Field Diary

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The Challenges Undisclosed

reflections on invisible experiences of doctoral fieldwork

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All photographs by authors.

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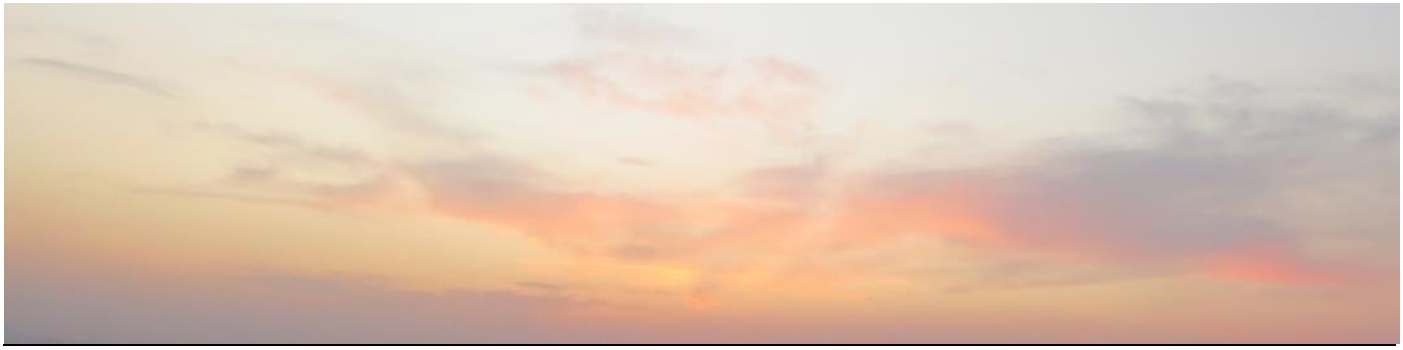

INTRODUCTION

Anna Shoemaker and Liz Storer

The mere invocation of 'the field' conjures up representations of adventure, understanding, challenges, deep connection and loneliness. Alongside these ideas of discovery, the field seems inextricably linked to quasi-scientific notions of precision. We often discuss the field as though it is clearly located and defined; it can be easily entered, and easily left. For some doctoral students, it is the allure of novel findings that drive their decision to embark upon a PhD. Or perhaps fieldwork does not require travelling to a distant place, but is a way of understanding ones own home using new epistemic and methodological approaches. For other unfortunate PhDs, fieldwork is a necessarily evil. In any case, field experiences often prove formative and enable students to contribute towards the academic disciplines which they seek to enrich.

Just as academia is continuously transforming, so too does its relationship with the field. This special issue of Field Diary presents a breadth of reflections on doctoral fieldwork experiences in order to generate insight and discussion on how we might better understand our interactions with the field.

Field sites today are certainly more varied and possibly more complex than ever before. Globalised flows of knowledge mean that however remote the field, the researcher is unlikely to encounter groups totally unfamiliar with research (or data collection in the form of national censuses or NGO surveys etc), and with this, expectations have changed. For those researching in communities experiencing extreme poverty or marginalisation the power imbalances between researcher and communities of engagement present certain issues. Some disciplines have delved into examining the impact of researchers on their field sites more than others. While not all researchers conduct extensive interviews or endeavour to immerse themselves in the field, if it often the case that they encounter situations where it is necessary to consult with government officials, and local people, or employ translators and field assistants. All of these kinds of relationships and exchanges that unfold during fieldwork deserve some level of consideration on the part of the researcher. This is particularly relevant considering once in the field all researchers become representative of the academic community leaving behind a legacy for others to encounter in their absence.

Building local insights and cultivating trustworthy and reliable relationships makes academic fieldwork notably different from, and an important critique of, an ever-increasing number of professional experts engaged in promoting projects such as international development or securitisation. Whereas many of our academic predecessors working in the field eschewed the importance of gaining the trust and consequent protection of so-called 'tribal' groups in remote places, this is no longer the case. Yet, equating successful fieldwork with the researchers unwavering ability to connect to specific groups equates difficulties encountered during fieldwork with a failure of the researcher to build rapport with their hosts – a culture of blame that seems to persist today with the unintended consequence of making it difficult to speak candidly about the challenges encountered in the field.

Furthermore, given the increased circulation of knowledge, and volume of academic research, there is often pressure for PhD researchers to travel to distant borderlands, to places and peoples engaged in active conflict, and to study difficult topics. These state peripheries, and sometimes, state centres, are often the site of processes that overwhelm local groups, and their ability to offer security. Such instability complicates romantic notions of fieldworker negotiation that permeate through disciplines including anthropology, archaeology, geography and other social sciences. In some locations,

researchers may have once been greeted with hospitability, though now these same visitors are not immune (and indeed could exacerbate) security challenges faced by local populations. Institutional channels responsible for evaluating and responding to the field researcher are not infallible. Many institutions are confined to conservative approaches for addressing the needs of researchers and communities of engagement. Of course one-size-fits all guide to the field is not desirable, and we discourage putting unnecessary limitations on the fieldwork process.

This special issue of Field Diary aims to contribute to a growing body of literature pertaining to critical research methodology and strategies of reflexivity and positionality, featuring entries from anthropologists, historians, geographers, political scientists and archaeologists. This is not a comprehensive study of doctoral fieldwork experiences. We recognise there is an overemphasis on contributions from humanities PhDs working in East Africa, a bias which no doubt reflects the networks of us, the editors. We encourage any students interested in expanding on the topics addressed in this issue, or inspired to organise an issue of their own to contact the regular editors of Field Diary.

The topics addressed in this special issue are diverse, but all entries openly and honestly confront the potential for fieldwork to position researchers as both powerful and vulnerable. Recounted here are stories of students navigating the field, and how factors such as inexperience, gender, 'foreignness', and varying abilities to access guidance and support have influenced fieldwork.

In particular this issue explores such topics as the unanticipated complexities of immersing oneself in the field, and the difficulties of readjusting when fieldwork is over. Also found below are reflections on striving to see ourselves through the eyes of our interlocutors. A common theme was the centrality of research assistants to the fieldwork experience. Entries examine the dynamic relationships between researchers and diverse assistants, and the problematic erasure of the importance of research assistants to the knowledge production process. Also discussed here is the ongoing confrontation of privilege, the colonial legacies of our disciplines and even our own ancestral pasts while in the field. Some articles highlