery.

I did take up the study of colloquial Arabic as a desirable skill for fieldwork anywhere in the country, though my strongest interest was in the linguistic and cultural diversity of Sudan's marginal regions. In reading more about the patchwork of peoples that had long survived in the

Map. The Blue Nile Borderlands: relief and key places in the text

borderlands with Ethiopia, I became interested in the earlier history of the Kingdom of Sennar, which had reached into the mountains (for many of the places mentioned in this article, see Map). In one or two places, I learned that twentieth-century missionaries had settled with the local people and produced both language materials and a rising generation of kids who were learning English – this finally decided me to focus on the Uduk-speaking communities and their immediate neighbours to take advantage of the language situation, as I wasn't sure how long I'd be able to spend in the region. By the time I set off for the field with a university Land Rover and driver in late 1965, it was nearly two years since the local missionaries had been expelled (like others across the whole of the southern Sudan and its borderlands) on suspicion of their sympathy with, and support for, the growing insurgency.

In applying for my funding and basic permissions from the university, I did mention the ongoing dam-building project on the Blue Nile at Roseires, and how there was a need for general information about existing communities and development possibilities in the region. I also made a point of discussing my plans with officials in Roseires, reading in their files, and visiting various places in the district. But no demands were made on me to make any 'useful' contribution to the ongoing potentials for development. I was then welcomed by various officials when I made it down to the southernmost district HQ of Kurmuk, right on the Ethiopian border. In fact one afternoon the police were kind enough to invite me to join them on a short walk over the rocky gorge for coffee on the Ethiopian side. This was very helpful, as the shops in Ethiopia's twin town of Kurmuk were filled with Italian pasta and other goodies on which I could stock up.

I did have problems with officialdom later in the rural areas, but they had nothing to do with my academic research as such. My first visit to the old station of the Sudan Interior Mission (SIM) at Chali, some thirty miles away, was made at the suggestion of a group of Kurmuk officials who had themselves been invited by the pastor to help celebrate Christmas Day. We went in convoy; I decided this was a good place to begin my real fieldwork, focusing on the little-known Uduk people with the help of language materials originally provided by the missionaries and assistance from youngsters who knew some English. My driver was glad to leave that evening with the officials on his way back to Khartoum, leaving the Land Rover with me, and the church people were happy to have me for a while. However, Chali was a sensitive place, having been part of the 'southern' province of Upper Nile until 1953, when it was transferred to the 'northern' province of Blue Nile. This was done purely for reasons of administrative convenience, but it did separate

the Uduk (and some others) from their friendly neighbours the Meban, who had also been part of the zone in which the SIM had worked and established their first headquarters at Doro, a good distance west of Chali.

After my first week or two as a guest of the Chali church people, a local policeman showed up. Did I have permission to stay there? I had to return to Kurmuk to be interviewed by the authorities and send a telegram to the university to get them to provide a letter from the Ministry of the Interior allowing my extended stay. Permission was eventually given, and I began to see that my research plan was not the key problem. Later that year, when I returned to spend the whole 1966 rainy season in a remote village, the local police from Chali occasionally visited my hut, six miles or so from their station, to check up; they were very pleased when I was once or twice able to present them with a bag of my own home-grown tomatoes. But the merchants in Kurmuk made a collective complaint to the officials about my presence – they thought I was a returning missionary. At the same time, I had left the Land Rover with the Kurmuk police for safe-keeping, but when I engaged a new driver and we collected it, we found several hundred miles on the clock beyond my personal allocation from the University, plus a minor broken spring or something. On the way back to Khartoum we had to report this to the Province Police HQ in Wad Medani and then get the university to sort it out with them. I emerged a blameless innocent, wronged by the forces of law and order.

I was keen to persist with the Blue Nile research, returning (mainly during Khartoum vacations) for shorter field visits in '67, '68, and '69, occasionally accompanied by a Sudanese student on 'fieldwork training' or teenagers now moving to and from schools or jobs to the north, which always smoothed the way with the officials and merchants. I always made it clear when passing through places that I was a teacher in the university, which gave me some standing. I think it is worth emphasizing the value of return trips to the field. I have always suggested to students that whatever time you have, at the very least, divide it in two. What you learn on your return will be well worth it: you will be remembered, may be greeted as a friend, and learn the background to things that happened when you were there before. Even in the context of modern aid projects, I believe that showing up as a known person makes you less obviously a tool of your sponsors and gives you more independence!

3. And on to Ethiopia: the calm before the Revolution

By 1969, I had left the Sudan to write up my D.Phil. I then taught briefly in Denmark and Norway before returning to a lecturership back in Oxford in 1972. In the course of 1974-75 I had my first chance of some sabbatical leave. I got a grant from the then Social Science Research Council to go to Ethiopia to start on a comparative survey of linguistic minorities in the west of that country (several related to those on the Sudan side) and their relationship with the history of Ethiopia as a whole. This involved quite a detailed application, to cover three visits in all, and a formal interview in London (though nothing like the scrutiny and form-filling demanded by the ESRC today). I secured an attachment as a visiting researcher at the Haile Selassie I University in Addis Ababa, which gave me access to libraries and a range of useful contacts. In the early 1970s, the university and its academic staff were very supportive of research on the variety of languages and peoples across all parts of the country. Many regional dissertations had been prepared by students in history and related disciplines, and were available in the library of the Institute of Ethiopian Studies. They put me in touch with a recent graduate from the western fringes of the then province of Wallega, bordering Sudan. She travelled there with me and introduced me to key people who helped me in planning my fieldwork. It was not long before I was able to liaise with Norwegian Lutheran missionaries, who were very supportive on both my first and second visits. These were the last years of imperial Ethiopia; there was no particular interference from the government. However, the third trip of the series (planned for 1976) had to be abandoned because of the demise of Haile Selassie and the advent of the socialist regime, accompanied by violence and the imposition of a high level of state surveillance. The western border with Sudan became a Cold War frontier from 1976-89, and anthropological research, whether by Ethiopians or foreigners, became impossible. My practical research activities moved back to the Sudan following the 1972 peace agreement there.

4. An opportunity for urban research in Juba: before the storm

My next sabbatical opportunity was in the academic year 1982-3: by this time I was married to Douglas Johnson, a historian who specialized in the southern Sudan. He was actually engaged by the Southern Regional Government based in Juba to work on the development of an archive project and was making regular visits there. It made sense for me to join him during my sabbatical, with our two small children. My academic plan, approved at the Oxford end as the

main basis for my sabbatical leave, was to study the rise of the southern dialect of colloquial Arabic ('Juba Arabic') as part of the social history of the town. I kept a low profile with regard to my research, deciding not to go through the formalities of grant applications and official permission, and was able to take things up quietly with neighbours and friends as I got to know the town and the ways in which Juba Arabic worked. I found a very useful source in the local church-sponsored radio, which broadcast popular plays in the street dialect, mainly about the dilemmas and temptations of life in the town and how to cope with them. I recorded these and engaged a local student to help with translation. I even met and got to know some of the production team, who gave me copies of the typescripts of some of the plays. In some cases, it was very interesting to compare the script with the live radio performance as I had recorded it! With the help of my husband, I was able to start on what I hoped would be a substantial series of interviews with some of the older residents of the town, many of them retired military men. This work could only proceed modestly, and while I encountered no official objections, there might have been problems if I had tried to interview elite figures.

However, our time in Juba could not be completed because of the fresh outbreak of civil war in May 1983. Douglas's project had taken us to Malakal for a while, from where we managed a quick trip eastwards to the Uduk villages of the Blue Nile, which was invaluable for my own research, though it lasted only just over a week. Back in Malakal, we found ourselves stranded as a family for a while following the outbreak of a mutiny in Bor. Douglas's Sudanese colleagues were able to take the truck back, but the road back through Bor to our home in Juba was closed to us as a foreign family. At the same time, while many foreign personnel were leaving, none of the UN or NGO bodies would give us seats on their planes. This was because Douglas was officially a Sudanese civil servant, and if he and his family were helped, many others would also feel entitled. We were eventually found places on Sudan Airways out of Malakal, though northwards to Khartoum, with the help of Douglas's old contacts in the Upper Nile provincial government. It was two or three weeks later that we were suddenly offered air passage back to Juba by the US embassy, who had seats opening up unexpectedly (Douglas being a US citizen). We could no longer carry on our respective activities in Juba and eventually got home to Oxford.

By 1986, Sudan's new civil war had spread from the southern provinces northwards, unexpectedly entering the Nuba Hills and the Blue Nile Province (here, of course, with particular support from the Ethiopian side). Some youngsters from the local minorities had already been

recruited into the national police and military forces, a regular career during the years of peace; by the early 1980s others were being recruited into the SPLM/A (John Garang himself visited the Blue Nile in 1986 explaining that their cause was a nation-wide struggle for democracy and religious freedom). The severe counter-insurgency measures of 1987 forced a very substantial proportion of the Uduk-speaking people to flee (James 2007). They were on the move for six years, crossing the Ethiopian border and back several times, at one point finding themselves deep in Sudan's southern Upper Nile Province.

5. Sudan's second civil war: refugee treks to and from Ethiopia

In the course of this prolonged disaster, I found myself engaged on a series of 'emergency' humanitarian visits to a string of camps where the core of the Uduk people had sought refuge; this was my first experience of working as a professional consultant. In 1991, I went twice to Nasir, Upper Nile, reporting to the UN Operation Lifeline Sudan in Nairobi on the background to the unexpected arrival of refugees from the Blue Nile. OLS were keen to have any background on who these people were and why they had left their homes for Ethiopia, moved to and fro a couple of times before being obliged to join the large camp for Sudanese at Itang near Gambela, then having to flee with everyone else from there back over the Sudan border as the new Ethiopian government established its grip. It was, I believe, the local military (SPLA) who suggested a site south of the Sobat river for the Blue Nile people to camp, where they were easy to guard, and neither the military nor the OLS officials minded me joining them there. After two reports in which I strongly recommended that these particular refugees should be allowed to move upstream to drier country, this did happen in early 1992. But partly because of a recent split in the SPLA, one night a faction suddenly led a dash back again to Ethiopia, and as an emergency were given shelter in Karmi transit camp, near Gambela.

I made it to Karmi later and reported on the background to this unexpected event to the Addis Ababa office of UNHCR. In this new situation of refugee need, I collaborated with all authorities as best I could, making recommendations and so on. From Karmi, I travelled with officials from the UNHCR and local government in Gambela, along with leaders of the refugees, helping decide which places to recommend as the best options for a new 'semi-permanent' settlement (nobody wanted to restore the old-style refugee 'camps'). No authorities were posted to Karmi, as it was to be only a staging post, though the aid agencies did visit on weekdays to provide assistance. By the

beginning of 1993, I had been back with ITV's programme *Disappearing World* to make a documentary. The series had pioneered modern ethnographic films in many parts of the world; in 1992 they had decided on three new films to be screened the following year, not about tribal worlds as such, but about the consequences for what were still thought of as traditional cultural groups who found themselves in war zones (the other films that year were set in northern Burma and Bosnia). Because the transit camp of Karmi was a new, open-ended, informal situation, as a film team we were able to get on with our project without much interference at all (MacDonald 1993). In fact, we ourselves had to act on behalf of the authorities when fighting broke out on a Saturday afternoon. This happened between the Blue Nile people for whom the transit camp had originally been set up and newcomers from the southern Sudan who had later been permitted to settle around its fringes. It began as a minor quarrel between women at the river, but stone-throwing against the new arrivals soon spread. One of their elderly women was quite soon knocked flat by a stone and injured. Rumours spread that she was dead, reaching us in the film team as we were trying to find out what was going on. Men took up their spears and throwing sticks throughout the camp, and fear spread as the women started packing up their possessions ready to leave. No agencies or officials were around, having gone home for the weekend. The only vehicle present at the time was ours (i.e. the television team's). As the sun was going down, we decided that something should be done, so I went with the driver and one refugee leader to report the situation to the UNHCR, the police and the army back in Gambela. Things had calmed down by the time we returned, but the film crew had caught some sensitive footage, including a couple of explosions and at least one fire on the fringes of the camp, which were attributed to guns or grenades brought in by the newcomers. Several injured people, including the first woman who had been severely hurt, were then ferried back to town by the officials. The refugees themselves requested that I should never allow scenes of the fighting to be shown in public. This was also the feeling of the film team, so the resulting documentary was relatively mild in tone – but as a result of our own 'ethical' judgement rather than the kind of top-down decisions that might be made these days. A fuller account of the event, the emotional memories it invoked and its wider significance can be found in James (1997).

6. Bonga: new government, new approaches to refugee settlement

Once the plan for a brand-new refugee settlement site had been decided, at Bonga further upstream from Gambela, officialdom slowly began to assert its presence. Again I provided background reports for UNHCR's Addis Ababa office; then, in 1994, the main Geneva office invited me back to do a 'Progress Report on the Bonga Scheme', which I accepted with backing from Oxford (details of the various reports I wrote during these years can be found in James 2007: 322). It was not easy, mainly because the provisions of the original scheme were to allow the refugees to have access to land and become partly self-sufficient. While this had looked all right from the central government's point of view, it began to look much less attractive to the local population of the Gambela district and to those in authority there. I sent in copies of my report – which did emphasize the original understandings of the scheme as a partially self-supporting one – after leaving the country, thus avoiding any interference in its arguments at the time. I also avoided any direct criticism of the kind I would certainly have faced in writing such a report for the UNHCR and government authorities a few years later, by which time there had been a population explosion of bureaucracies, institutions and officials in Bonga.

7. Officialdom spreads to Bonga

During the 1994 stay, I did meet early representatives of ZOA Refugee Care (the initials mean 'S.E. Asia' in Dutch, referring to the region where they had first worked). They specialized in training refugees in productive skills, such as weaving, ironworking, bee-keeping, basketry and various agricultural skills, all of which would help them settle back in their home countries when the time came for them to return. A few years later, in 2000, I was formally invited to return to Bonga as a consultant for ZOA to produce a report on the community services in the scheme (everybody had now slipped back into calling it a 'camp'). Again, with Oxford's support, I went with the best of intentions. However, I now ran into the modern kind of situation which so many scholars in the aid and humanitarian fields find themselves in.

The situation was that the UN had offered ZOA a budget for the development of 'community services' in Bonga. But I understood that the official government Administration for Refugee and Returnee Affairs (ARRA) was of the view that this budget should have been allocated directly to them, as they had an official presence and range of activities already in Bonga. On my arrival in Addis Ababa, the head of ZOA was called up to a Ministry office, and he took me along. The disagreement over the UN funds was quite evident, and I was asked specifically about my role. I

made it clear that I was not being paid any fee by ZOA for my proposed assistance and that my international travel expenses were being covered by my university. In return for what I could do in helping plan the extension of community services, I would be glad to accept practical assistance with accommodation and local travel in the Gambela region. The top official then looked at me and in a distinctly accusing way said something very close to this: 'Now, Professor Wendy, *you have your own agenda,* don't you?' So I said, more or less, 'Yes, of course I do; those refugees in Bonga, I have known them quite a long time; I know something of their history since they lived in Sudan, and I can speak to them in their own language. If there is anything I can do to help improve social services in Bonga, I am very glad to be of use in this area.' The official finally gave his approval for me to join the ZOA team in Bonga, but said that I could only have permission to go there and do the work if I were accompanied by a minder from ARRA. So that's what happened; a young fellow was appointed as my minder, and we took the plane together down to Gambela. I had in fact met him on my 1994 trip and had found him a bit difficult. But we now had to get on with things together. He stayed in the ARRA compound, and I knew I was expected to stay in the ZOA compound, so I did. At the start we met up each morning and went around the settlement as he introduced me to various people I already knew and explained to them what I would be doing. But after three days or so, he said to me quietly, 'Look, you know, I have a number of things to see to in Addis. I think I shall have to leave soon; do you think you will be able to manage on your own?' I thanked him for his support, said I'd do my best and wished him a safe trip back. The next day I asked the permission of the second-in-command at the ZOA camp (the head person was away for a bit anyway) if I could move out and stay in the camp itself; he had no objection, so I left for a hut vacated for me by a family I had known well since the 1960s and who were probably expecting me.

8. Concluding observations

Because of my extended experience on both sides of the Sudan–Ethiopia border, I was asked to be a 'Resource Person' to speak on issues concerning the Blue Nile in 2003, when the official Sudanese peace talks had moved from Machakos in Kenya to Nairobi. The resulting text of the Comprehensive Peace Agreement (CPA) of 2005 included various quite positive recommendations for post-war demobilization and rehabilitation, but unfortunately these have never been fully implemented.

The Bonga refugee scheme remained in place up to and beyond the CPA. But the return and rehabilitation of the Bonga refugee community to Kurmuk and their homeland within the newly instituted Blue Nile State (the southerly portion of the old Blue Nile Province) was carried out mainly through 2006-7. The CPA included provision for a referendum to be carried out among the people of the southern provinces of Sudan (but not for anyone from the northern provinces, including Blue Nile). The overwhelming vote was for secession by the South, which duly took place on 9th July, 2011. However, provisions for post-war recovery were not fully implemented for the transitional areas of Abyei (as of the time of writing, still waiting for a promised referendum of its own), Blue Nile or the Nuba Mountains in S. Kordofan. These latter two regions in particular were simply treated by Khartoum as fully back under central government control, despite arguments on their behalf that had been made in the course of the peace negotiations. The war has since continued in both regions and has led to fresh massive flights of refugees (James 2015). In the case of Blue Nile, these have been either back yet again to Ethiopia, where at least one of the old camps has been reopened alongside several new ones, or over the brand-new international border with South Sudan, where Doro in Meban country is just one of several brand-new camps (as explained above, the Uduk and their Meban neighbours have much in common, and still get on well). I have not been back to 'the field' since this new period of complete upheaval, but have tried to keep in touch with events from a distance and have had several enlightening meetings with old friends now resettled in the Sudanese diaspora (mainly USA).

As indicated above, I have tried at particular times in the past to co-operate where I could with those who were helping to deal with emergency situations in regions I already knew. It was quite gratifying to me, therefore, to be approached for background and advice by representatives of various aid agencies that have been carrying out recent emergency work in the Sudan–Ethiopia border regions, now complicated further by the secession of South Sudan. The most influential of these have been Médecins Sans Frontières and Amnesty International. I have been extremely fortunate in having had a good deal of individual research freedom, backed by established universities, but also face-to-face encounters with the real world of struggle between the powers-that-be and the world of well-funded organizations. I am very conscious that the rising generation of researchers in anthropology have to cope with a jungle of new regulations that mine knew nothing of, but also very confident that they have the resilience, good sense and imagination to produce illuminating and original work of historical and cultural value that will outlast any

specific tasks they may decide to take on. That surely must remain our basic ambition and 'ethical' obligation.

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THE ETHICS OF RESEARCHING IN CONFLICT:

PERSONAL REFLECTIONS FROM GREATER PIBOR AND SOUTH SUDAN

DIANA FELIX DA COSTA[1]

Introduction

I first went to Boma in southern Pibor in South Sudan in November 2012 on a preliminary visit to determine the site for my doctoral fieldwork. In the hinterlands of South Sudan, previously located in Jonglei state, Boma and Pibor are the home of the Murle, a small agro-pastoralist people that have lived on the margins of the state and experienced difficult relations with their neighbours and the government. In South Sudan, the Murle have been subject to a widespread, politically motivated narrative that demonises them as hostile, violent, infertile and as child abductors. As Anne Laudati argues, 'Despite the reality of a politically and economically marginalized Murle, they are often cast as the aggressors and perpetrators of the continuing insecurity of Jonglei—a narrative that has been upheld by media agencies, prominent figures in government, NGO staff, and local citizens' (Laudati 2011: 21).

I was interested in learning about the social meaning of violence among the Murle, the ways in which it was perpetrated, experienced and lived, and how it was made legible, accounted for and constituted as a central element in Boma Murle representations of collective self and in articulating relations to the state. By the time of my first visit, the conflict between the largely Murle rebellion known as the South Sudan Democratic Movement/Army – Cobra Faction (SSDM/A-CF) and the South Sudan Government's Sudan People's Liberation Army (SPLA) was already affecting much of Pibor county.

The Murle have been part of cycles of inter-communal violence with neighbouring Lou Nuer, and to a lesser extent Dinka Bor, which particularly escalated from 2009 and led to a SPLA-led civilian disarmament campaign in 2012 across Jonglei (Small Arms Survey, 2012). This campaign started peacefully by engaging local chiefs, but it assumed a

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particularly violent character in Pibor county, where the SPLA reportedly committed rapes, simulated drownings and other grave human rights abuses (Human Rights Watch 2013). These abuses of civilians encouraged many young people to join the largely Murle rebellion led by David Yau Yau, SSDM/A-CF, against the Government's SPLA. As the country fell back into a civil war between the SPLA and the SPLA-In Opposition (SPLA-IO) in December 2013, the government of South Sudan agreed to sign a peace deal with the SSDM/A-CF and give in to its greatest demand, an independent state. A quasi-state was established in May 2014, known as the Greater Pibor Administrative Area (GPAA) (see Todisco 2015).

But Boma town remained a relatively quiet and sleepy place, and the few Murle villages in the Boma Hills even more so. This seeming tranquillity and safety was a significant factor in selecting Boma above other places in Greater Pibor to conduct my doctoral research: I was welcomed by initial contacts and authorities, and equally importantly, I could walk freely between villages without too many security restrictions and *be in the world* (Duffield 2014: 77), rather than remaining secluded behind fortified aid compounds (Duffield 2010).

I therefore moved to Boma in early February 2013, when it was still possible to walk around in relative safety. I began my fieldwork under the assumption that I would spend my time moving between a couple of villages in Upper Boma over the following year. This did not happen, not only because I had not anticipated that the war across Pibor would affect Boma to the extent it did, but also because I assumed that my everyday life as an anthropologist would be static and sedentary. Instead research proved much more mobile and dynamic. Over the following months, the conflict drew closer and the area became more insecure. In addition to the fear of the rebellion and of an increasingly tense and agitated SPLA, sporadic ethnically motivated attacks and violence on the roads became a concern. While this latter type of violence did not target me directly, my Murle research assistant became very exposed, and like many residents of Boma we limited our movements. By early May the conflict had effectively reached Boma, leading to the displacement of most of its Murle population and the looting and destruction of homes, the hospital and schools.

The Murle in Boma had begun leaving the town for the safety of the hills in Upper Boma. As the rebel army was about to take Boma from the SPLA, all I had to do to physically leave Boma was run down the mountain very fast and jump into an NGO-chartered aircraft. All this took was a quick phone call by an NGO representative to my satellite phone warning me that the town was on the verge of being taken by the rebels and that NGOs were evacuating and asking whether I wanted to leave with them. Faced with unpredictable violence, I chose to take up their offer. Within less than two hours I had landed, estranged, in South Sudan's capital city, Juba.

The privilege I possessed as a foreign researcher was never made more crudely clear. It brought to the forefront how challenging it was to remain an 'independent researcher' when I was constantly dependent on the logistical support of aid structures. It also made visible how new technologies have altered how researchers *exist* in the field. As Mark Duffield notes, referring to his fieldwork in Maiurno in Sudan in the 1970s, 'Limited external communication meant local immersion, learning the language, making friends and trusting people' (Duffield 2014: 77). Conversely, I had my own satellite phone and could occasionally access the internet at the INGO compounds. I had privileged access to information about events taking place across South Sudan, and in some cases I was even better informed about significant events relating to the rebellion taking place in Pibor than many of my local informants in Upper Boma with no immediate access to information on events as they unfolded. I was also regularly in touch with my supervisor, family and friends. Inevitably, this affected the extent to which I immersed myself in 'the field.' Nevertheless, echoing Mark Duffield's words above, I was still able to improve my (limited) knowledge of the Murle language (continuing the lessons I had started in Juba), make friends and trust people.

I had begun my relationship in Boma as a doctoral student with no formal affiliations. I established good relations with aid workers at the couple of NGOs operating in Boma and was fortunate to benefit from their friendship and support. When in Boma town I could stay at the NGO compounds and use their facilities such as their offices and internet, and I was informally included in the security plans in case a situation arose requiring sudden evacuation, as indeed happened.

I was suddenly in the capital Juba at a loss over what to do as the conflict unfolded in Boma. A few weeks after the SSDM/A-CF took control of Boma, the SPLA recaptured the area, and in the process the majority of Murle civilians fled. Friends and informants fled south to Eastern Equatoria and across the border to Ethiopia, losing family members, their homes and their belongings. I felt useless; what could I do ethically and practically? Practically, and with a renewed sense of urgency, I continued with my research by following the trajectories of Murle friends and informants into displacement, trying to record what was taking place and how people were making sense of these events and of the new reality. Ethically, what kind of moral obligation and responsibility did I have to use the information I had gathered and my privileged access in a way that could perhaps contribute to alleviating the human suffering I was witnessing and somehow had become part of? In a context of uncertainty, war and human suffering, how could I learn from people in a way that was ethical? Should I continue with my own research by collecting people's narratives of events? And, in a highly polarized political environment, would this mean endangering myself, my work or the very people I hoped to help? What should I do with that information? Could it, and should it, be used in advocacy efforts for the people I was working with? And how could I be sure that it was for their benefit? Would I compromise myself by associating directly with institutional actors? These were all issues and questions acutely in my mind over those initial weeks and months.

The remainder of this article offers a short personal account of how I have navigated some of these ethical questions in relation to my involvement and fieldwork with the Murle in South Sudan and in areas to which many Murle fled to during the period of crisis in recent years. I remain convinced of the potential of ethnography as a valuable approach to learning about societies facing war, not least because of how improvisation and adaptation are central to its *modus operandi*. I will discuss in particular what has come to seem like an inescapable relationship between research in remote and insecure areas and the aid industry and some of the problems this entails.

The value of ethnography as an improvisational practice during crisis

Ethnography is based on long-term and systematic engagement with a group of people and the development of social relationships and emotional bonds, in which 'participant

observation as a methodology blurs into and becomes indistinguishable from living itself' (Duffield 2014: 81). With time, these experiences and relationships – a part of everyday living – result in cultural, social and political insights and knowledge. Much in line with Lisa Malkki, I understand ethnographic fieldwork as 'simultaneously a critical theoretical practice, a quotidian ethical practice, and an improvisational practice' (Malkki 2007: 164), *being in the world* to develop knowledge of it (Duffield 2014; Jenkins 1994).[2] But how possible is it to conduct ethnographic fieldwork in contexts of conflict, disaster and human suffering? And what kinds of specific ethical dilemmas surface in contexts of crisis?

Allaine Cerwonka (2007) speaks of fieldwork and ethnography as a 'nervous condition', and I agree with Malkki when she says that 'there are many improvisational dimensions to knowledge production and writing in general, but for ethnographic research … improvisation is indispensable' (Malkki 2007: 163). Ethnographic research in difficult and unstable contexts intensifies this 'nervous condition' and relies even more on improvising and on ethnographic intuition. Researching, encountering and experiencing human suffering is never going to be an objective and straightforward process. Rather, it is full of uncertainty, intense involvement and self-reflection, as well as permanent ethical reassessment and improvisation.

My research took place in a shifting environment in which I navigated between physical, social and political spaces of instability and constant change, where improvising – theoretically, ethically, emotionally and methodologically – was not a secondary feature, but rather an ever-present, conscious and necessary strategy. I found myself documenting events and informants' narratives and strategies as they occurred. Researching and conducting fieldwork of any social phenomena are inevitably unpredictable and uncertain. This is multiplied many times by situations of violent conflict and instability. Pieke (1995) speaks of what he terms 'accidental anthropology', where he urges anthropologists neither to hold on to the execution of a predetermined research plan, nor to start all over again when encountering unexpected events. For Nordstrom and Robben (1995: 16), accidental anthropology is 'not about emergencies but rather about understanding contingencies in a wider social and cultural context'. I find the principle of 'accidental anthropology' not only

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[2] This sentence alludes to Mark Duffield's critique of current remote research and aid management methodologies, where he states that 'being in the world is no longer a requirement for developing knowledge of it' – a reference to Maybury 2010 (Duffield 2014: 76).

relevant but inevitable if fieldwork is to remain a genuine endeavour and reflective about what is happening in a shifting social world.

Convergence of the researcher and the aid encounter

Mark Duffield has observed and written incisively about the retreat of aid workers into secluded fortified compounds and the growing use of remote methodologies (Duffield 2010; cf. Collinson et al. 2013). More recently, he has also written about the dangers of the convergence between research and aid, arguing that academic 'area studies has collapsed into aid policy' (Duffield 2014: 86).

It is difficult to conduct research in hard-to-reach and insecure areas. The sheer physical isolation of Boma, in addition to an insecure environment and a lack of options in movement, did mean that I was extremely dependent on aid structures, particularly for flying in and out. But having the support of NGOs can be very helpful in this regard, while it still being possible to maintain some independence. In Boma, there was no telephone network, and the only roads connecting the area to Ethiopia and to Kapueta in Eastern Equatoria State are impassable during the eight-month rainy season. But even on a good day in the dry season, it takes roughly three days to drive from Juba to Boma. Prior to the conflict, flying in and out of Boma was only possible through the weekly United Nations Humanitarian Air Service (UNHAS) flight, permitted only for humanitarian staff (during the instability even this more or less regular flight was cancelled). To fly with UNHAS to get in and out of Boma, I had to find an NGO that would agree to sponsor me.[3]

After the conflict, research logistics and access became even harder. Looking to follow the new social and political circumstances of the people from Boma, I travelled to areas of Murle displacement to learn how people were making sense of events and of their new reality. At the same time, however, I also actively searched for ways to return to Boma. Boma became a highly militarised and controlled area, and access was even more challenging. The UN and NGOs were occasionally flying in, and I began to engage formally with some of these agencies in order to access Boma. For the researcher, there are both benefits and risks in associating with an aid agency. The most obvious benefit is

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[3] This was done through informal relations and friendships with aid workers who would assist me in managing the bureaucracy. The NGO would buy the flight from UNHAS, but I would reimburse them. Alternatively, an NGO would fly me in in return for a briefing of the situation.

logistical and at times institutional support to access hard-to-reach areas. I considered these collaborations as opportunistic engagements from both parties, where both the agency and myself had a simultaneous purpose, as well our own individual agendas. There were also serious risks, as I somewhat naively came to learn: one can easily become caught up in the politics of aid.

The politics of information, positionality and competing accounts

In the highly politicised context of South Sudan, how could I protect my informants and myself as a researcher? I was a doctoral student with first-hand knowledge in a world of advocacy NGO-types often operating remotely, and who often felt entitled to information, despite their distance from the field. The compelling moral mission of NGOs may lead them to acquire a sense of entitlement and expectation. This is perhaps grounded in a combination of agendas (for example, competition for donor dollars and advocacy attention), a desire to operate independently and a genuine belief in the morality of their cause.

Just before the conflict reached Boma in May 2013, I published a short paper for a Norwegian peacebuilding institute discussing briefly the context experienced by the Murle in Boma (Felix da Costa 2013). I had intended to use the policy brief to shed light on the specific environment of the Murle in Boma and counter the wider anti-Murle discourse, with internationals in Juba as an audience. On the one hand, I was keen to draw attention to the situation of the Murle in Boma. On the other hand, I feared representing Murle in an unfavourable light that could have negative consequences for the already tense political environment and be used to legitimise anti-Murle rhetoric and policies, particularly in terms of reproducing anti-Murle discourses and contributing to the narratives promoting the division and disunity of the Murle people. I became well aware of the perils of publishing in highly politicised contexts, of how information can be distorted and taken out of context by individuals and how researchers can easily become scapegoats.

In this scenario, choices regarding how to handle and present certain information at both the ethical and emotional levels are difficult to take, particularly when they relate to human rights abuses and political and structural violence. Rather, in such politically charged environments, it may be wiser to resort to silence and self-censorship. Laura

Hammond (2011) notes that silence can and should be read and interpreted as having meaning and significance, both from research informants and researcher alike. Ultimately, given the practical implications of knowledge, Cramer et al. (2011: 17) argue that difficult questions and decisions should be analysed through the following lens:

> 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.

For Nancy Scheper-Hughes (1995: 419), morally engaged research involves being more than a spectator. Rather, it recognizes that what emerges 'ln the act of writing culture (…) is always a highly subjective, partial, and fragmentary but also deeply personal record of human lives based on eye-witness accounts and testimony'. At this level, witnessing 'positions the anthropologist inside human events as a responsive, reflexive, and morally committed being, one who will "take sides" and make judgments, though this flies in the face of the anthropological non-engagement with either ethics or politics' (Nancy Scheper-Hughes, 1995: 419). Scheper-Hughes, then, sees ethics as 'responsibility, accountability and answerability to "the other"', arguing that a politically and morally engaged discipline requires its practitioners to be 'witnesses' instead of 'spectators' (1995: 419). However, this political righteousness suggests that such decisions are straightforward and simple: in reality, they are set in complex and messy political, social and ethical webs. As Adam Kuper suggests, in his response to Scheper-Hughes, 'most ethnographic situations are less dramatic and most political choices more complicated' (Kuper 1995: 425). Rather, Aihwa Ong is right to argue 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: 428). There are scientific expectations that researchers should be neutral and impartial, but like Hutchinson (2011), arguing on the basis of her long-term involvement in South Sudan, I have also found this to be an illusion. Drawing on long-term ethnographic research in India with Maoist insurgency and counterinsurgency groups, George Kunnath questions the meaning of objectivity 'in the face of violence and oppression' (Kunnath 2013: 740). He eloquently states that

> I consider not speaking and not acting against oppressive structural arrangements and power relations unethical. As an anthropologist, I am not advocating the blurring of the boundaries of anthropology and activism, nor am I demarcating them as separate identities. Being an anthropologist among the poor in itself is a call for participation in their struggles. (Kunnath 2013: 742)

Social research is often presented as an objective and sanitised exercise. But more recognition should go into acknowledging the contingent and the failures. Importantly, there should also be greater recognition of the emotional and human aspects of research and the positionality of the researcher.

My research has been interested in how competing 'knowledges' are constituted relationally and structurally. I have found it impossible to create a linear and structured picture and reconstruction of events that have taken place in Boma, as these are contested and subjective. Instead, the various voices, sometimes in contradiction, offer a more complete understanding of social processes in war. It is also difficult to discern what is objectively 'true' and 'false' from what are just rumours, which can nevertheless be equally important. At a workshop at the University of Birmingham in November 2014, one participant made the insightful remark that 'it's rumour when "they" say it, but it's knowledge when "we" say it', astutely alluding to the hierarchy of knowledge, authority and 'expert knowledges', and contested and partial truths.[4] This increases the ethical and moral dilemmas faced by the researcher, who has even greater power and responsibility when telling a story.

Some of the relationships I have built up from Boma have come to challenge 'traditional' understandings of informed consent, as they become long-term relations of friendship. In this sense, I became part of a complex web of relations that was also inevitably political. While long-term relationships and friendships made over the years have evolved in such a way that it does not always make sense to engage in standard informed consent, ethical responsibility lies in being very aware of where to draw the line and discern what is being told as a friend and otherwise as an individual related temporarily to an aid

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[4] Anonymous participant at the workshop entitled 'Knowledge, "information" and conflict: what we know, how we know it and what it means', 7 November 2014, organised by the Institute of Advanced Studies, University of Birmingham. The workshop was held under the Chatham House Rule.

agency – and of course, when in doubt, asking, and if need be, ultimately censoring oneself. This may not always be easy and straightforward, yet ethnography is always an improvisational and intuitive process.

My emotional attachment to Boma also grew after its destruction in May 2013. My trips to areas of Murle displacement strengthened relations that had begun months earlier in Boma. I would also often carry news from place to place. I became involved in some of the peace meetings, either by being part of writing the funding proposal for donors alongside Murle intellectuals, or by documenting the meeting as part of the secretariat team. In turn, I saw these invitations and participation in Murle debates as consent and support to my interest in documenting Murle narratives of events, and as informal collective approval to continue my research on the part of the broader Murle 'community'.

Conclusion

In May 2015 I returned from just over a month in Pibor, where I was hired by an NGO to learn about the effects of conflict on the Murle's relationship with cattle. The Greater Pibor Administrative Area (GPAA) had been established a year before, in May 2014, with the signing of a peace deal between the Government of South Sudan and the SSDM/A-CF. After over two years of heavy fighting, destruction and displacement, people were returning home, enjoying a refreshing period of peace and some relative stability.

This visit was a delight. Many people who had fled to exile were back. People in towns, in villages and in cattle camps were optimistic about their new political home, the GPAA, which they had fought long and hard for. I had witnessed the Murle collective struggle and the suffering experienced during the war that led to the establishment of the new political entity. I was unequivocally supportive of the GPAA, convinced of its potential to provide a chance for peace in the area.

During the research debriefing, after I enthusiastically shared my research findings with a small audience composed of the NGO staff, I was jokingly asked by the NGO's country director if I had been nominated as an ambassador for the GPAA. The friendly remark brought to the forefront issues I had often asked myself. On the one hand, it raised awkward feelings of failing to adhere to the supposed principles of research detachment and of the neutral observer. It questioned my research objectivity and my ability to offer an unbiased

picture. On the other hand, the remark recognised where I stood – that my research was positioned and my knowledge situated and anchored in Murle aspirations and perspectives in which the establishment of the GPAA was seen as a recognition of the Murle struggles and as a way in which they could govern themselves. I do not see that it is possible to do research in any other way: recognising the researcher's intellectual and emotional subjectivity and partiality, while striving to tell a story as rigorously as possible.

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THE INNER WORKINGS OF AN ETHICS REVIEW BOARD FOR SOCIAL SCIENCE
RESEARCH: REFLECTIONS ON RESEARCH IN DIFFICULT CONTEXTS

KAREN LOWTON[1]

Introduction

The purpose of an ethics review board (ERB) is to uphold high standards in the conduct of research and ultimately to protect research participants from harm, principally through the process of informed consent, as well as the communities to which they belong. However, the ERB also assesses the possibility of harm to the research team and, if the ERB is 'in house', the possible risk to the institution's reputation and future research activities. The potential for risk and harm runs all the way through the research process, from finding a funder for the study and agreeing the research questions to the dissemination of findings and follow-up studies.

This article considers the position of the ERB in assessing social-science research applications that are deemed to be high risk, including those conducted in situations of conflict. I write from the perspective of both ERB applicant and ERB member, having submitted for ethical approval several studies deemed to be high risk, as well as having served on a university board for six years. In this article I outline how ERBs have evolved to encompass approval of social-science research and, as a result, how 'vulnerable' people have become one focus of their review of applications before offering some tips for how organisations can strengthen ethical oversight of the research they conduct.

The foundations of ethics review

In the context of research with human participants, four pillars support the ethical review process; autonomy, justice, beneficence and non-maleficence (Beauchamp and Childress 2001). These pillars are grounded in a biomedical research paradigm, itself developed through ethical guidelines and standards, including the Nuremberg Code, written in 1947 (BMJ 1996), the Helsinki Declaration (WMA 1964) and the Belmont Report (National Commission 1979). Although first developed in biomedical research, the ERB model of reviewing biomedical

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applications has slowly evolved to include review of social-science research. Although all disciplines have professional and/or ethical codes of practice to which their members must adhere, these do not always relate to what Guillemin and Gillam (2004) call 'procedural ethics', as distinct from 'ethics in practice', for social science researchers. Oversight at a level above a professional body can therefore allow for assessment of risk and harm 'in practice' for each study proposed. For example, Médecins Sans Frontières' ethics code followed the same scientific pathway until less than a decade ago, at first not considering qualitative work to fall under its remit (Ford et al. 2009; Schopper et al. 2009). This may be due to the relatively small proportion of qualitative versus quantitative research undertaken by the organisation until very recently, or to a view that qualitative research was in some way not as 'scientific' as that conducted using quantitative methods.

The transition for ERB members on single-board university panels to include review of social-science research has not been easy, primarily because members were not familiar with the methods used by social-science researchers and so do not always understand social-science methods and approaches, but also because ethical difficulties and their remedies are not easily translatable between the 'hard' and 'soft' sciences. For example, it is relatively straightforward to design and approve a coding system to ensure confidentiality for people donating their blood for research and to ensure that participants know exactly what their blood will be tested for, but it is more complex to protect these participants' confidentiality if, for example, they take part in a focus group or to alert them in advance to what the group's disclosures are likely to contain. Prolonged participant observation, of the kind that anthropologists do, further stretches the biomedical model of ethics review, as research boundaries becomes less contained and predicted 'findings' less certain. However, regardless of discipline, well-thought-out ethics review processes should have the same end. Ultimately, both the research team and the ERB are required to think carefully about the nature and extent of harm in each proposed research study and whether the benefits of the research outweigh the harm it might cause. Yet each 'benefit' and 'harm' can be contestable, unpredictable and unknowable.

For both quantitative and qualitative research, the ERB is extremely concerned with the storage, security and handling of data, and alongside ethical issues it will also seek to assure itself that legal obligations will be met. Typical questions that members of the ERB seek to be reassured about include: What will the data be used for? How will it be stored, moved and

accessed in the field? Who will have access to it? Will it be publishable or useable in particular political contexts? Who might learn what about the participants' world? In the context of social-science research, ERBs are also keen to understand what researchers would do with data that exposes illegal acts, poor organisational practices or abuses of human rights. Here, as with biomedical research, clear strategies to ensure security and confidentiality of data must be devised, for example, by using codes for participants, or writing notes instead of recording speech. However, there is no 'one solution fits all' for ethical review.

ERBs and social-science research

Social-science research aims to understand a situation more fully through the collection and interpretation of rich data (Bryman 2012). Understanding an often complex issue using data analysis is the first step in being able to explain and then influence undesirable situations, for example, by recommending policy changes or disseminating new knowledge. In all the applications it reviews, an ERB is looking for an answer to the question 'so what?' No matter how small the study or 'answer' might be, why does it matter that the proposed research will be conducted in a particular setting, with a particular group of people, at a particular time? It is notable that, within the ERB I've been a member of, there have been a number of discussions around whether the 'science' should be reviewed by the ERB as much as the ethical issues raised by doing it. This is not a debate around whether the physical sciences are more ethical or more important than the social sciences or whether one discipline's method is more robust than another's, but what constitutes 'bad science' and whether bad science is always unethical. The general standpoint after these discussions is that conducting bad science in whatever discipline is unethical. Therefore, if the design or method is clearly unlikely to work in terms of practicalities, obtaining rich data, obtaining enough data or learning something new, the application is unlikely to succeed without further justification.

In universities it is academics from the physical and social sciences who sit on an ERB, together with a small number of lay members, whose positions are advertised widely and are drawn from roles outside the organisation. However, lay members may have much in common with the organisation's members. For example, a survey conducted with lay members of the British National Health Service ERB found that they are likely to be older and educated to degree level (Simons et al. 2009). As a social scientist sitting on one university board for six

years, I do believe that review is required in all settings to try to limit the likelihood of harm occurring. However, I also acknowledge that, in the UK at least, the bureaucracy of ethics review and the charge of 'ethics creep' (Haggerty 2004) has led some academics to decide not to conduct the projects they believe in passionately, since they perceive that approval will not be forthcoming or be too time-consuming to obtain. This has meant that those outside the ethics review system, such as journalists using covert observational methods, may be the ones who uncover and expose harmful situations, such as the cases of abuse recently investigated in older people's care homes in England (BBC 2014). Although the ERB may not initially appear open to research designs that are flexible and adaptable, involve people who may be vulnerable or appear to the research team to be 'high risk', applicants should be able to justify to the Board why these research designs are necessary and what will be achieved through them. This involves giving the ERB as much information as possible about the planned research to help it make a decision, thus allowing those members who are unfamiliar with the context and approach of the research to make as fully informed a decision possible.

Conducting research with vulnerable people

From my own applications and from being an ERB member, I have had to counter assumptions that, for example, all older people, dying people or people in care homes are vulnerable, and that vulnerable people should not be approached for research participation, especially when that research involves the investigation of issues that are deemed sensitive. The concept of vulnerability and protection is central to ERBs, yet much discussion takes place within meetings about what the boundaries of 'vulnerability' might be in the context of research participants and to what extent people should be protected in the context of the proposed study. Vulnerability can be intrinsic to the individual (e.g. limited cognitive capacity), extrinsic through situational factors that limit freedoms (e.g. being in a refugee camp), or relational, where autonomy is limited by another person (e.g. being a prisoner). Of course individuals may experience multiple vulnerabilities, especially in situations of violence and conflict, with vulnerability being a dynamic concept that is continually evolving over time and that is reflective of social values and beliefs; Delor and Hubert (2000) offer a useful discussion of the heuristic capacity and practical relevance of the concept of vulnerability. In my own research, I've examined how adults with cystic fibrosis who had lived past the current average survival age perceived their health and

risks of treatment; how bereaved parents experienced end-of-life care delivered to an adult child with cystic fibrosis; how schoolteachers have managed suddenly or unexpectedly bereaved students at their school; and how the first UK cohort of childhood liver transplant recipients experienced growing up and growing older. I believe that the increasing involvement of social scientists on ERBs will, through their experience in conducting research in these areas, help problematize and resolve the assumptions of vulnerability and sensitivity, provoking change in research ethics protocols and processes.

As an applicant, I've come to understand that submitting a detailed application to an ERB allows me not only to think through the potential ethical issues around my research, but also to consider the practical ones. For example, how exactly will potential participants be able to learn about the research I want to conduct? How will they be contacted and invited to take part, and by whom? Can the study I propose really not be carried out in any other context or setting, or with less vulnerable participants? In the context of emergency, conflict and post-conflict research, does the research study have to be conducted at a time when the population has been displaced or is under extraordinary stress, for example? It is not hard to argue that most ethical issues that arise in this type of research are also found in more 'routine' social-science research in the west; however, I suggest that in the former case more ethical issues may come together in a single project than in the latter case. Thus, the likelihood and extent of risk and harm that could arise is greater, the context in which the research is to be conducted is more politically fragile and lacking in infrastructure and human resources (Ford et al. 2009: 1) and the respondents are likely to have more acute or immediate needs. This potentially raises many more dilemmas for the research team and makes it more challenging to find the 'right' solution to ethical and practical issues.

The justice principle of research ethics involves the ability to bear burdens and the appropriateness of placing an extra duty on people who are already carrying a heavy load (see Belmont Report: National Commission 1979), not only in their research participation, but also in the context of the publication of the findings. Research findings should enable dissemination and make it possible for the subject population to benefit, although not necessarily the research participants themselves. A key ethical issue for ERB members in this context is that of vulnerability, whether stemming from a personal characteristic, a behaviour, a situation or a wider environment, or an interplay of any of these. For example, in research in emergency or

post-conflict settings, the population might be mobile or migrant and have new language and/or literacy needs. The aim of the ERB is to protect and to prevent the participants or research team from becoming more vulnerable than they already are, or at least to ensure they understand the potential risks and consequences of the situation. Usually the ERB would look for evidence of support structures (e.g. psychological support) for participants during or after their research participation. This means that the research team must think ahead as far as possible about what might be the potential risks and harms arising from the study and how these can be avoided or mitigated. In this way, as noted above, there is in principle little difference between reviewing ethically a research proposal situated in an emergency context and reviewing one that is closer to home, although in practice they are far apart: a sensitive environment and potentially vulnerable communities 'heighten and amplify the ethical challenges faced by all researchers' (Goodhand 2000: 15). The research team can also be more vulnerable in emergency or (post-) conflict settings. ERB members would look to assure themselves that the applicants had a track record of research and relevant experience in their area or an experienced supervisor who was able to advise them, and that reasonable plans to ensure the safety of both the participants and the team had been made.

Gaining informed consent

It is crucial to have participants' informed consent in research of this type, but of course there are problems around what constitutes being fully 'informed' on the part of both the research team and participants, who consents and how, and to what (Corrigan 2003). One of the most difficult research approaches to have approved by a university ERB is covert research, except in psychological research, where for participants to learn the true purpose of the research may spoil the very thing that the researchers are trying to capture. In these kinds of psychological research, the participants must be debriefed after their participation if ethics approval is to be given. At some ERB meetings in the UK, members have expressed difficulties in understanding how informed potential participants can or should be, both generally in the context of qualitative research and more specifically in the context of participant observation – which some ERB members are liable to see as a type of covert observation. Anthropologists may seek to gain informed consent from an entire town or community as a more practical approach (Schopper et al. 2009), yet ERB members sometimes have difficulty in approving studies in more bounded

settings such as a hospital, where both patients and staff are the focus of the research. For example, how can an unconscious and perhaps unidentified patient consent to become part of the proposed research? Although I believe this difficulty has lessened over the past few years for many ERBs, with more board members being aware of the issues and open to the research team's local solutions to such problems, more progress still needs to be made in this area.

In the context of consent, the power relationship is a crucial element for both ERBs and applicants to consider, alongside local issues of language, culture, traditions and social norms. I believe that illiteracy and the potential mistrust of those who are perceived to be 'in authority' by local communities are issues that are now more widely recognised by ERB members. We are more flexible, I would argue, with participants giving limits to their consent in both contributing their data and in how widely and in what format that data can be disseminated. This does not mean, however, that researchers do not have to think about what they tell potential participants. There is most likely a greater degree of mistrust among communities caught up in emergency and aid situations. Being as detailed as possible as to what might happen, what you believe will happen and what you will do when things go wrong, in a language and style that participants will understand, is crucial. A key danger here is that of inadvertently misleading participants into thinking that their situation will soon change for the better because of the research. This can not only bias the research, but turn participants against it, as well as future researchers. One must be clear about the boundary between the care or aid participants might receive and the research. Significantly, this confusion is also a problem that occurs frequently for health researchers at universities who work closely with clinical staff, with any care benefits for participants needing to be clearly separated from their research participation.

Conclusion

To conclude, I offer ten insights from my experiences of being on both sides of ERB review for the benefit of researchers and organisations undertaking fieldwork in complex emergency settings:

1. Staff working in organisations concerned with providing aid or emergency relief might work towards drafting specific and nuanced local guidance for conducting social-science research in these situations and with specific countries or populations. These organisations have real-world

experience of putting people into the field and gaining information from local populations that the organisation can draw on and that ERB members could learn from. Aid and emergency relief staff might work with academics to develop knowledge and understanding of ethical research conduct in this specific area, both in conflict and post-conflict situations.

2. In the context of research ethics approval, organisations might construct working definitions of what constitutes research and what does not (e.g. service development or audit). Consider what activities or foci differentiate these activities and where ethics review would be needed within or outside the organisation. If your organisation does not have an ERB, think about building one within the organisation or joining an existing one that regularly reviews research in your field.

3. If you are working in an organisation that does this type of research regularly, construct an organisational code of research conduct or framework that all researchers will adhere to in specific research situations. This could be taken from the British Sociology Association's (BSA, 2002) or Association of Social Anthropologists' (ASA, 2011) codes of ethical research conduct, for example, and worked into an overview of the organisational position and response in different cultural contexts and emergency scenarios. For example, what is the organisation's stance on disclosing human rights abuses that the research might uncover?

4. If there is no time to put in a full ethics application for research in an emergency context, think about whether the researchers could confirm that they will adhere to the organisation's agreed code. Alternatively, could the organisation put in place an internal expedited review structure? Some university ERBs now stream applications into low or minimal risk and high risk categories and review these applications proportionately. Médecins Sans Frontières, for example, has put in place a retrospective review process (Schopper et al. 2009) for situations when time is pressing and the research project would risk failure if a longer review process were called for. Ultimately, the ERB system needs to be as flexible and responsive as the applicant's proposed research design, yet robust enough to maintain the highest standards of ethical research.

5. Plan as far as possible before the research starts. Think of a realistic Plan B and Plan C in case the ERB or local situation prevents the research team from following its proposed protocol. Alternatively, could the proposed project be split into discrete stages of research and therefore discrete stages of ethics approval?

6. As a social scientist working in the area of emergency or difficult situations, put yourself forward as a member of your institutional ERB or research ethics working groups so that you can inform and influence the process from within. There is still work to be done to separate institutional bureaucracy from fundamental ethical issues and high standards of ethical conduct in order to approve research that will make a difference to local communities.

7. When applying to ERBs that are likely to be unfamiliar with research in emergency or aid situations, be as detailed as you can in explaining your approach or responding to the ERB's questions on your applications. For example, why can't you collect participants' signatures indicating their informed consent, and how will you ensure that consent is given and will be as fully informed as it can be at that time? This will enable ERB members to understand more fully the context in which you are working and to highlight issues it thinks you have not considered, as well as enabling you to show your competence in your research planning. Aim to show that you do understand the inherent risks and how to mitigate them against harm, rather than trying to argue that the risks will not appear in your project.

8. Most ERBs in the UK will seek assurance that ethics approval has also been given by the host country or host institution, or confirmation by the research team that there is no organisation that can give this. Building relationships with overseas hosts before submitting an ERB application is likely to increase the chances that local ethical issues come to light quickly and can be planned for in the research approach.

9. Think about whether the research team will include people from the local population or be recruited solely from your organisation. If the latter, would your research be more fruitfully conducted with local people on board as researchers or project managers, for example? Whether

from the organisation or local context, train those conducting the research to increase their ability to use good judgement about more abstract organisational rules (see Wood 2006: 374).

10. All researchers need to leave the research site in a condition in which future researchers would be welcomed by its community. Think carefully about what training you provide for new or inexperienced researchers, from both the academic/research and emergency/conflict perspectives, and in both research procedures and ethical conduct. Ensure that experienced researchers are able to pass on their wisdom in this area to ensure that local or specialist knowledge is built upon for future research studies.

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SUBCONTRACTING ETHICS:

MEDIATING THE COMMODIFICATION OF

'LOCAL KNOWLEDGE' IN CRISIS AND CONFLICT

MELYN MCKAY AND ALISSA DE CARBONNEL[1]

Introduction

In his recent contribution to the *Forum for Development Studies*, David Mosse writes

that, in his decades of experience conducting anthropological research for the Department

for International Development, participatory techniques used to elicit '*local* knowledge

and *local* choices for *local* control' were the methodologies most valued by beneficiary

communities (Mosse 2014: 516). Mosse's emphasis on the importance of local

knowledge production and local participation in development decision-making echoes

almost exactly the language adopted by nearly every major donor, non-governmental

organization (NGO) and aid implementation agency working in 'big D' development

today (Hart 2001: 650).[2]

The drive toward community-informed or community-driven development in part

reflects the impact of anthropology's sustained critique of the development industry. In

fervently advocating the inclusion of local ways of knowing, however, we have

obfuscated the 'fragmentation of ethical views' within our own discipline (Meskell and

Pels 2005: 2), while simultaneously and often unconsciously instrumentalising and

objectifying 'local knowledge' to the detriment of 'local knowledge producers' – the

local people implicated in the neo-liberal economy of international development by virtue

of their geographical, economic, or otherwise marginalized status vis-à-vis so-called

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[2] Whereas 'development' (little d) generally refers to broad processes of change, particularly under
capitalism, 'Development' (big D) refers to specific, intentional interventions led by international actors
with the aim of achieving 'progress', generally within a largely Western, neo-liberal framework (Chant and
McIlwaine 2009).

'developed' states. As more international anthropologists are brought in at the highest levels to advise on complex interventions, local researchers subcontracted to collect or produce 'on-the-ground' data are often constrained by pricing and delivery timelines, while scant attention is paid to building their skills or offering them psychological or risk-reduction support.

When, as is increasingly happening, development actors become involved in war-torn countries or 'fragile and conflict-affected states' (FCAS)**,** an unwillingness within the discipline of anthropology to lead on the articulation of a coherent code of ethics ignores our obligations to the production and producers of 'local knowledge'. Ethical considerations that are central to anthropology – representation of subjects, researcher–subject relationships, the complexities of consent, etc. – are subsumed by the concerns of the procurement departments of major aid donors like the World Bank and the United States Agency for International Development (USAID). These departments, and their bureaucratic contracting protocols, are insufficiently able to account for the complexities of anthropological research.

In the following pages, we will trace the origins of this particular blind spot in anthropology before outlining three trends that are shaping the production and consumption of localized knowledge as a means of exploring the ethical quagmire of development aid research today: (1) the rising popularity of evidence-based programming; (2) the near-universal push toward adherence to value for money (VfM) principles; and (3) the increasing reliance on 'remote management' as a means of transferring rather than confronting risk.[3] We will argue that these three trends contribute to an aid economy that simultaneously fetishizes local knowledge and subcontracts risk down to its most precarious producers – local researchers.

We conclude that, if anthropologists are to continue to advocate better understandings of local dynamics and greater respect for local values and epistemologies, then they must, as Meskell and Pels suggest, be prepared to 'facilitate the negotiation of expertise within as well as outside of the profession' (Meskell and Pels 2005: 3). Having

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[3] Remote management is an approach that removes non-local staff from immediate physical danger whilst retaining international and/or national staff hierarchies within program management structures (Collinson et al. 2013).

contributed to the growing demand for local 'ways of knowing' (Harris 2007: 153) within the development industry, anthropologists must be prepared to engage with the system and advocate on behalf of those who produce it.

Subcontracting Ethics

Two Anthropologies

It is important to understand why there exists little theoretically-engaged ethical guidance for anthropologists involved in 'big D' development. In part, the lack of clarity is a symptom of the enduring science/morality dichotomy (Meskell amd Pels 2005), one with deep roots within the discipline best illustrated by the very public and conceptually muddy 'intellectual joust' between D'Andrade and Scheper-Hughes in the mid-1990s (see Fassin 2008: 333).

As anthropology cast itself as a discipline of counter-hegemonic inquiry in the post-stucturalist period (Sahlins 1996: 16), a lively debate over the 'situatedness and partiality of all claims to knowledge' gathered steam (Marcus 1998: 198). At the same time, however, the 1990s also saw a peak in what had been two decades of uprisings of marginalized indigenous peoples in South America, as well as the anti-apartheid struggle in South Africa (Nash 2007). Inspired by this critical moment in history, Scheper-Hughes called for a 'militant' anthropology, arguing that anthropologists must be at once 'anthropologists, comrades, and companheiras' (Scheper-Hughes 1995: 420). Those who have embraced this perspective, whether wholly or in part, go by many labels, most frequently 'engaged', 'activist' and 'applied'. Though most move between these mantles situationally, all share an expressed desire to be politically and morally involved in the lives of their subjects – an aim Scheper-Hughes argues is what it means for anthropology to be 'ethically grounded' (ibid.: 410). D'Andrade, on the other hand, fought the move toward what he termed 'moral models in anthropology' and called for anthropological objectivity, bemoaning what he believed to be growing support for the idea that 'the moral agenda of anthropology should take priority over the scientific agenda' (D'Andrade 1995: 408), despite, he argued, moral models being themselves ethnocentric. For D'Andrade, anthropology's claim to moral authority rested on 'knowing *empirical* truths about the world' (ibid.).

Interestingly, as Fredrik Barth pointed out, both D'Andrade and Scheper-Hughes attacked relativism, although for opposite reasons (Barth 2005). The debate, then, was not so much a disagreement over the degree to which anthropology was or was not an inherently moral undertaking; rather, the feud hinged on a fundamental disagreement over when the anthropologist should engage in 'creative ethical work' (Robbins 2012).[4] Entangled as these two anthropologies are, dialogue between them is often poor except for moments of existential panic, such as those wrought by infelicitous flirtations with *actual* militant anthropology.[5] The result has been a gradual cordoning off of theoretical anthropology from the pragmatic concerns of development, which has in turn impoverished both the theoretical and methodological strengths of anything we might call 'development anthropology'.

Development Anthropology and the Anthropology of Development

Many anthropologists, the present authors included, do not believe that 'development anthropology' constitutes its own discrete sub-discipline, or more importantly, that it *should*; these remain, after all, fairly porous and arbitrary groupings. However, the various monikers adopted under the mantle of 'engaged anthropology' can be helpful in distinguishing certain relational characteristics that are brought to bear on the positionality of anthropologists within what we might call various 'knowledge ecosystems':

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[4] Robbins uses this phrase in a discussion of Faubion and Zignon's various attempts to distinguish between unreflexive and reflexive moral reasoning in times of ethical change or crisis. 'Creative ethical work' is that by which a subject seeks to draw together discrete elements of moral code to create a new means of behaving ethically in new or otherwise previously unencountered situations.

[5] The use of anthropologists in the US military, for instance, has been condemned by the American Anthropological Association (AAA). Human Terrain Systems (HTS), as they are often known, claim to use anthropological knowledge to mitigate civilian and troop casualties in active conflict.
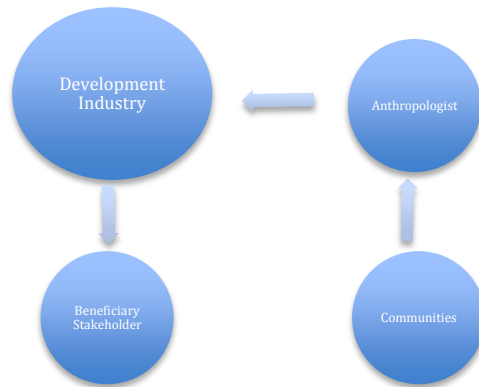
Anthropology of Development (critical)

Figure 1. The (Critical) Anthropology of Development

The 'anthropology of development' seeks to examine the assumptions and practices that are core to the development industry. It has thus far been characterized by post-structuralist criticism, namely the linking of 'big D' Development with neoliberal agendas and Euro-American socio-cultural dominance. Within this model, the anthropologist understands herself as an external objective voice. Criticisms of specific interventions, such as the IMF-led restructuring of numerous African economies, are generally derived from insights gleaned through extended fieldwork with so-called 'beneficiaries'. 'Local knowledge' in this model is presented as what is produced by and/or channelled through anthropologists. It is therefore not structurally incorporated within the development industry, but rather externally located and portrayed as post-action 'objective' critique.

Within this model, the anthropologist's ethical responsibility is conceived of in nebulous terms and translates into a desire to uncover the forces of coercion and sources of inequality within the development system as a whole. Anthropologists may or may not understand themselves as having an ethical responsibility to individual communities, even when those communities have contributed to the development of the empirical claims presented.
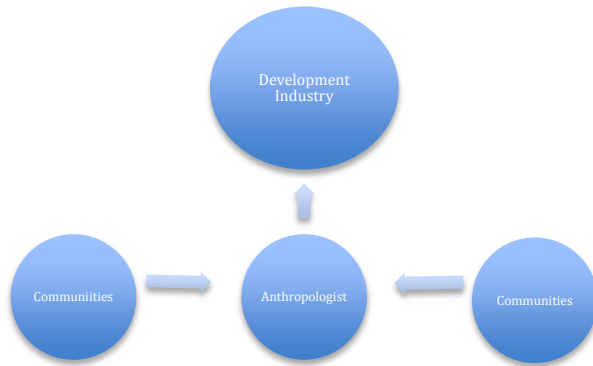
Activist Anthropology

Figure 2. Activist Anthropology

Activist anthropology, by comparison, tends to position the anthropologist as a comrade-in-arms with beneficiary stakeholders. 'Local knowledge' is co-produced but made visible by virtue of the anthropologist's willingness to engage with the development industry or, as the case may be, the government, military or corporate sector. The anthropologist's ethical concern lies firmly with the beneficiary stakeholders. This, for instance, represents the anthropologist's idealized positionality in Scheper Hughes's meaning.
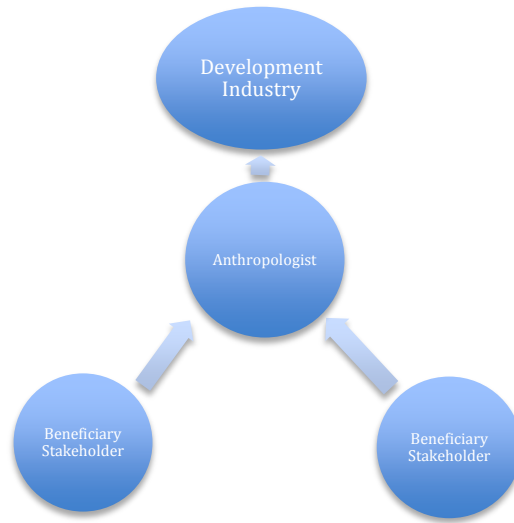
Development Anthropology

Figure 3. Development Anthropology

Development anthropology is a hybrid of these two forms. Anthropologists collect 'local knowledge' from beneficiary stakeholders in order to strengthen their empirical evaluation of specific development interventions. Anthropologists are expected to produce 'local knowledge' of specific interest to a predefined aid intervention, often with the aid of local research assistants. Within this arrangement, anthropologists 'serve as cultural intermediaries or "brokers" between the worlds of development and community; collecting the local knowledge and point of view; placing local communities and projects in larger contexts of political economy; and viewing culture holistically' (Wulff and Fiske 1987: 10). Where the ethical responsibility lies here is somewhat more ambiguous. While it remains the case that anthropologists feel a sense of moral duty toward the communities they study, they are *contractually obliged* to adhere to the ethical codes laid down by whichever development body hires them. Although in some cases these codes are well defined, rarely are they based on a deep theoretical engagement or designed with the particular concerns of anthropology in mind.

Commoditizing 'Local Knowledge'

In this section, we explore three trends that bring to the fore the precarious position of development anthropology when considered through an ethically engaged lens that assumes multiple and at times conflicting moral responsibilities.

Evidence-Based Programming

Over the course of the last decade, big 'D' Development has been characterized by a surge in large-scale interventions in crisis and conflict contexts, or Fragile and Conflict Affected States (FCAS), which the Organisation for Economic Co-operation and Development (OECD) estimates will be home to over sixty percent of the world's poor by 2030 (OECD 2015: 1). The growing emphasis today on 'evidence-based programming' by major aid donors is in part a response to the poor record of interventions around the world, notably in the Middle East and Africa.

Previous failures have been widely blamed on the lack of a solid understanding of the complex realities on the ground. The UK's Department for International Development (DfID) appointed its first Chief Scientific Advisor in 2004 in response to a damning report by the parliamentary Science and Technology Select Committee, which said that DfID's lack of attention to evidence and attention to developing country inputs made a mockery of its claim that is follows a demand-led approach (House of Commons 2012). Similar moves on the part of other major bi- and multilateral agencies have fuelled a renewed commitment by donors to research (DfID Research Strategy 2008-2013). Beyond the rhetoric, the elevation of the evidence-based agenda is backed by funds. Spending on research by DfID's Research and Evidence Division (RED), tasked with generating knowledge and its uptake into policy, grew almost 150 percent from £125 million in 2008-09 to £320 million in 2014-15 (National Audit Office 2011: 46). Overall, the UK agency allocated at least £1.2 billion for research, evaluation and personnel development from 2011 to 2015 with the explicit aim of 'improving' the impact of interventions (ICAI 2014: 1).

The drive toward evidence-based programming is at least in part a response to academic and activist criticisms of perceived failures to tailor aid effectively to complex local dynamics and calling for greater and better inclusion of 'local knowledge'. It

demands an answer to who is best placed to provide this contextual input. External researchers, some of whom are anthropologists, lay claim to professional qualification and impartiality, but few would deny that local actors hold legitimacy as an indispensable repository of knowledge about the local context. The Independent Research Forum argued in its February 2014 brief that engaging local researchers in what it calls 'bottom-up participatory learning' is essential to realising the post-2015 Sustainable Development Goals (Independent Research Forum 2015). The importance of moving from effective donorship to effective partnership has been highlighted time and again in high-profile summits such as the 2005 Paris Declaration on Aid Effectiveness, the 2008 Accra Agenda for Action and the Busan 2011 Global Partnership for Effective Development Cooperation (OECD-DAC 2005). Although major donors hold partnership up as the standard, it is rarely achieved in practice. Despite the professed desirability of local knowledge, a host of factors conspire to put local researchers and local research consultancies at a disadvantage compared to external actors. Anthropologists and other social scientists continue to act as intercultural go-betweens tasked with transforming impossibly complex social dynamics into data points for decision-makers.

The kinds of development projects that are generally accompanied by significant research budgets are large, multi-year programs delivered by consortiums, which are encouraged, if not required, to include local partners. However, these partners are often partners in name only – they don't deliver substantive elements of the program but rather are subcontracted by larger international firms within the consortium, many of whom are for-profit entities.[6] Cost structures and the use of national employees differ widely across contracts and firms, but stark disparities in pay scales between international and local staff is a common thread. Despite being hired primarily for their greater knowledge of the local context, local partners are often tasked with completing the 'lowest value' work, their work being devalued both in terms of overall cost and cost in comparison to degree of effort, discomfort or risk. It is common practice for surveys to be designed and analysed by an international consultant or consultancy, but for the data to be collected by a local consultancy or a collection of enumerators and presented to the donor by the

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[6] Interviews with members of DfID's RED team and veteran development-sector consultants in August 2015 in London, UK.

heads of consortium. Although it is often precisely for their local relationship that international implementers win bids, local consultancies and local researchers are rarely placed in a position to contribute additional value beyond the fact of their simply *being* local.

The dual salary scale and its effect on the ways in which 'local knowledge' is both produced and consumed is not only a normative problem but one that may have a knock-on effect on the quality of results achieved during development or humanitarian interventions. Research in the field of international human resource management suggests that peer-to-peer relationships and skills training are often undermined by the feelings of superiority, guilt, demotivation or distrust it may engender (McWha et al. 2011: 29-40, 2011: 29-40; Dudley 2003). Studies suggest that funding for research in low-income countries does not necessarily lead to a more highly skilled talent pool if it is unaccompanied by capacity-building efforts (OECD-DAC 2008).

To some critics of the development paradigm, the very idea of 'local knowledge' may be seen as the invention of well-intentioned anthropologists, development workers, colonialists and various western 'others' or observers (Said 1978: 1-4; Ellen, commentary on Sillitoe, 1998: 238). Those most able to move comfortably between various forms of knowing are placed in positions of authority and expected to synthesise complex local knowledge into targeted sound bites (Mosse 2005). As a result, strategic decisions that are meant to be informed by 'local knowledge' are rarely taken in the company of local researchers or communities, but rather elevate foreign anthropologists or international researchers to the role of community or 'cultural' spokespersons. The multiple layers of subcontracting, as shown in Figure 4, exacerbate the gulf in the field between donors and locals hired to deliver contracts, whether for research or program activities. Britain's Independent Commission for Aid Impact (ICAI) has described DfID's relationship with its local suppliers as 'too arm's length', saying it 'limited their early involvement in shaping programs' and their ability to 'foster genuinely productive relationships' (ICAI 2010: 10). As such, local researchers have few potential routes by which to contribute to programming, despite often being sought out precisely for their insight into the local context. Anecdotal evidence and research highlight how rare it is for agencies to draw on this indigenous knowledge in any systematic way (Eyben et al. 2015: 10). Despite lofty

principles, locals are more often service providers than partners.

Figure 4. Simplified version of a standard research contracting process. Adapted from Balt et al. (2015: 11).

Value for Money

The growing pot of money for research brings challenges alongside opportunities. The goals set out in the 'terms of reference' of tenders for development projects are often undercut by the rigid and demanding procurement process in an increasingly competitive and commercialised aid market (Bilzen 2015). DfID spent nine per cent of its aid through private-sector partners in 2011-12 (ICAI 2010: 31). It has been increasing its use of commercial contractors since then, according to the OECD's 2014 peer review (OECD 2014: 21), particularly in conflict settings, where staffing is difficult.

Under scrutiny to ensure that taxpayer's money is well-spent, major international donors such as DfID prioritise value for money (VfM). They seek quality guarantees that are often best met by tried and tested consulting firms, which restricts the space for new

entrants and smaller players, particularly national organisations or loose associations of local consultants. In the scramble for funding, bids to win contracts for development projects often come from known applicants who understand and can tailor their proposals to the donor's agenda. A dozen companies maintain a hold on the majority of contracts, with a tail of smaller niche consultancies bringing up the rear (ICAI 2010: 4). Although UK aid has been fully untied since 2001, the vast majority of contracts – 90 per cent of centrally managed ones – are awarded to British suppliers (OECD: 21). DfID own data show that only one of the top twenty firms managing the largest budget projects in 2014 was based in a developing country. Fragile and conflict affected states, by their very definition, are almost entirely absent.

With the increased emphasis on value for money, and by association predictable results, DfID is also looking for firms that offer cheaper services, that is, giving more weight to pricing in tender criteria. Coming under this pressure, some contractors complained to ICAI they were no longer able to put their best staff forward for jobs (ICAI 2010: 17). Cost-cutting measures can have serious repercussions down the procurement chain by undercutting margins for subcontractors, which are often local consulting firms and researchers.

The political imperatives to meet spending targets, keep costs low and achieve short-term results also shrink the space for untested, innovative approaches. The standard method requires a DfID program team to draft a business case, based on a 'theory of change', describing how the policy intervention will achieve the desired goals. It is typically drawn from a mix of evidence and assumptions that are hard pressed to capture complex political and fluid dynamics. Some recent studies suggest these models may stifle opportunities for more iterative, bottom-up learning (Booth and Unsworth 2015: 9).

Value for money has been translated, often literally, as 'technically capable, lowest cost'. Similarly, moves to limit opportunities for corruption and cronyism have been applied to these forms of contract, leading to the maxim that researchers should profit as little as possible. The result has often been a codification of 'rates' based on biographical data, which is heavily weighted in favour of previous salary. As a result, local researchers are often locked into below-market compensation, despite the overall importance of the knowledge, experience and data they provide in shaping and implementing policies.

Furthermore, the OECD's 2014 peer review warned that DfID's focus on value for money risked undercutting its perceptiveness and ability to react to changing contexts (OECD 2014: 21).

As local researchers and consultancies come to be increasingly viewed as necessary but inefficient, the drive toward value for money seems to encourage procurement processes that limit profit and overheads so severely that most local researchers and consultants in conflict-affected environments struggle to provide for themselves and their families, let alone grow their business, develop new skills or bring on additional staff.

Remote Management

In conflict settings, where contracting out aid delivery and research is a means of managing high levels of risk, private-sector actors often have a greater ability to source and hire local consultants where it would be too costly and risky to attract international staff for long-term assignments (ICAI 2013: 1). When a low capacity for basic services characterizes fragile states, one of the most often cited rationales for not turning to nationals has been the lack capable individuals. Paradoxically, it is precisely in the most violent contexts that development and humanitarian actors rely increasingly on locals drawn from NGOs, universities and diaspora networks for situational understanding. As more international aid is delivered in areas where the security situation is perceived as too risky for expatriates, program implementation, analysis and data collection to track aid effectiveness falls primarily – or in some cases, entirely – on national and local actors (Collinson et al. 2013: 6-7). 'Remote management', or allowing aid agencies to continue assisting civilian populations while removing expatriate staff from danger, is no longer a stopgap measure but has become standard practice in Afghanistan, Iraq, Somalia, Syria and elsewhere (Rivas 2015: 8).

In such challenging environments, normative considerations linked to the production and consumption of 'local knowledge' are difficult to codify in contracts and often take a back seat to the drive for evidence-based programming and value for money. On the one hand, local actors gain leverage through their monopoly on access in contexts where expatriates withdraw into fortified compounds or from the country entirely, as in Syria. However, the balance of power in terms of decision-making and funding clearly remains

in the hands of international actors, who in turn place greater importance on monitoring and evaluation to prevent graft.

It is often assumed that locals who are known to their communities and familiar with local customs face lesser security threats and can help build acceptance in communities in which interventions are taking place.In many contexts, however, association with western aid agencies may put nationals in greater danger. Differences in ethnicity and in religious and social status within the country may also pose as great a security challenge to national staff as being a foreigner (Stoddard et al. 2014; Egeland et al. 2011: 36). In such situations, the misconception that nationals are less in need of security and peer support translates into a lesser duty of care by international organisations (Collinson et al. 2013: 28), thus raising complex ethical issues. Given power inequalities, this is particularly true when nationals are driven to take higher risks because they have few other alternative sources of earning their daily bread (ibid.: 14). A recent survey of nationals engaged in humanitarian work found that most national aid workers believed they were exposed to greater risks than their expatriate counterparts (Egeland et al. 2011: 31).

Rapprochement

Input from local researchers has the potential to offer much-needed contextual analysis in politically challenging and complex environments, as well as access to areas deemed no-go zones for internationals. However, their contracting involves a tangled set of trade-offs and challenges that threaten to undermine both ethical standards and the quality of the research on which sensitive programming is based.

Anthropologists have repeatedly advocated the importance of highly textured, culturally sensitive and locally derived insights produced through long-term engagement with communities – exactly the kind of research that is theoretically most valued by aid interventions. However, anthropologists have also long been wary of how our research is put to use; we should remain vigilant over how we engage with local systems and producers of knowledge. All anthropologists should be concerned with the ways in which our tools and insights may be appropriated. If our true reason for engaging in development in the first place is to critique the systems that have harmed populations, then this is clearly an issue we should pursue.

To do so, we need to rethink the focus of 'research ethics': it is clear that our ability to be ethical is not just a matter of our interactions with the populations we work with, nor even of our representations of those populations. To do so meaningfully, there must be a well-intentioned and sustained détente between engaged, activist or applied anthropologies and anthropologies of development to develop a clear criticism of the way the field of ethics is currently constructed. Lastly, it must be paired with a willingness to engage.

Conclusion

A rapprochement between academic anthropologists, who are critical of international aid and development systems, and engaged anthropologists who work within these systems to improve or mitigate impact would serve to strengthen the anthropological critique of global aid. It would also help address anthropologists' ethical obligations to both the communities with whom they work and the local producers of knowledge, which is often commoditised through procurement processes.

Anthropologists should prepare an ethical framework for the procurement and use of localised research, particularly in uncertain environments. Currently, ethical guidelines for procurement tend to focus on decreasing cronyism and corruption, while research ethics tend to emphasise the treatment of research subjects. There is little if any consideration for the systematic devaluation of 'local knowledge producers'. The ad hoc systems developed under the guise of 'duty of care' often mask risk avoidance, rather than representing a proactive attempt to rectify an unbalanced and unfair system in favour of the researchers who risk their lives in precarious contexts.

In Fassin's words, 'avoiding moral issues may be seen as a moral position as well' (Fassin 2008: 340). If anthropologists continue to call for increased attention to local forms of knowing, as well as for improved appreciation of the value of anthropological tools and analysis, then they must remain simultaneously engaged in understanding and striving to address the supply/demand quandary they have helped create.

As a comment on the positionality of the anthropologist, what we are suggesting is that there is no such thing as an un-engaged anthropology, though perhaps not for the reasons most often suggested. Rather, what we are arguing is that, in criticising the

development industry for its lack of consideration of local perspectives, knowledge, voices, etc., we have all contributed to the fetishisation and resultant commodification of exactly these things. In continuing to produce anthropology that attends to 'local knowledge', our work is just as likely to be appropriated by the development sector as it is to be consumed by fellow academics. Activist, engaged, applied or otherwise, we are each and every one of us liable for the local researchers, and their communities, from whom 'local knowledge' is dangerously extracted in the name of better aid or development delivery.

Given the importance we as anthropologists grant to elevating the voices of populations in which we work, we should advocate on behalf of local researchers. The opportunities this creates for sincere co-authorship demand careful reflection: how do we best do this in practice, particularly where the well-being of local researchers is at stake? Concretely, this means ensuring that local researchers are paid fairly and engaged in meaningful, high-level work and ensuring that local researchers are brought into strategy sessions with donors, both at the point of research design and after analysis. This is not just a question of 'building capacity', but rather one of truly 'engaged accompaniment'.

The codification of ethics, discussed by Meskell and Pels (2005), as well as others in the present collection, is at once seemingly necessary and exceptionally challenging. As the so-called 'ethical turn' has made clear, ethics is not something static, nor is it universal or inherently 'good'; rather, it is anchored in historical and cultural values. We need a theory of ethical engagement that transcends the bounds of our own fieldwork to inform the ways in which institutions, guided by our criticism, set about doing the very thing we've told them they *have* to do if they *want* to be ethical.

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BIG DATA AND ANTHROPOLOGY:

CONCERNS FOR DATA COLLECTION IN A NEW RESEARCH CONTEXT

JUSTIN LANE[1]

Introduction

Traditionally, anthropologists have worked within relatively small groups of individuals (at least relative to the scope of modern big-data analytics). Traditionally, we have known our informants and participants and likely have had some personal relationship or connection with them at some level. Such research has carried with it a practice of protection; anthropologists are keenly aware that we often work in fragile parts of human societies and ask personal questions; therefore we have strived to protect the identities of our informants.

The modern digital environment is one where researchers have access to individuals' data—sometimes deeply personal data—at the touch of a button. Participant anonymity becomes a thorny problem. Given relatively easy access to massive amounts of unique individual data, one can reverse-engineer the data in order to obtain the specific identity of the person, even if their name is changed or erased from that data. In addition, it is often the case that, when a researcher obtains social network data—even when assuming complete consent and legal transfer of the information—information concerning real individuals who have not consented to participate in the research is also transmitted.

This paper argues that we have not given enough thought to such problems as online data becomes of increasing interest to anthropology. I outline some of key issues around data security and big data, and highlight the dilemmas that are likely to confront anthropologists in the near future. My conclusion argues that anthropologists must keep in mind a combination of "traditional" research values as well as the fact that we are in a new frontier of information as we enter the world of "big-data". I finish with some suggestions for participant protection.

Introduction to data security

The digitization of data represents a new horizon for anthropologists, though one that comes with new challenges and ethical questions. Here, I hope to start what I believe is an overdue conversation on the nature of data security in anthropology. It focuses on two aspects of data security: digital data security, and 'big data'. These two aspects of the modern digital world present different problems for anthropology. I begin by outlining the problems generally before discussing some of the initial steps that we can take in order to safeguard the security of our participants and informants, as well as ensure that our research conforms to the most rigorous ethical standards.

Data security can be defined as protection against unwanted or unauthorized access or use of data or of the systems that store and manipulate data. *Digital* data security is the extension of this concern to include digital forms of data such as those stored on a computer or hard drive, or even the data we transmit by phone or email. Securing this data can involve extremely simple physical methods, like locking our hard drives in a drawer, or electronic methods, such as using complex passwords or encrypting our hard drives. Securing this data is important not only because our digital data include intimate details about our own lives, but also because as anthropologists our data include intimate details about the lives of our informants as well. I discuss some basic precautions in greater detail in later sections and offer simple suggestions as to their use and where one might go to learn more information.

Like so many terms in contemporary media, 'big data' is used so frequently that its meaning is becoming lost on many. 'Big data' refers to massive amounts of electronic data that are indexable and searchable by means of computational systems. Generally, such data are stored on servers and analysed by algorithms, since the amount of information to be analysed is too large to be interpreted initially by human coders. 'Big data' is not only a way of describing large electronic datasets, it is also an industry. Massive dot-com companies like Google, Facebook and Twitter, as well as telecommunications companies, are able to study, measure and even buy and sell our data. This has given rise to companies such as Palantir[2] and products such as IBM's 'Watson'[3] that specialize in making sense of big data.

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[2] www.palantir.com
[3] www.ibm.com/smarterplanet/us/en/ibmwatson

Ultimately, big data is human data: it is generated by humans and—key for the discussion at hand—it can be reconstructed to identify those who originally produced the data. Although this topic has been of great importance to contemporary media and political debates, to discuss the use and collection of big data by modern government agencies would go beyond the scope of the current article, even though it is conceivable that this too may impact on the anthropologist's research. Instead, I focus on the ability to identify individuals participating in studies conducted in anthropology departments based only on their data or 'meta-data'. This is a concern not only for anthropologists who might conduct research among vulnerable populations or in repressive regimes, but for anyone who, for example, makes use of social media or cloud services when dealing with participants or participant data.

Meta-data is the data we have about data. For example, rather than recording a conversation (the data), meta-data is the record of how long the conversation lasted and who participated in the conversation. Anthropologists often record both data and meta-data in their research. This is sometimes done directly with our notes or audio recordings, or passively by means of the timestamps generated automatically by our devices and online communication tools. Furthermore, and more to the point, the social sciences are currently moving in a direction of increased digitization and utilizing online social networks either passively or directly in research. Therefore it is important we understand what can happen with the data and meta-data records because this affects the ability of anthropologists to maintain the privacy and protection of their informants and research participants.

How big is big data?

One question that often arises in discussions of 'big data' is how big is 'big'. Largely, this is a semantic issue. Generally, 'big data' refers to datasets that are too large to be manipulated or stored on a single computer. The quantity of such data generally goes far beyond the ability of any one individual or even group of individuals to analyse. For example, one may take weeks to read through the entirety of the New International Version of the Bible (which is roughly 6,000 kb). However, this file is could be one of millions of equally large files stored on a consumer external hard drive available at almost any computer store (a 6TB drive could take 1,000,000 copies of the Bible), representing an amount of text that could not be read within the lifetime of any one individual. To put this in perspective, the ARCUS-b system is the new 'supercomputing'

facility for the University of Oxford and is open for use to researchers in any department; the Institute for Cognitive and Evolutionary Anthropology has been using the system for advanced data analysis and simulation since 2013. This system, while impressive and useful, is not competitive with many modern cloud computation platforms, presently having approximately 1500TB of space.[4]

Currently, there are a number of 'big data' projects in anthropology that really are 'large data', projects such as the SESHAT data archive (Turchin et al. 2012) and to some extent the eHRAF database (Human Relations Area Files, 2015). These databases are archives for works produced by small numbers of individuals, but although they are impressive in their size and scope, they would not be considered 'big data' by most data analysts. Furthermore, the type of data in these aggregation projects rarely if ever records individual-level data points. As such, they represent great archival resources but do not necessarily involve the ethical dilemmas that collecting individuals' personal data would.

Some researchers, however, utilize corporate–academic partnerships or have found ways of obtaining data from websites such as Facebook, Twitter and other online social networking platforms. Other researchers have utilized data produced passively (i.e. without user intervention) by electronic devices such as smart phones and GPS tracking devices for their research (e.g. Backstrom et al. 2012; Eagle and Pentland 2005; Gonçalves et al. 2011; Lerman et al. 2010; Leskovec and Horvitz 2008; Pentland 2014; Ritter et al. 2013). This can be done by gaining access to their data servers but can also be done by 'web-scraping' or downloading and restructuring the information (such as usernames, timestamps, posts, replies, 'likes', etc.).

What is obtained?

In principle big data can be almost any type of data; in so far as anthropologists are concerned, it is data about individuals and their beliefs and behaviours. Currently big data ranges from our credit card records, internet usage, social network contacts, phone records to even dating habits (Rudder 2014). However, when it comes to data for human communication, of a sort that would interest anthropologists, big data can provide information about an individual, who they communicate with and what was said. This does more than provide a framework for data analysis – it also provides an opportunity for data reconstruction. By this I mean the use of large

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[4] Figure based on the current allocation of 5TB per user and an average of 300 active monthly users.

datasets to interpolate relationships between parts of the data in order to recreate the underlying social networks from which the data were obtained.

For example, when working with Facebook data, the actual social networks of an individual can be downloaded (assuming that the appropriate agreements and consent have been provided by all relevant parties) in a machine-readable format. However, one could also utilize a web-spider in order to harvest the list of friends put on a website and the associated links for that person, then have the program go to each of those links and download the list of friends for each individual, and subsequently for each of those individuals in turn, and so on. Such a process allows us to publicly recreate approximations of social networks without the actual consent of any individual.

Accidental data collection

A second issue with data protection now arises. Specifically, when I grant access for an outside party to gather my data, by implication it also allows them to collect information about other individuals (i.e. my friends). This is the case even though there was no informed consent on the part of any other person besides myself. Given how many friends an individual is likely to have on a social network, what results is that informed consent has not been obtained for most of the 'participants' who have now become part of a study.

What can be done with big data?

Now that there is at least a general overview of what big data is and where it comes from, this leads us to a practical question: what can we do with it?

In theory, we can do almost anything with such data. It can be analysed for correlations, mined for patterns of speech or social interactions, measured for descriptive analyses of sociality, or used to better understand how information is transmitted between individuals (among many other things). A recent monograph has shown the full power of big data to predict human behaviour in its title: *Predictive analytics: the power to predict who will click, buy, lie or die* (Siegel 2013); to these ends, predictive analytics of big data is not a matter of looking at population trends, but of targeting individuals for (mostly) marketing purposes.

What we should concentrate on for the purposes of this article is the more nefarious use of such data. By nefarious, I'm not exclusively referring to its use by 'hackers' or identity thieves

(although this possibility is extremely real). I am generally referring to the use of big data to identify individuals for any reason beyond that intended by the primary researcher who collected the data and the participant who consented to its use in a specific manner.

How can this be done?

Given enough data, individuals can be identified quite easily, even if they are anonymous or have been re-coded in the dataset. Think for a moment about all of your friends, and all of your friend's friends. Imagine that one has all the data necessary to recreate the social network of your friends. Now, given a single extra data point beyond the initial network, you have to specify which individual is X. You know X's friends and you know that X also has a unique data marker (say a specific political leaning). One can then take this unique marker and match it against the publicly available data accessible through basic search engines. After interpolating the unique data marker against a foundation of the social network, one has only a very few statistical targets left.

This doesn't have to be done algorithmically; it can be done manually as well. Given a deep understanding of someone's beliefs, likes and predispositions, one could easily acquire a deep qualitative understanding sufficient to target a needle in a haystack. For example, marketing consultant and entrepreneur Brian Swichkow obtained online quasi-celebrity status recently by playing a prank on his room-mate. Knowing basic information about the latter, such as the fact that he was a professional sword swallower, he was able to quickly construct Facebook ad campaigns that targeted only his room-mate (Holiday 2015). He was helped by the fact that the relevant information was mostly demographic, such as his room-mate's employment and location, and was thus able to create intimate ads that targeted only one of Facebook's 1.5 billion monthly users. Given the ability of one person to target another, the possibilities to reconstruct social network information only become greater given the widely available data-stores on the Internet.

Individual identification data can also be hacked. This is sadly a very real possibility that anthropologists should take seriously. As we move from our pen-and-paper field notes to increasingly digital information storage platforms such as Dropbox[5] or NVivo,[6] we open

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[5] Dropbox is computer program that allows anyone to freely store documents and files on their computer and automatically back them up externally 'online' and access them from an internet browser if need be (Dropbox.com).

ourselves up to having our data taken by anyone who can gain access to those digital files. This means that we need to take careful consideration of how and where our data are stored.

Two issues to start with

Given the outline presented above, readers may have many questions that they would like addressed. I will take two issues and discuss them a bit further, namely data security and the propensity to find unexpected results, though they are only two among many important issues. Data security refers to the way in which researchers store, transport and utilize the data they have at their disposal; this is inextricably linked to participant protection. 'Unexpected results' refers to the ability of researchers to discern information about their subjects or participants that they did not intend.

1) Data security

Data security is one of the most talked about and least understood issues in our daily lives. Taking even the simplest steps to secure our data can go a long way. This section will briefly present three ways of increasing data security for our subjects or participants. The first is physical security, which means keeping close tabs on the physical location of our data. The second is encryption, or the process by which we make our data unintelligible to unauthorized entities; this can be done physically or digitally. The last, related to 'physical encryption', is anonymization, or taking steps to ensure that the data cannot be reverse engineered to reveal the identity of someone even if it falls into the wrong hands.

Physical security

One of the first ways to keep data safe is to make sure that we keep them securely stored in a way that only we can access them. This goes not only for digital records, but also physical records such as pictures and field notes, which obviously contain very important and often identifiable information. Although digital data can clearly be hacked and reconstructed, physical records collected by anthropologists often include identifying information; after all, the personal life of people is the professional life of the anthropologist. Therefore, knowing at all times where

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[6] NVivo is a popular software program used to store field notes, videos, audio files, transcripts, photos and other materials electronically, thus allowing one to organize and analyze the material.

our physical and digital records are and who has access to them is of the utmost importance. For some, such as myself, big data security means not allowing a computer that has access to the data to be misplaced or stolen. The same goes for the physical data of field notes. In some cases, this can be hard to do; conflict zones often include checkpoints or border crossings where searches and seizures of one's belongings are possible. In such a situation, we can rely on two more concepts in order to protect our data: encryption and anonymization.

Encryption

Encryption is a method by which data are rendered unintelligible without a key. In the digital sphere, even if someone were to get hold of an entire encrypted hard drive, it would be useless without the encryption key. In the physical world, using codes that have keys stored separately can serve the same function.

Digital encryption uses mathematical transformations of information in a computer to make the data appear essentially random. This is done by using extremely large prime numbers which could not be factored due to current computational limitations. That is to say, if your data are encrypted, they will not be understood unless you want them to be. This is used by banks, governments and journalists to secure the information sent between two people. Many operating systems, webservers and software programs have settings that allow you to encrypt your data. For example, the free operating system Linux allows the user to automatically encrypt all the data on their computer. Email systems, such as the free email client 'Thunderbird', allows the sending and receiving of encrypted emails on all operating systems. Being knowledgeable about what you can and cannot encrypt on your own computer is crucial.

When creating our own field notes, we include a great deal of personal information. We can, for the sake of argument, take this information as similar to the information that is collected in online social networks. This information can allow an individual to pinpoint who it was that provided that information by attempting to resituate the information back to its original context. Because the physical location of our field notes is often either tied our field site, or in the field site itself, it is easy to pinpoint the context from which the information was drawn. Allowing our physical notes to become separated from us under any means therefore represents a security breach that can have detrimental effects on the anonymity of our informants and research participants.

This form of security breach also comes in a quasi-digital form. It is common practice at border crossings for individuals to have their personal belongings searched. This provides the potential for physical notes to be taken, especially in areas where governments claim expanded powers of search and seizure and can legally access your belongings (Schoen et al. 2011). This is also the case in contexts of political instability or when internal leadership exerts further endogenous controls on a population. These are examples of situations in which information carried across borders could potentially harm our informants if they are linked. The loss of direct control of our data represents a similar, if not more serious threat to the security of our informants, and this holds whether the data are stored digitally or physically.

In the modern world, we communicate through online social networks (e.g. Facebook, Twitter), email, or programs like Skype or Google Hangouts that allow us to 'call' or 'video chat' over an internet connection. This information is all trackable and—almost definitely— tracked. This can be recorded either as it happens by tracking information as it goes between internet connections, or by compromising the physical security of electronic devices (Nakashima and Wan 2011; Timmer 2015; see Waksman and Sethumadhavan 2011 for an analytical overview from the computer science perspective). This too is an opportunity for our data to be taken out of our control and therefore represents a potential breach of data security. If someone has access to our laptop and contacts, they know who our informants are and could potentially use this information for nefarious means. Encrypting files, hard drives and email accounts is the least we can do to protect our data in this regard.

Securing data

One potential solution to the threats of physical *and* digital security breaches is a form of 'two-factor authentication'. In the digital security world, two-factor authentication is a system that requires two types of authentication before someone has access to the information. Typically, this is something held by the user and something known by the user. For example, a digital two-factor authentication system could potentially be unlocked by physically inserting a USB (aka 'memory stick' or 'flash drive') into the system and then providing a password or answering a question only the user would know (e.g. where one met their spouse, the name of their first pet, etc.); almost anyone who has ever had to deal with a bank online is familiar with such a system. This principle also applies to physical data (field notes, audio/video recording devices, etc.).

Quite simply, the first key can be physical: a lock on baggage or physical storage. Such physical locks are required for a lot of research and are currently used by researchers in our department to store physical files. The second form of physical data security is to split it up. If you have recorded participants' responses, store their responses separately from their names or consent forms (if such a form is collected physically). Anthropologists often change informants' names, but changing one name to another can be potentially useful for finding the source of the information, such as gender, race, or age. Instead, we should anonymize participants and informants using strings of letters and numbers simultaneously. For example, we could anonymize participants based on the site, year and researcher, combined with a unique identifier. So, if your research group knows that you are researcher 633848, your field site is coded as 145 and the year of research is 2014, you could code the information as 633848781452014, where 78 is a code for a specific participant. The researcher can then store a list of names and simply the number 78. Doing this means that, if you lose that piece of data, the receiver would only have access to a list of names and numbers.[7] If one loses the data itself (interview, survey, transcript, etc.), the receiver would have a lot of data, potentially enough to reverse-engineer if enough contextual details are included. However, they would have that data and the number 633848781452014. If by chance someone finds their data, they would also need to know how to decipher the embedded strings of numbers included in 633848781452014 to discern who the individual was by name. This technique can be strengthened by taking the responses of individuals and breaking them into smaller pieces, all stored separately, thereby increasing the difficulty of reconstructing the dataset without knowing the key to its reconstruction.

2) Unexpected results

One other issue that often arises from some research is finding unexpected results. Typically, scientific studies are approved for a specific purpose, and confidential or identifying data are kept secure. Therefore, the usage and results obtained by studying such data are restricted for specific pre-specified use. However, in large datasets we can often find unintentional patterns in

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[7] Although speaking with an anthropologist or outsider could be potentially threatening to a participant. It is the researcher's responsibility to understand the risks associated with the research and to make all risks explicitly clear to all participants *prior* to initiating any data collection.

the data. While naturally many such correlations or 'significant results' are spurious at best,[8] some may have vast repercussions. If results are both significant in a statistical sense and imply broader consequences for the subjects of or participants in a study, they could be either beneficial or detrimental in their effects. For example, the data could reveal a pattern that may compromise participants if the information got into the wrong hands, as if they were to reveal the specific importance of an individual as the crux of a social movement, such that opponents of the social movement could target that individual.

Big data answers big questions

The above information may come off as very bleak and negative, as if, by utilizing electronic data, anthropologists are compliant in an Orwellian dystopia. I assure you this does not need to be the case. On the contrary, I am personally very optimistic about the research prospects of big data. This is primarily because anthropologists often ask very big questions concerning human sociality and what sets humanity apart from much of the biological world in very interesting ways. To answer these big questions, we can use big data to acquire better understanding through statistical inference and data analysis. We can also use this information and data to generate further questions about human sociality and how individuals in different cultures act similarly or uniquely.

To an extent, big data overcomes issues of sampling and generalization known to the more empirical schools within anthropology. However, we should not think that the issues it raises are unique to anthropological approaches reliant on large sample sizes. As seen above, the in-depth qualitative data that are the hallmark of more qualitative approaches within anthropology can also be abused in the world of big data.

Conclusion: (towards) a framework for consent and the responsible storage of data

Data security issues are an undeniable aspect of contemporary research in anthropology. As studies relying on large samples (i.e. big data) become increasingly common, a host of ethical issues are raised that are both familiar and new to anthropologists. As researchers, we have the responsibility to be informed about the ways in which we can protect our informants and their data. We also have a responsibility as members of the academic community to push our review

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[8] This is so common in datasets with large variables that statistical procedures such as Bonferonni Correction have been devised in order to account for studies that test for many relationships simultaneously (see Abdi 2007).

boards to address the problems while understanding the potential for research that utilizes digital data.

So far this article has largely posed questions and only offered brief answers. However, if we are to tackle the problems noted above systematically in such a way that a single ruler can be used to measure the merits and ethics of a research proposal for use in review committees, we must create some systematic way of approaching these questions.

One proposition has been offered by MIT Media Lab's Sandy Pentland (Pentland 2014), who is a world expert on big data gathering and analytics both online and through 'reality mining' (see Donget al. 2011; Eagle et al. 2009; Eagle and Pentland 2005; Waber et al. 2007). Pentland argues that informed consent and the ability of participants to delete their data at will is the key to protecting the data of individuals. On the whole, Pentland's framework starts a great discussion. However, it is not always enough. As noted earlier, so much big data results in information about non-participants that is passively collected. They must be protected as well, and Pentland's framework (presented at the end of Pentland 2014) is insufficient in this regard.

Ultimately, passive data aggregation is the result of an individual's lack of knowledge about what information is presented publicly about them. As such, I suggest, it is the responsibility of the researcher to protect all data, whether or not they are tied in any way to a direct participant in the research. For example, many in cognitive anthropology use psychometrically validated scales. If these are deployed on a social network platform, both social network data and psychometric data are collected in the research. Currently, the ethics review boards of most institutions feel that encrypting and storing the data is sufficient for protection. I argue that this is not the case because a single lapse in the security of that data results in a breach of both social and psychological data, easily allowing participants to be identified. Therefore, different aspects of a project (i.e. the psychometric data, the social network data, the ethnographic or qualitative data, etc.) should be stored in different physical locations using different storage systems and different encryption methods; this still does not make the data impervious to being 'hacked', but nonetheless one can argue that reasonable and necessary precautions have been taken to protect the identities of those who have entrusted their personal information to us.

Clearly, this article is in no way an attempt to finalize a proposal or even nail down what is likely to be the best course of action; it surely fails in this regard. It is only intended to initiate a conversation among anthropologists about what can happen to our data and therefore to our

informants and participants. This will hopefully result in a more rigorous conversation at the institutional level whereby minimum standards can be implemented that all researchers must adhere to in order to best protect the data of those with whom we work. As always, the onus is on the researcher to take the necessary and sufficient action to ensure the security and safety of themselves and their informants or participants. Traditionally, anthropologists have attempted to prioritize the anonymity and welfare of their informants. This priority is well suited for the age of big data. Our intimate knowledge of communities—and what can happen if anonymity is not maintained—makes anthropologists particularly well-suited for this discussion, not only amongst themselves, but within the greater debates that are currently happening in the academic and corporate worlds.

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BOOK REVIEWS

N. BANKES and T. KOIVUROVA, *The proposed Nordic Saami Convention: national and international dimensions of indigenous property rights*, Oxford: Hart Publishing 2013, xv, 417 pp.

This volume is a compilation of essays, written primarily by legal academics who specialize in Sámi or other indigenous people's topics, and it evaluates and discusses the draft of the Nordic Sámi Convention through a mixture of legal, historical and social prisms. The Nordic Sámi Convention is a ground-breaking rights document that shares many features of the UN Declaration of the Rights of Indigenous People and ILO Convention 169, but only addresses issues connected to the Sámi. The book is divided into four parts, each focusing on a specific aspect of the Convention, such as property rights, international law and country-specific Nordic land rights, altogether consisting of fifteen rather disconnected papers. For the purposes of this review, I will comment on the overall feel of the book and some of the more striking ideas put forward before evaluating some of the essays specifically.

As a whole, the book leaves generally eloquent, persuasive, coherent and comprehensible impression regarding the way it portrays, argues and affirms the main ideas. However, it also leaves the reader with a slight disappointment concerning the accessibility, flow and continuity of the way the book has been edited. Each essay is carefully crafted to a very high standard of academic writing, most of the ideas are introduced well, and some of the prior knowledge required of the more specialized topics is addressed. The specifically legal terminology is clarified in most of the essays in a way that can be understood by people outside the field. There is a good balance between the legal, historical and social discussion of the main ideas, and the legally saturated beginning of the book is gradually transformed into a more historically oriented middle with a predominantly social and cultural end in the way the key issues are evaluated. The introduction and conclusion excellently outline and summarize the main aspects of what the volume is all about. Nevertheless, it is rather challenging to read and follow because of the long, sometimes unnecessary footnoted references, the heavy use of terminology and sometimes unrelated historical facts and clarifications that have already been addressed in previous essays in the book. The footnotes were an aspect I found particularly troublesome, especially when there are

many pages where more than half of the page is footnotes, and some pages where the actual text is outnumbered by the quantity of footnotes. The flow is also interrupted by the presumption that the readers are fully familiar with the Nordic Sámi Convention, the UN Declaration of the Rights of Indigenous People and ILO 169, which at times leaves the reader with a disconnected feel. Within each of the four parts of the book, the reader is left desiring a better connection between the essays, essay 15 in particular apparently being placed randomly at the end of the book. Despite this, in Part 3 of the book, the country-specific Nordic Sámi land and rights laws are very well discussed and described, with an excellent flow between them. One other drawback is the way the book has been referenced only through footnotes, which does not offer comprehensive reference lists after each essay or at the end of the book, makes it harder for readers to follow up on particular ideas in other writings. Overall, therefore, while the collection is highly informative and persuasive in the way it discusses the key ideas and creates some excellent points about the validity and strengths of the Convention, it leaves readers slightly disconnected due to its editing, which is understandable when dealing with such a broad topic.

Some of the more stimulating ideas discussed in the book include issues connected to property laws, legal pluralism, self-determination, Finnish domestic laws and women's rights. In the opening essay, Nigel Bankes clearly and successfully starts off the legal evaluation of the Convention without making any ground-breaking points, but he does introduce the lines of argument concerning interests in property very systematically. The aspect that grabbed my attention was his reference to James Tully's political theories, with their strong argumentative opposition to John Locke's justification for colonial supremacy to claim indigenous lands without the consent of the indigenous people. Bringing in Waldron's ways of dealing with historical injustices via his argument regarding two models of reparation seems to continue the theoretical evaluation of how the situation with the Sámi could be framed. The 'what if' and 'what now' reasoning that Waldron uses is a very philosophical way of dealing with a primarily legal situation, but it does add to Bankes' already developed arguments that he later linked to the draft of the Convention in a rather brief but to the point manner. In the next essay, Jonnette Watson Hamilton gives generally excellent legal introduction and justification of legal pluralism, except that the lengthy legal historic outbursts, which are saturated with terminology, could have been kept to a minimum, as they do not add much to the author's Convention-specific arguments.

The best point to emerge here is the rather basic but appropriate claim that legal pluralism is a critical component in dealing with a transnational indigenous society like the Sámi, but there is clearly no 'one size fits all' (p. 75), and any action in the direction of achieving any pluralistic goals will require recognition, reconciliation and highly gradual processing.

Moving on to the fourth essay, Timo Koivurova addresses the exercise of self-determination among transnational indigenous people and its advocacy in international law. After reviewing the historical representation of self-determination since World War II, Koivurova moves on to dwell rather predictably on the use of the word 'people' in international law, which has long been a sore point for anyone dealing with indigenous rights, but then he quickly recovers by discussing the excellent example of the case of Kosovo and ethnic minorities. Koivurova points out that, according to Article 36 of the UN Declaration and Article 32 of ILO 169, self-determination is 'very much based...within the established Nation States' (p. 119) and 'neither document encourages the segments of transnational people to unite' (ibid.). I do not agree that it is such a black and white situation as Koivurova suggests because the way the articles are written provides some encouragement and a lot of freedom for transnational cooperation. Just because it has not been addressed by an article explicitly demanding such cooperation, this does not mean that an indigenous population divided by modern national borders does not have the right to self-determination across states. Upon a closer reading of Article 36, point 1, indigenous people divided by borders do have the right to self-determination with their own and other peoples across international borders. At the end of the essay, Koivurova addresses Scheinin's criticisms that the draft Convention seem to resemble a social contract rather than an international treaty, and he puts forward a valuable constructive argument that the members of the Expert Committee should take into account when putting forward the draft as a contentious Nordic law treaty.

Juha Joona's essay on the situation in Finland that is linked to the draft Convention deals with a particular injustice based on some historical evidence related to the 1673 Settlement Decree for Lapland, the movements of reindeer-herding Sámi into Kemi Lapland, primarily settled by hunter-gatherers, and a methodologically flawed 1962 interview identifying indigenous inhabitants. The outcome of all the above factors was the misleading creation of a definition of the indigenous population in Finland, which prioritized the newly settled reindeer-herders and almost entirely

shut out the original semi-nomadic Sámi population of Finland that has occupied the area 'since time immemorial' (p. 241). Joona's remarks about the faulty means of identifying the indigenous population in Finland leaves out a huge number of people who are not legally recognized as Sámi, an issue that must be rectified before the draft Convention is implemented. Jennifer Koshan evaluates the lack of articles that address the inequality between men and women when it comes to securing rights within the Convention. The Sámi are historically and traditionally a very gender-neutral society, where men and women are equal in most respects. However, this was affected in a major way after other people started occupying their areas and colonization forced the Sámi to introduce more unequal gender relations. Koshan mentions Åhrén's criticism of the draft and how it is failing to address issues connected with children, youth and women. I agree with this statement, especially the children and youth aspect, but to me it sounds that for all the purposes of the draft, women form an inseparable part of the adult Sámi population. However, the Reindriftsavtalen 14/15 (the annual Reindeer Husbandry Agreement) in Norway does cover women herders, and even though it is discontinuing the female-oriented grant, it will be implementing organizational techniques and various organizational measures regarding gender equality by doing more than just offering money. Overall, some excellent key ideas are brought up about the draft Convention throughout the book's essays.

A closer examination of Else Grete Broderstad's essay on cross-border reindeer husbandry and Christina Allard's discussion of reindeer rights in Sweden reveals a few more areas of improvement that the draft of the Convention could address before being implemented. Else Grete Broderstad gives a good overview of the cross-border situation between Norway and Sweden when it comes to reindeer herding, but the essay has an overall feel of Norwegian-based subjectivity. One of the first questions raised, 'How can we explain why it has been so difficult to reach agreement on cross-border reindeer management?' (p. 151), targets exactly the historical data revealing a centuries-long conflict between the two countries. Broderstad divides the theoretical models for dealing with such political situations into two: norm-based and interest-based policies. This particular section seems to dwell too much on the rather simplistic policies, but it makes an excellent point in using Walton and McKersie's dichotomy between distributive and integrative bargaining. The Lapp Codicil of 1751 is historically the first and one of the most important treaties for the Sámi, and

Broderstad appropriately compares its importance to that of the Magna Carta and its symbolic representation of liberty and the rule of law. This has also been the one and only document for many centuries to preserve the right of herders to continue their traditional way of herding, which includes crossing the Norwegian-Swedish border with their reindeers depending on the season. Broderstad assesses the historical narrative from 1751 all the way to 2009, when a new reindeer herding convention between the two countries was signed. What Broderstad seems to be doing excessively is to express a more Norwegian-based opinion with examples and quotes primarily covering the Norwegian side of the argument, leaving the Swedish argument lacking in force and credibility. Bearing in mind that this is a cross-border problem, it is only fair to cover both sides and convey the opinions of both representatives. This is not an essay in which the author necessarily needs to take sides, but even if Broderstad decides to argue more for the Norwegian side, the Swedish argument should be done justice by at least being better represented in the paper. The bold and rather inappropriate statement that 'the Lapp Codicil was ahead of its time' (p. 174) seems to conclude the essay in a very bitter way. I do not agree with this statement, as historically speaking the Codicil fulfilled its purpose perfectly. The fact that later the governmental systems of Norway and Sweden failed to protect the Sámi does not mean that the Codicil was ahead of its time. It would be more appropriate to say that the two countries were behind in their political, moral and juridical abilities to implement justice for the reindeer herders, but the Lapp Codicil was created and implemented at the right time and under the right circumstances.

Christina Allard's essay gives a comprehensive overview of how the reindeer herding laws have changed in Sweden through the use of historical data and specific cases. Allard appropriately begins her historical exploration by raising the question of who is entitled to herd reindeers in Sweden under the Act of 1886, which was the first of its kind in Sweden and unfortunately was very Darwinian in its views. The Act laid down that reindeer herding was a collective right for all Sámi. Allard identifies the problems with the Act and then follows its progression through the 1971 Act and 1993 Amendment with the different eligibility conditions imposed on top of being able to identify oneself as Sámi. Membership of a Sámi village seems to be the latest addition to the otherwise collective right to herding. What Allard describes excellently are three specific examples, the Taxed Mountain, the Nordmaling and the Girjas cases, each of which contributes to Allard's assessment of the conceptual

confusion that Swedish laws have created when it comes to herding laws. Norwegian developments in the legislation connected to herding are also pointed out, but what Allard mostly argues is that the confusing and rather inappropriate collective policy in Sweden should be addressed before the draft Convention goes forward. From my own experiences with Swedish and Norwegian reindeer herding, it is a highly competitive market and has a close to zero entry margin for Sámi who, despite being part of a Sámi village, have historically not been involved with reindeers or have shown no interest in this form of livelihood. Even those who have been herders but have then given up herding for various reasons also stand a very slim chance of getting back into herding due to the peer pressure they encounter. In my view the legislation is strictly formal: when it comes to the actual herders, the laws of social inaccessibility and negotiation with one's peers are more powerful tools. Nevertheless, Allard is correct to identify the flaws in Swedish legislation, which have to be addressed before the draft Convention can be finalized.

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YARIMAR BONILLA, *Non-sovereign futures: French Caribbean politics in the wake of disenchantment*, Chicago and London: The University of Chicago Press 2015, xviii, 229 pp. ISBN 9780226283814.

In a rich and historically deep ethnography, Yarimar Bonilla describes political histories of postcolonial 'disenchantment' through the rise of labour activism in Guadelope and the French Caribbean. As one of the French *départements d'outre mer* (DOM), or overseas departments, Guadeloupe is often understood as a non-independent exception to its surrounding postcolonial Caribbean neighbours. Over the course of the book's six chapters, Bonilla explores Guadeloupe not 'as a site of problematic sovereignty' but rather as a place for the 'exploration of sovereignty itself as a categorical problem' (10). In doing so, Bonilla argues for re-imagining the Caribbean 'as a non-sovereign archipelago' in which representations of 'non-sovereign societies as sites of paradox and exception' (10) have served only to obscure the larger possibilities for these non-sovereign pasts and futures.

The book is divided into two sections with three overarching goals. The first goal is to explore labour activism and subject formation through the political transformations of Antillean labour struggle and the navigation of unwritten 'transcripts of the future' (5). Drawing on Michel-Ralph Trouillot, Bonilla further examines the intersection of 'historical and political praxis' in the 'rich tradition of historically grounded Caribbeanist anthropology' (5). Practices of contemporary labour activism such as the use of Creole throughout negotiations, drum circles outside labour courts and memory walks through places and practices of slavery and slave resistance beautifully demonstrate the relevance of both history and memory in contemporary ideologies. Finally, she seeks to locate Guadeloupean labour activism within a larger Caribbean negotiation of postcolonial politics.

The first section of the book sets out a broad historical overview of the production and evolution of Guadeloupean political histories. Chapter 1, 'The Wake of Disenchantment', begins by reframing the controversial departmentalization championed by Aimé Césaire and the subsequent rise of anticolonial nationalism and syndicalism in the 1970s. By tracing the trajectory of French Antillean thought through different political generations, Bonilla argues that each subsequent set of political activists and leaders has been neither uniform nor easily categorized. She ends the chapter by asserting that present-day Guadeloupe faces 'a moment of categorical uncertainty', but 'also an era rife with emergent possibilities' (39). These possibilities are fleshed out in the following ethnographic chapters.

Chapter 2 examines contemporary notions of freedom, nation and sovereignty through the use of strategies and metaphors of slave resistance in contemporary labour activism. Bonilla asks how and why unions in Guadeloupe have used this 'strategic entanglement' (40) with practices of slave resistance such as *marronage*, a term that refers to the *nèg mawon* or rebel slave and includes a 'broad range of practices through which enslaved populations contested the system of slavery across the Americas' (41). In doing so, she continues the intergenerational analysis of shifting Antillean political thought around self-determination and sovereignty through a re-imagined, postcolonial *marronisme* (46). Bonilla provocatively situates Césaire's own pursuit of departmentalization for the French Antilles over the 'flag independence' (xiii) of other Caribbean entities within the practice of 'pillaging, othering, or marooning' (52) of slave resistance.

In the book's second section, 'Emerging Transcripts', Bonilla explores ethnographic evidence from nearly a decade of her own fieldwork among contemporary Guadeloupean labour activists. She draws on interviews, public documents, labour negotiations and participant observation with a purposeful intent to 'engage with [her] informants as theorists' and 'reflective actors' (xvi) in order to 'grant them analytic competency over their own acts and forms of cultural production' (xvii). Each chapter draws heavily on Bonilla's work with the *Union générale des travailleurs de la Guadeloupe* (UGTG)*,* the General Union of Guadeloupean Workers, the largest labour union in the French Antilles. She weaves together the reflections and analysis of labour activists, government *fonctionnaires* and social theorists with particular aplomb.

Chapter 3 provides a thick description of 'life on the piquet' (65), or picket line, and details the forging of everyday communities and subjectivities in the liminal spaces of the strike. Bonilla argues that the effectiveness of labour action cannot be measured solely by its material or economic consequences but must also 'be more subtly gauged by analysing the affective and subjective transformations that take place during collective action' (66). Countering the narrative of a strike as a site of inevitable disappointment, Bonilla delves deeply into the 'bittersweet place of the piquet grève', simultaneously a 'space of community and solidarity' (73) and one that can bring destabilizing interpersonal conflict to relationships at home as well as work. Her description of the *piquet grève* as a 'liminal space, betwixt and between the domains of labour and leisure, on the margins of the capitalist economy and enmeshed in new forms of community with those around them' (79) is particularly striking. Chapter 5 expands upon the affective transformation of labour practices by examining the role of history in French Antillean memory walks organized by labour unions. The Creole slogan '*fè mémwa maché*' means to 'make your memory walk' (130) and is used in promotions for UGTG walks, which aim to 'generate [a] feeling of historical intimacy' (132) as well as a 'newfound faith in the political efficacy of the present' (147).

Chapters 4, 'Public hunger', and 6, 'Hope and disappointment,' give accounts of the 2004 'Madassamy affair' and the general strike of 2009. In the first, Bonilla recounts the arrest of a Guadeloupean labour activist of East Indian descent, his subsequent hunger strike while imprisoned and the activists' political tactics in seeking to shape media and labour negotiations in its wake. In Chapter 6, Bonilla

describes the progression of the general strike of 2009. Returning to the postcolonial 'wake of disenchantment' of the opening chapter, she argues that the disappointment with the fruits of the general strike was accompanied by high hopes for future political engagement.

In this first book, Bonilla brilliantly blends political, historical and media anthropology to reimagine the historical trajectory and political futures for non-sovereign polities in Guadeloupe, the French Antilles and beyond. Though addressing her framework to the French Antilles and the Caribbean more broadly, Bonilla draws primarily on fieldwork in Guadeloupe. Future ethnographic work could expand upon this excellent foundation for a twenty-first century Caribbeanist anthropology. Indeed, the non-sovereign framework proposed here may have broader relevance for social movements beyond the geographical bounds of the French Caribbean.

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C.A. GREGORY, *Gifts and commodities* (2nd edn.), Chicago: Hau Books 2015 [1989], lxiii, 268 pp.

Chris Gregory's *Gifts and Commodities* has been republished by HAU, the book imprint of the popular online journal of ethnographic theory. First published by Academic Press in 1982, the book has since been a mainstay of reading lists for students of economic anthropology, iconized as a key work within the gift–commodity debate. Originally inspired by Marcel Mauss's *The Gift* (2002), one side of this debate has held that the exchange of gifts is distinct from the exchange of commodities. As an example, whereas gifts create social ties between transactors, commodity exchange occurs between independent transactors. The other side of the debate, championed notably by Pierre Bourdieu (1977) and Arjun Appadurai (1986), holds instead that this distinction is largely irrelevant, as both exist for the same reason – to further the self-interest of the transactors. In Gregory's description, this implied universalization of a particular subjective 'self-interest' is a conceptual foundation of neoclassical economics and its associated 'theory of goods'. In *Gifts and Commodities,* Gregory argues against this theory, proposing instead that gifts and

commodities *are* distinct and that it is this distinction that invalidates the use of economic principles to understand exchange within unique societies. This second edition reprints the original text (save for a few typographical revisions) with a new preface by Gregory and a foreword by Marilyn Strathern.

As the foreword and preface describe, Gregory (an Australian) originally went to Papua New Guinea to teach economics at the University of Papua New Guinea. During his time there, he was struck by the inability of economic theories to describe his observations of trade and exchange. Where commodity exchange had increased, and labour and products became things that could be bought for money, so too did the exchange of gifts – a resurgence that could be neither explained nor accounted for by the economist's models. The text thus critiques the neoclassical economic 'theory of goods' in favour of the political economy 'theory of commodities'. Extending the latter, Gregory offers a complementary 'theory of gifts', building upon the work of anthropological heavyweights such as Morgan, Mauss and Lévi-Strauss, as well as Melanesian ethnographies by Mead and Strathern, amongst others. These theorists, along with Gregory's own observations in the region, are then used to demonstrate that gift exchange has flourished in Papua New Guinea amidst a growing colonial 'commodity' economy. This efflorescence provides the evidence for Gregory's critique of economic theory, which rejects the idea that all exchange can be explained, *a priori,* by the universalised 'theory of goods' or the related 'formalist' mantra, or that principles of exchange are constant across societies.

The book is divided into two parts and preceded by a helpful introduction to the complex colonial history of Papua New Guinea. Part One, 'Concepts', marries the political economy technique of analysis (which is predominately explained through the work of Marx) with anthropological concepts of kinship and gifts. The final chapter uses these concepts to critique the focus of economics on individual choice, a focus that denies economists the ability to understand the peculiarities of gift exchange, wherein debt, rather than capital, is accumulated. This failure has resulted in the renunciation of such forms as 'primitive capitalism' or 'distortions' in a universal model. Part Two, 'Theory', uses Papua New Guinea to show that gift exchange is in fact a 'modern' phenomena, one that has increased alongside commodity exchange in the growing colonial economy of the region. Part two is a particularly impressive synthesis of historical and anthropological data related to the region, illuminating a relationship of exploitation between Australia and Papua New

Guinea that today remains notably absent from the Australian historical imagination. Gregory's conclusion is brief and succinct, offering a final restatement of his approach, as well as his view of the importance of his thesis.

Throughout the text, Gregory works through his propositions and conclusions in clear, methodical and often repetitive fashion. His background as an economist is evident in his generous use of diagrams and mathematical examples, which, while supporting the text, often lose the reader in detail and render reading a rather dry task. This dryness, however, does contribute to the convincing nature of the work, and the reader may wonder whether this was a deliberate aim of Gregory's. One of his key charges against the economic method is that it is subjective and psychological, given that 'the preferences of utility-maximising individuals [that] provide the data of the analysis' (p. 116). The arid prose therefore serves to heighten his contrast between the 'intuitive' neoclassical economics and the 'factual' political economy method. Whether intentional or not, this lack of literary flare situates the work as pre-*Writing Culture* (cf. Marcus and Clifford 1986). In contrast to most anthropological works today, the text is resoundingly free from the subjective voice, and as a result is likely to be less appealing to students than more recent works in the discipline.

Despite the difficulty the reader may have with this text, the original work remains an inspiration for any student wishing to publish anthropological theory that reaches and engages with debates outside the discipline. As Gregory states, many countries have been 'developed' based on economic theories. Economics as a discipline is an, if not *the*, authoritative voice in domestic and global politics (cf. the 2010 documentary *Inside Job*). Hence the charge outlined by Gregory, that the economic method is insufficient, has potentially huge ramifications. Yet, as Gregory notes in the preface to the second edition, much of the book's reception has remained within in the discipline, and to his disappointment it 'has had no impact on the thinking in the dominant mainstream paradigm: members of the economics discipline have simply ignored it' (p. x1iv). This new edition, we hope, will maintain and perhaps help to elevate the work's status as a rigorous counter-argument to theories that remain largely unquestioned in political decision-making.

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CHRISTINE HINE, *Ethnography for the internet: embedded, embodied and everyday*, London: Bloomsbury 2015, 221 pp.

The internet as a theoretical and methodological concept has stoked the interest of several academics in the last twenty years. From media theorists to anthropologists, researchers have looked into internet communities and other internet-supported social networks to understand multi-modal layers of peoples' lives. In recent years, the immersion in internet realms has become an ordinary activity. One uses extensively smartphones, online services, and other related technology in daily interactions, and as a result, one's connectedness to the internet, and also, to other internet users, is enhanced.

Christine Hine, in her seminal book *Virtual Ethnography* (2000), looked at internet-based social research in the 1990s. Her recent publication *Ethnography for the Internet* (2015) gives continuity to the theme of the social study of the internet in the early twenty-first century, acknowledging change in relevant technologies and practices related to the use of the internet. Hine's latest publication is a textbook aimed at students, researchers, scholars, or other internet researchers. In many ways *Ethnography for the Internet* is a guidebook for doing ethnography online, as it provides information regarding practices, strategies, and challenges in internet research.

One of the key arguments in *Ethnography for the Internet* is that the term 'virtual' is no longer helpful when discussing the internet (p. 87). Today we are a long way from romantic, exotic, and futuristic notions of the internet as a cyberspace or an information superhighway, areas of virtual reality based on concepts that were

prominent in early 1990s cultural studies literature. Internet-based activities often have physical manifestations; the internet is entrenched in our daily lives in so far as we have the capacity to be constantly online and putting to use internet services, be it shopping, communicating, reading, being entertained and more. The acknowledgment of this social transformation of internet use was imperative in updating Hine's published work, although it has been previously discussed elsewhere (see, for example, Miller and Slater 2000, Boellstorff et al. 2012).

The book contains seven chapters which can be perceived as a two-part division. The first three chapters comprise the introduction, literature review, and methods section. In chapter one, Hine situates the book's place in the broader literature of internet research. Following Geertz's interpretative framework, she stirs the methodological direction followed in *Ethnography for the Internet* towards ethnographic methods. As Hine herself argues, the book addresses an audience interested in doing ethnography in contemporary societies in which various forms of computer-mediated communication are employed. Hine aims at a holistic understanding of this context by searching for meaning and meaning-makers. She also argues that new technologies suggest new strategies for knowledge production (p. 2). On the one hand, she discusses the banality of the internet (pp. 8-9) and how it has become part of everyday activities. On the other hand, she explains certain challenges and limitations that this change presents to ethnographers as well as certain ways of dealing with these.

Chapter two explains the three epithets Hine attaches to the internet: embedded (the ability to connect to the internet using everyday objects, p. 32), embodied (the internet as part of us in daily experience, p. 41), and every day (the internet as a mundane medium that offers the infrastructure for doing other activities, p. 46). She states that she is interested in multi-modal sites, be it online or offline (p. 23). Based on previous literature and her own research, Hine highlights that we cannot talk of a holistic understanding of the internet (p. 26), as it is immense. Thus, she moves towards an open approach to ethnographic holism and seeks the meaning of the internet in people's lives (p. 27). In this respect, Hine examines the internet as a 'contextual and contextualising phenomenon' (ibid.).

In Chapter three, Hine centres on strategies for engagement with the field and for collecting and analysing data from the field. She develops a methodological toolkit that can be applied and modified by ethnographers who seek to generate knowledge

from internet fieldwork. Here, Hine discusses certain characteristics of internet ethnography (multi-sited, mobile, flexible, adaptive, reflexive and networked) that are useful in exploring the connections of people online. Secondly, she analyses data collection methods for internet research such as writing field notes, activity logging tools, scraping, sentiment analysis, visualisations and interviews. Notably, in this chapter, Hine demystifies autoethnographic methods and argues for their importance for the study of the internet, given that the 'experience of navigating the contemporary world is so individualized' (p. 83).

The middle section of the book is divided into three chapters aimed at demonstrating ethnographic examples based on the theoretical and methodological framework described in the previous chapters. Chapter four discusses Freecycle, a network of goods' exchange. In this case study, Hine primarily uses her autoethnographic account to describe goods' exchange in the local Freecycle network, as well as its infrastructure from an insider's perspective, based on her experience as a discussion group moderator. In addition, she explains how other methods such as discourse analysis interviews and scraping that demonstrate evidence of the use of Freecycle on various social media were significant for the understanding of people's experience of Freecycle.

The second case study described in Chapter five overviews the use of digital technologies in the discipline of systematics. Hine explains her methodological choices, given that her case study was institutionally complex, and participants were involved in a distributed set of activities (p. 155). For example, one of her first choices was the specific field in which she had some knowledge as an insider (p. 131). This was particularly helpful in finding research sites and participants. Hine describes the process of selecting interviewees, analysing online forums to understand the discipline better, and using online visualisation tools such as Touchgraph SEO to map the online field. This chapter also discusses policy pressures and their effect on participants' involvement in digital initiatives (p. 145). Hine also touches upon material culture in digital practices (p. 149) and ethical commitments in fieldwork (p. 152), but she does not go into greater analytical detail – perhaps an underdeveloped area of the book in total.

Chapter six looks at the third case study, the television series *The Antiques Roadshow* and how people made sense of the show in their everyday lives (p. 158). Here, Hine employs unobtrusive methods based on found data (ibid.) that were

primarily online traces of comments and discussions that the fans engaged in on the internet (for example, in Twitter). As she observed, through initial research certain patterns emerge, which she followed up with interviews (p. 163). In this chapter, Hine explains the steps towards cross-platform research and argues how the initial examination of data may lead to pop-up ethnography, that is, ethnographic research that has not previously been intended and/or designed. Here, she adopts a perspective agnostic about content (p. 176), which is in line with Marcus's (1995) proposal to 'follow the people'.

There are various positive comments that can be made regarding the structure of *Ethnography for the Internet.* First, the clarity between the chapters is very helpful in working one's way through the book without reading it cover to cover. Secondly, at the end of certain chapters are summaries of key components (see Chapter 3), or points for reflection (for example, Chapters 4, 5). In all, the book is very well signposted and the key concepts are constantly reiterated – although certain repetitions could be omitted (particularly, the arguments for the banality of the internet throughout the chapters). There is some reference to ethical considerations spread throughout the book, and a section regarding the need for emergent ethics for adaptive ethnography in the conclusion (p. 187). However, given that *Ethnography for the Internet* is a textbook, a more complete analysis and explanation of ethical frameworks in internet research would be beneficial, particularly citing key sources such as the ethical decision-making document published by the Association of Internet Research (which is currently absent both from the main text and the list of references).

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SHARON KAUFMAN, *Ordinary medicine: extraordinary treatments, longer lives, and where to draw the line***,** Durham and London: Duke University Press 2015, xv, 314 pp.

Kaufman's recently published analysis of the American health system arrives on the academic scene as increasing numbers of researchers turn their gaze away from the rituals of death to focus on the process of dying. Dying, and dying well, have been defined, questioned and criticized by theologists, physicians, nurses and now anthropologists, as the threshold is extended by technology and medicine. Kaufman's book is a continuation of an extensive body of academic work focusing on aging, the end of life and the effect of the medical community's focus on healing at the end of life.

Kaufman argues that 'ordinary medicine', a set of radical and intrusive medical interventions, now marks the treatment of the elderly, extending aging lives, but not promising any change in the quality of life. In order to do so, Kaufman traces the care received by aging American patients on Medicare, the medical financial insurance system provided in the United States. Beginning with the effect of evidence-based medicine (EBM), Kaufman argues that the practice of relying on statistical evidence has removed the personalized care that once marked the medical profession.

EBM provides the data to argue for increased insurance coverage of intrusive medical procedures, which then become 'ordinary' and standard practice. For example, implantable cardiac defibrillators are now common treatment for heart disease, with a growing number implanted in patients aged 80 and above. Patients receiving the defibrillators may have their lives extended, but in exchange they must suffer the painful jolts delivered in response to cardiac failure. Similarly, patients suffering from liver cancer at the end of life are given the opportunity to wait for a donation, though they can also opt for high-risk donor organs or livers from donors with Hepatitis B or C. Surgery is now recommended by physicians because they are covered by Medicare, having been recognized as successfully extending life. And yet, doctors and patients continue to struggle with the quality and quantity of life post-intervention.

These questions of exchanging the quantity of life for the quality of life echo throughout the book, as Kaufman examines the questions and choices faced by

patients and doctors at the end of life. The availability of treatment and the possibility of extra years of life place into sharp focus the question of impending death. Different families navigate the pressures of aging differently. Some patients refuse assistance from their children but accept it from other kin, while others expect their children to donate to them or support them at the end of life. In the face of such impossible choices, the issues of an accurate prognosis when the timing of death remains elusive and the increasing pressure of family obligations have become part of the contemporary landscape of dying.

The machinery of the health-care system in the United States is examined with a careful and meticulous eye. Little attention has been given to the 'drivers' of health care, namely the influences of EBM and insurance reimbursement. On their own these drivers are benevolent, but combined, Kaufman argues that they lead to interventions that are seen as necessary at the end of life. Physicians caught in the machinery feel helpless and can no longer advise palliation; likewise, patients and their families face impossible dilemmas between life and death that make intervention an easy choice.

And yet, although Kaufman hints at negative health outcomes, most of the cases she presents seem to end well. Patients live happy, longer lives post-intervention, making the ethical underpinnings of the analysis difficult to grasp. Hope and benevolence remain possible. Among Kaufman's contemporaries examining the same issues in other health landscapes, these issues are placed in sharper relief. Sherine Hamdy (2012) evaluates the intersection of organ donation, religion, politics and economy in Egypt. In countries where the health landscape is more uneven, health outcomes are not assured, payment is difficult, religious perspectives complicate treatment, the question of extending life is even more fraught, and negative outcomes are a real possibility.

These problems can also be found in the American health system. Kaufman illustrates the link between treatment reimbursement and standardized medicine, but in doing so she ignores the huge percentage of the US population that remain uninsured and thus face starker problems at the end of life. Moreover, the approach is strictly secular: spiritual beliefs at the end of life are rarely discussed.

Kaufman delivers a provocative argument, and students examining medical anthropology, geography or sociology will benefit from the book. Among the growing body of work in the anthropology of dying, Kaufman makes an important contribution to the political economy of treatment. *Ordinary medicine* provides a valuable

counterpoint in the literature, arguing that while money can provide the option to delay death, it doesn't guarantee 'a good death'.

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EMILY MARTIN, *The meaning of money in China and the United States,* Chicago: Hau Books, 2015 [1986], xiii, 160 pp.

The meaning of money in China and the United States is the first publication in a collaboration between Hau Books and the Morgan Lecture series at the University of Rochester. This manuscript, which was originally delivered by Emily Martin over the course of four lectures in 1986, is a valuable (re)addition to the literature for those studying value in China, the United States, or elsewhere. Fitting with the mission of Hau Books, Martin develops a theoretical argument about how and why money has been used differently in these two contexts and does so with rich ethnographic detail. The volume is also enriched by over thirty photographs (though exclusively in the first half of the book dealing with China and Taiwan). Although this is the first time that the four lectures have been published together in a single manuscript, the main thrust of her argument has been influential in the discipline, not least through its extension in Parry and Bloch's theory of the two transactional orders of money (1989: 28-9). The introduction, written by Martin herself, and the afterword, written by Sidney Mintz and Jane Guyer, help to place the work historically and theoretically within the discipline and also provide a forward-looking gaze. Here, I hope to show how the arguments that Martin has made, especially about money in the United States today, may be developed further by looking at the work of philosopher Charles Eisenstein, coincidentally referenced by Guyer as part of 'current popular efforts to "relearn gift culture"' (2012: 501).

Martin's primary argument is that there are two paradoxes that emerge from the use of money and that these play out differently in the United States and China. While in China a paradox relating to the socially integrating effects of money appeared to be prevalent at the time of writing (i.e. 1986), in the United States a second paradox linked to social disintegration appeared more salient. Martin's writing, as highlighted by both herself and Guyer, is heavily influenced by Marx and Polanyi, though she makes a conscious effort to avoid artificial distinctions between materialism and symbolism, seeing the two as equally important for understanding the complexity of reality and preferring to 'look for the traces of mind in matter' to help overcome this distinction (7). While betraying influences of classic economic anthropology, including the debate between symbolism and materialism, these lectures were delivered in the same year (1986) that *Anthropology as Cultural Critique* (1986) was published and are a fine example of the same. Unlike many others, according to Marcus and Fisher, who make only implicit comparisons or marginal comments (1999 [1986]: 111), Martin clearly sets out her critical stance in the first lecture, referencing Morgan as an inspirational pioneer of this approach (9). She continues to develop her argument by means of direct comparison, first by looking at money and value, then spirits and currency in China, before mirroring this with an exploration of money and value, and of spirit and prosperity, in the United States.

The first paradox presented by Martin details how money as a means of facilitating exchanges is seen to create webs of both interaction *and* social freedom. Martin makes a clear and convincing argument about money and value in China using this paradox as a central theme. She details rotating credit societies, bridewealth and pigs as specific examples highlighting the socially embedded logic of both exchange and accumulation that tie people together, yet that also give them access to have personal autonomy and/or protection from the extractive power of more dominant classes. For Martin these specific forms of exchange, which centre around kinship and community, are seen to keep 'the disintegrating potential of money in check' (14). This finding echoes that of Polanyi, who argued that 'man's economy, as a rule, is submerged in his social relationships' (2001: 48). Martin also identifies the existence of conversions between spheres of exchange as marked by different currencies – also apparent in the case of spirit money – which she suggests may be another element that helps to keep money's potential for abstraction in check (69). The Chinese view of capitalist accumulation of wealth was also seen to include inherent risk with the

potential for social losses, with greedy gods that could easily take away even more wealth than they had bestowed (77) and architecture whose potential for being influenced by geomancy mirrored the risks and rewards in farming and market exchange (79). As she concludes at the end of her second lecture on China, 'the value that money measures is concrete, time-worn, messily embodied, and socially embedded' (82).

Conversely, in the United States, Martin argues that money primarily disintegrates, paradoxically producing both 'greyness, confusion and feelings of moral uncertainty' *and* intense desire for the accumulation of more money (83-4). She makes this point by briefly tracing the history of Western capitalism and the development of what Polanyi called the 'self-regulating market' independently setting prices and interest rates (89). She goes on to highlight three related processes that have increased 'the dominion of money or models of money making over all else' (90). Using Marx's terminology of 'general illumination' for the extension of both market principles and market models into other domains of life (91), Martin describes the use of industrial production metaphors for the female body and proposals for selling body parts to illustrate these first two processes respectively. In a third related process, money is also associated with infinite accumulation through exchange-value (101; a notion developed in more detail by Sahlins 1974), leading to profit-seeking behaviour that can be harmful to others. Martin describes how the logic of money and potentially infinite accumulation has been adopted by the prosperity movement in the Methodist Church, and how this reflects capitalist logics of accumulation and the flow of money, yet she does not similarly consider the social bonds that could be created by participation in these ministries. In other words, while the social embeddedness of exchange in Taiwan is convincingly argued, its disembedded nature in the United States is not as clear. Parry and Bloch, in their development of the notion of two transactional orders, cite Martin's lectures as a source of inspiration for highlighting the symbolic role of money, but they go further by theorizing a distinction between a short-term market-based transactional order and a long-term transactional order of social reproduction. In capitalist ideology, they argue, there has been a unique 'conceptual revolution' so that 'the values of the short-term order have become elaborated into a theory of long-term reproduction' (1989: 29); thus, the logic of exchange and accumulation continues to be social, but corresponds to a different set

of (capitalist) values. Martin, alternatively, sees the struggle in the United States to be the result of a clash between moral and economic life (108).

Although Martin's argument about the disintegrating effects of money in the United States is largely convincing and captures various elements that could have caused this shift from a (presumed) prior state of being embedded within and controlled by socially mediated forms of exchange, it is perhaps less developed in explaining the mechanism behind the processes of 'general illumination' and infinite accumulation. Martin does mention interest and usury at various points throughout the ethnography of China and the United States and hints at their importance, at least symbolically, but she does not clearly locate this as central to the processes of the expansion of the monetary realm. The philosopher Charles Eisenstein provides a simple explanation that brings clarity to the distinction Martin makes between the embedded uses of money in Chinese society and its seemingly disembedded use in the United States. Interest-bearing money, Eisenstein argues, is the source of many of the ills that Martin describes: 'The imperative of perpetual growth implicit in interest-based money is what drives the relentless conversion of life, world, and spirit into money,' (2011: 77). In this view, the increasing commoditization of various spheres of life is neither intrinsic to money itself, nor an outcome of the presumed lack of social embeddedness, but rather a property of a monetary system that requires a continual return on investment. This proposal seems to explain neatly the situation described by Martin and should be further explored, or at least considered, in anthropological studies of money.

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BORIS PETRIC, *Where are our sheep? Kyrgyzstan, a global political arena*, New
York and Oxford: Berghahn 2015, xvi, 170 pp. ISBN 9781782387831.

What role does post-Soviet Kyrgyzstan play within the new order of globalization
characterized by a major increase in worldwide exchanges and the
transnationalization of power? In his latest publication, *Where are our sheep?
Kyrgyzstan, a global political arena,"* social anthropologist Boris Petrić provides an
elaborate and in-depth account of the contemporary political regime in Kyrgyzstan,
thereby debunking a number of clichés of Kyrgyz identity present in the Western
imagination. Through a combination of personal observations and careful, critical
analysis, he portrays an increasingly frequent occurrence in the contemporary world –
the encounter between newly emerging or reforming states and a body of actors
participating in social change. Petrić contributes to the study of major trends in
globalization and its multitude of flows by drawing an incredibly detailed account of
numerous actors that have come to bear upon Kyrgyzstan's fate and *that* influence the
country's future. Kyrgyzstan is thus represented as an arena of international rivalries
between international organizations, such as the UN, the IMF, WTO or the World
Bank; regional organizations, such as the EU or the OSCE; major national powers *like*
China, Russia and the United States; large international foundations*;* and an infinite
number of NGOs and aid agencies*, all being* pitted against each other *and* competing
in their desire to shape and guide Kyrgyzstan's history. Democratization, *the*
propagation of civil society and economic liberalization are the main tenets of these
'good governance experts*'* who *have* initiated an unprecedented transformation of

Kyrgyz identity. Petrić implicitly paints a theory of globalization as neither a natural nor *an* inevitable occurrence, but a carefully crafted process *that* rests in *the* hands of Western or Western-influenced powers.

The author writes an account of *the* unparalleled transformation of living conditions for much of the Kyrgyz population, which ensued upon the fall of the USSR and Kyrgyzstan opening its borders. An exodus of the Kyrgyz European population and subsequent dramatic demographic changes, the collapse of production and the rise of new business elites emerging from trade and tourism as new sectors of the economy, an increasing dependence on remittances and international aid, *the* flight of *increasing* numbers of rural poor to growing urban centres and proliferating international labour are but a few of these extensive changes and certainly do not complete the list. Perhaps the greatest change occurred in Kyrgyzstan's principal industry, sheep-breeding, which was decimated by reforms suggested by international institutions providing assistance. Through this account, Petrić criticizes the international community for creating *the* conditions for its own existence in Kyrgyzstan by *making* the country dependen*t* on its provisions. *One* theme*,* only touched upon in this work*,* which I hope Petrić will elaborate on in future publications is Kyrgyz agency.

Although emphasizing that globalization plays *itself* out in the *interface* between local and global forces, there is little attempt to account for Kyrgyz agency. In this book, the author describes numerous encounters, often amusing and ridiculous, as well as tragic and shocking, between the local population and the *usually* well-meaning foreigners who came to reform them. Rarely, however, are the Kyrgyz represented as active agents in navigating the*ir* present-day circumstances. It is important, in my *view*, to present Kyrgyz people not as victims of globalization, but as agents in its construction, constitution and transformation. Globalization is a concept, not a fact, and whether or not it is mythical or true is a collective evaluative judgement that changes through time and space. This is a political issue *with* regard to how anthropologists write history and from whose perspective.

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LAURA POUNTNEY and **TOMISLAV MARIĆ,** *Introducing Anthropology: What Makes Us Human?* Cambridge: Polity Press 2015, 330 pp. ISBN 9780745699783.

As the title suggests, *Introducing Anthropology: What Makes Us Human?* is an introductory textbook composed of fourteen chapters presenting an engaging mixture of selected core issues of anthropological enquiry such as 'personhood', 'identity', 'ways of engaging with nature' and 'gender', together with various subfields of the discipline and the contemporary challenges that anthropologists face. Authors Laura Pountney, Senior Examiner and Lecturer in Anthropology at Colchester Sixth Form College, and Tomislav Marić, Lecturer in Anthropology at Heston Community School (both in the UK), effectively describe their professional experience of introducing anthropological knowledge at the pre-university level. With A-level students and teachers in mind, this experience culminates in an informative yet approachable introduction to the field for budding social and cultural anthropologists.

Instead of aiming to provide a comprehensive overview of the field, the authors present a selection of appealing topics ranging from body modification techniques (43), animal rights (118), rites of passage (171) and cyborg theory (220) to public health (296) and explore them in a lucid and engaging way. What makes the textbook even more captivating, apart from its suitably chosen topics, is its focus on active classroom engagement and independent exploration, prompted through the numerous lively activities, discussion points and ideas for personal investigation that accompany every chapter. The text also serves as an invaluable beginner's guide to the often intimidating language of anthropological theory and practice, providing intuitively organized glossaries containing accessible definitions of high-level concepts.

The authors begin their publication by discussing a question that is central to the anthropological discipline: 'What makes us human beings different from all other species?' (3). The first out of fourteen chapters, entitled 'What Makes Us Humans', explains how early hominids diverged from other primates and examines some of the important physical changes that occurred, such as opposable thumbs. The chapter also discusses the intimate connections between human cultural and physical evolution. While this introductory chapter provides a comprehensive survey of fundamental concepts in evolutionary anthropology, all the other chapters focus almost exclusively on the concerns and perspectives of cultural and social anthropology. As such, the book strongly adheres most closely to the British school of social anthropology, and

less on the four-field approach common in the United States.

The remaining chapters explore different aspects of human culture, from different culturally constructed ideas about what it means to be a male or a female and what is the difference between anthropocentric and biocentric view of animals to how people use the body to express their identity. Classical anthropological themes are accompanied with newer topics, providing the student with an understanding of ritual processes, witchcraft and kinship as much as about new forms of communication through digital technologies, globalization and tourism. Contemporary anthropological research and acknowledgement of the contributions of classical anthropology are skilfully intertwined to provide a cursory overview of the field's past and present. The central position of ethnographic research in social and cultural anthropology is reflected in the structure of the book, which includes many summaries of noteworthy ethnographic studies, as well as in the chapter dedicated to research methods.

The closing chapter, 'Applied Anthropology', explores what anthropologists do with their knowledge and experience of fieldwork and how they use their skills beyond academia. While the first part discusses the theoretical aspects of applied anthropology and advocacy, the second part consists of interviews conducted by the authors with different anthropologists around the world who apply their anthropological knowledge in different fields. Assuming that the book has managed to achieve its goal and that its readers have begun to consider pursuing a degree in anthropology, dedicating the final pages of the book to a discussion of the professional applications of anthropological knowledge and of the job prospects for those trained in the field undoubtedly finds a suitable place in this disciplinary primer.

Overall, *Introducing Anthropology: What Makes Us Human?* presents existing anthropological material in a way that is accessible to a wider student audience. The book does not reach the depths of many undergraduate textbooks, but rather contains selected topics which may be of interest and explores them in an introductory manner. Because of its clarity and approachability, the book will be a useful companion to introductory courses to anthropology at both university and pre-university level, as well as to anyone who is new to the subject.

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MICHAEL TAUSSIG, *The corn wolf*, Chicago and London: The University of
Chicago Press, 2015, 200 pp.

The corn wolf by Michael Taussig is a work which forecloses its reviewers from the
outset. One can hardly extract its arguments and lay them down in a familiar format
without succumbing to precisely the sort of academic 'agribusiness' writing that
Taussig roundly deplores; writing which 'knows no wonder' (5), beating into
submission the chaotic multiplicity of meanings that the eponymous corn wolf, in a
nod to Wittgenstein, represents. This is a particularly refreshing, if frustrating,
sentiment for the weary graduate reader with whom Taussig begins his title essay.
That Taussig is variously hailed as a rock star and a radical is testament to his success
amongst a demographic who arrive at university as dreamers and storytellers and
leave as newly qualified custodians of an orthodox, and no less seductive, *knowledge*.
For the ethnographer, this loss is a particularly poignant one, as the magic of
fieldwork is precisely its quality of unknowingness.

In an effort to salvage this sensuous and elusive quality, Taussig adopts a now
infamous style of mimesis which he terms Nervous System Writing (NSW). A
surreal, imagistic collage of theories and stories, Taussig's NSW attends to the
sensual and the bizarre, unravelling categories of knowledge and revealing 'how
strange is the known' (6). This is not a work of classifications; here is a work of
delightful living contrasts, conveyed in a juxtaposition of conversational intimacy and
disoriented, woozy estrangement: 'He really lets his guard down, our old wolf, our
would-be wolf, when he goes further in imploring us to love the strange, be patient
with it, let it get into you, so to speak, and then you will learn what love is – and that
will be how the strange rewards you' (6).

This style of writing is indeed, at times, wondrous, and it transcends mere stylish
analogy. The attention to what is formless, elusive and pre-rational finds its most
brilliant incarnation in Taussig's 'Humming' as 'alphabet soup, wetlands, where all
manner of life forms thrive' (34), citing an unlikely and enjoyable range of examples.
From Winnie-the-Pooh's exclamation of 'Oh help!' to the cries of the Trobriand
gardener, the hum always anticipates a punctuation; it is a 'dialectic at a standstill'
(Benjamin, quoted in Taussig p. 35). This particular conceit recalls Michel Serres's *la
belle noiseuse* (1995), the noisy multiplicity at the pre-phenomenological genesis of
our understanding which we can only apprehend blindly, without reason or evidence.
Noise here becomes method, and for Taussig humming proposes a kind of deontology

114

which anticipates all possibilities and bears implications for one's role as an ethnographer. We are offered a holy trinity in which subject matter, written mimesis and epistemological position are mutually constitutive. It is an interesting and masterful essay, bound together with a motif of noise-as-epistemology that has been fruitfully explored by philosophers, but has not yet received much anthropological attention.

However, the success of this model is rather patchy elsewhere in this collection. Taussig's writing owes a clear debt to modernist aesthetics – one recalls Woolf's dictum to 'record the atoms as they fall' (1923/2003: 150) – and many of the same charges of navel-gazing and insularity that have been levelled at the latter might also be addressed to the former. As Martin Jay once remarked, 'I can't remember another non-autobiography in which the pronoun "I" appears so frequently as it does in your books' (1994: 163). The spurious premises of the Jay–Taussig falling out, in which poet and exegete were set up as arch-rivals, makes one especially unwilling to adopt the role of the aesthetic disciplinarian, but there is certainly truth in Jay's statement. Taussig records sensations and ideas with a self-perpetuating solipsism that quickly wears thin. Consider the following, taken from 'Animism and the philosophy of everyday life':

> Those stripes of the zebra dazzle me. The stripes are things in themselves that have come alive. It is impossible to domesticate zebras and use them like horses, Thomas tells me as we ride along. Might that have something to do with those dazzling stripes? I wonder, and then I think of the stripes on Genet's convicts in the opening pages of *The Thief's Journal.* (13)

The irony of such a passage in the context of animism is that it never quite manages to move outside itself, never quite harnesses the liveliness of the world outside Taussig. The Beat influence is evident far beyond the frequent references to Burroughs; many of these whimsical, romantic encounters smack unmistakeably of Kerouac and Co. heading out for an adventure.

But what of the informant in such a work? Against this backdrop of sensation, ghostly characters slide in and out of view, often making little more of an impression than as a miscellany of names: 'I am cycling through the Tiergarten in Berlin behind Bretta and followed by Thomas' (12). In longer pieces, such as 'Two weeks in

Palestine', we find some brief but insightful interviews between Taussig and people living under occupation, but the piece is largely centred around sensual descriptions of the space ('gazelles! I cannot believe my ears' [124]) and theoretical musings which are rightly, and gratifyingly, critical of the role of the Western anthropologist in such a context. However, at points I am left with the uncomfortable feeling that the real stuff of sociality is barely hinted at, and even that social relations do not occur without Taussig's presence forging them. Take, for instance:

> the forty-year-old man I met in the subterranean market in Hebron selling spices at the same stall all his life and who has never seen the sea, holding my arm, eyes burning, when I tell him I am from Sydney. Although it is quite close, he has never seen the sea because he doesn't have a permit to travel the necessary roads. (114)

What we gain in terms of style we lose in our understanding of the social. But this is perhaps an uncharitable criticism given that the collection is largely not an ethnographic work. Most essays take the form of extended aphorisms. 'The go slow party' reads like a situationist manifesto, outlining a general strike in pursuit of an 'aesthetic and magical' (149) new practice of time. Beautifully written, it describes this temporal revolution as 'a butterfly on a hot summer's day. It speeds up and slows right down to alight on something interesting or beautiful, making it more beautiful' (149). At other points informal advice is offered, seemingly with the graduate student in mind, as to how to conduct fieldwork. 'Excelente zona social' ends with an exaltation of the field notebook and a 'plea for following its furtive forms and mix of private and public' (76).

It might, then, be more accurate to say that this is an ethnography of the ethnographer and his methods. Taussig attests to this in his final essay, 'Don Miguel', which bemoans the fact that 'the famous "method" of participant-observation tends to be weighted toward the observation end of things and, what's more, tends not, according to the profession, to allow much by way of self-observation' (194). However, by the end of his fieldwork in Colombia, Taussig claims, he and his compatriot had 'become objects in our own story' (195). There are the fragments of a useful point in here; it is the anthropologist's burden that their object of study is always inevitably and irrevocably altered by their presence. But this point is never quite hammered home. It is telling that the entry reading 'Death of the author' (158)

in the 'Iconoclasm Dictionary' does very little to address the question; instead, it is a bizarre detour into Foucault's sexuality. The overriding sense is that the chief subject of this collection is inescapably Taussig himself, to whom other people, animals and things play only an attendant role.

The corn wolf is an exhilarating example of the ethnographic method Taussig has devoted much of his career to refining, but its usefulness to anthropology as an academic discipline is perhaps less clear. At times, one wonders if he has written the very ground from beneath his feet.

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MARISA WILSON, *Everyday moral economies: food, politics and scale in Cuba*, Chichester etc.: Wiley Blackwell 2014, xxvi, 232 pp.

Marisa Wilson's *Everyday Moral Economies* provides a timely, readable and clearly argued ethnography on the economic realities of life in contemporary Cuba, seen through the perspective of food provisioning, consumption and production. As Wilson argues, as a state run along socialist principles for over fifty years and seemingly cut off from international trade until the recent development of tourism, Cuba is an interesting case for studying the ways in which its citizens deal with the seemingly conflictual realities of the socialist versus global markets in everyday experience. Wilson calls these 'Leviathans', powerful structures that stretch from the micro to the macro level and which deeply influence human behaviour (her definition is inspired by Latour and Callon, amongst others).

Her main aim is to consider 'how people in rural Cuba rationalize the practicalities of living in this contradictory moral and political economic world' (xii), using food as the lens through which to study how 'commodified and non-commodified provisioning processes are morally embedded' (21), to reveal 'ideal principles of justice and value in Cuba and uncovering how such principles are adopted or counteracted by particular people in the difficult…conditions of their everyday lives' (75). In the socialist state of Cuba, food is a basic right to which citizens should have adequate access through the state. However, the reality in Cuba is that state channels rarely provide adequate nourishment, so Cubans must find alternative means of food sourcing in the informal economy, while justifying them as 'moral' or acceptable in accordance with Cuban socialist principles, such as struggle (*luchar)*, hard work, self-sacrifice and familial and national solidarity.

In keeping with her ethnographic approach, she introduces her ideas in Chapter 1 through a fieldwork experience which demonstrates Cuban food realities (in this case, substandard pizza), the two-currency system and issues of moral and political economies, topics that are to reappear throughout the work. She also skilfully explains her 'positioning' as a field researcher, as the study of food ensures that the ethnographer is 'committed in the body' (Jenkins 1994, as quoted by Wilson). The author also makes it clear that she is working both as an anthropologist and a geographer (considering this is published as part of a Royal Geographical Society series, this is not surprising), in particular using her ethnographic approach to respond to a 'need voiced in geography for empirical evidence to unravel the political potentialities of everyday spaces' (11).

In a subject such as this, it is easy to fall into the trap of either idealizing or denigrating the socialist or capitalist systems, but Wilson manages to avoid this by not critiquing this dichotomization, and also remaining an impartial observer. As she perceptively points out, in her field site of Tuta there is 'a multiplicity of capitalism*s* … and socialism*s* with political potentialities that are not captured by stark binaries between state and market' (13).

Chapter 2 provides an excellent discussion and description of Cuban nationalism, introducing us to concepts that will be essential for understanding contemporary national ideologies that are the basis for moral values today, such as anti-imperialism (first against Spain, and then the USA), and how this has developed the ideal of struggle (*la lucha*). Chapters 3 and 4 provide an excellent ethnography of the day-to-

day perceptions of Cubans under study to their interactions in the realm of food, be it in unofficial and illegal exchanges, legitimate relations with the state, or through state-legitimised means which may seem to conflict with the apparent socialist ideal, such as farmer's markets. Her account of the complex mentalities surrounding foods at an individual level is fascinating, particularly how national ideals are applied in situations that frequently contradict the very foundations of the state, for example, applying the national ideal of being a *luchador* (fighter) to the daily struggle to find food when the state is inefficient, or the use of irony in these conditions to criticize the state while simultaneously defending Cuban socialism.

Chapter 5 reveals how shifts to self-employment have changed individual means of food provisioning, and how these lead to re-evaluations of relationships and roles within the local and state systems. Chapter 6 is dedicated to the production side, considering the moral situation of small farmers within the socialist system, as well as providing an overview of the Cuban agricultural system.

There is no dedicated literature review, and considering the huge selection of (at times, seemingly disparate) authors, this would have been challenging. Instead, Wilson continually backs up her statements and ethnographic experiences with regular quotations from a variety of academic sources. While on the whole the literature is thus well integrated with Wilson's own ethnography and personal opinions, at times it felt as if they were drowning out the author's own capacity to make valuable conclusions from her own work. This sometimes creates the impression that she is showing off how extensively she has read around the subject, or is cherry-picking these ideas without considering their context. Wilson should instead have more confidence in stating her own views, rather than perpetually finding back-up quotes in (occasionally obscure) published literature. At genuinely enjoyable moments in the ethnographic chapters, the inclusion of references in the text jarred with her experiential approach, suggesting that dedicating a separate chapter to these authors might have been beneficial for the sake of flow and clarity.

Despite this, the book is an enjoyable and interesting read for anthropologists and geographers interested in food, agriculture, nationalism, economic systems and their moralities. I particularly enjoyed her descriptions of how nationalist ideals are applied and continually embedded in everyday life. Having combined the study of both nationalism and food in my own research, I enjoyed seeing the same approach in a new context. *Everyday moral economies* contributes to a growing literature on how

nationalisms are actually lived, by considering the complex interactions of individuals with the state at both the local and national levels.

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DAVID ZEITLYN and **ROGER JUST**, *Excursions in realist anthropology: a merological approach*, Newcastle: Cambridge Scholars Publishing 2014, x, 156 pp. ISBN 9781443864039.

Based on an understanding of the world which constrains but does not determine us, a world we understand through our observations and actions, but which nevertheless exists independently of social construction, *Excursions in realist anthropology* asks: 'what if we accept our limitations and start thinking seriously and positively about partial views and incompleteness?' (5) This is a symptomatic question for this intellectually provocative book, which challenges readers to investigate the essence of contemporary anthropology, how it should be practised and theorized, where the discipline is now and where it should or could be heading. This gripping investigation of the fragmentary nature of our enterprise sheds light on the challenges, problems and limits of our methodological and theoretical toolkits, and encourages us to question, rethink, reformulate, reshape and improve them. It should be a seminal text.

Today, strong currents aim to pull anthropologists towards extreme postmodernist, (de-)constructivist subjective viewpoints. These paradigms champion complete context dependency and inundate realism and empiricism with all-encompassing social construction. Zeitlyn and Just provide us with an alternative option. As a technique based on fieldwork, their approach provides an interface between realist and relativist objectives by opposing both absolute positivism and the view of universal social construction.

Stimulated by their own ethnographic experiences in conducting fieldwork in Australia, Cameroon and Greece, and inspired by a wide range of theories, the book's nine chapters, three of which take the form of excursions, provide the reader with thoughtful reflections on a broad variety of topics, offering inspiration for further inquiries. The six previously published articles on which the book is based are still clearly recognizable. The chapters stand on their own and can be read independently. What weaves them into a coherent, interrelated text are philosophically inspired contemplations on the nature of anthropological research and the specificities of its products.

Drawing mainly on philosophy, the authors examine forms of understanding, investigating, knowing and believing encountered both by anthropologists and their interlocutors. Incompleteness is presented as a strength, which allows for overlapping and complementary accounts. In calling for a reflexive, nuanced understanding of reality, the authors recognize and respect the complexity of social life. The sophisticated realism they adhere to is bound neither by the requirements of exhaustiveness and certainty, nor by the pressure to resolve or conceal ambiguity. On the contrary, they include and readily discuss any equivocality that arises. Zeitlyn and Just analyse the possibilities of cultural translation and cross-cultural understanding as a dialectic and heuristic exercise, thus challenging the radical translation problem. Additionally, they offer a unique and insightful critique of Bourdieu's theory of practice and probe questions of culture. The authors also discuss various forms of realism and relativism applied by anthropologists when investigating 'ways of living in the world and modes of attending to the world' (6).

The book convinces with clarity of expression; highly accessible writing meets multi-layered, complex, provocative content. But what is novel about this sharp, inspiring account when compared with other works? Highlighting partiality and incompleteness is nothing new in anthropology: indeed, ever since the ontological turn of the 1980s, when postmodern scholars voiced their criticism in the *Writing culture* collection of essays edited by Clifford and Marcus (1986), it has become a modus operandi in the discipline. It is not the ideas in themselves that are new, but rather their combination, their interweaving into a unique merological approach connecting a biased and subjective standpoint with a realist view of the world. Moreover, this account radically strips anthropology of the illusion of ever being unproblematic and serves as a timely advocate not only for the deliberate emphasis on

partiality, but also for its reduction. Consequently, inevitable incompleteness does not absolve researchers from their obligation of due diligence. The value added by this book lies in the evidence provided. Zeitlyn and Just render visible the presuppositions and assumptions that accompany our work and call on us to develop ways of limiting them, thus 'avoiding the extreme claims either that the problems are insurmountable or that they do not exist' (111).

However, this is not a book in which readers should look for concrete, practical tips. The authors offer us an approach that incorporates their understanding of anthropology into a methodological framework, an overall attitude, but no explicit methods. In fact, some lines of argument are merely touched upon without being investigated further. Zeitlyn and Just do not attempt to fill gaps where currently they have no stuffing and so live up to the partiality they champion. However, through their unusually honest account of their ethnographic experiences, they unmask stereotypes connected to fieldwork and offer readers lessons to remember, such as how Zeitlyn dealt with his difficulties in believing respondents' statements that cocks could lay eggs and Just that boats were women. They provide readers with critical ideas, possible toolkits with which to construct their own product. The book should thus be kept as a companion, a questioning partner in our anthropological work.

Additionally, the authors are very critical of their colleagues, especially post-modern theorists and researchers like Callon, Behar or Spivak. These sharp discussions are an exciting read, and the often well-deserved criticism encourages readers to form an opinion.

If the challenging questions that this book poses inspire debates about a possible future for anthropology, I am certain they will stimulate our enquiries about how we can best understand and embrace our 'otherness' (122) and accept 'discomfort and elements of bad faith' (126). The book offers a different path on which to continue what anthropologists do best: in-depth, socio-culturally sensitive research, aimed at an understanding of 'how different social groups around the planet live and understand their lives' (1). By being 'realist without assuming a single definitive or synoptic overview' (3), the book is in fact a 'manifesto for a "realist" anthropology, for the militants occupying the middle ground' (10).

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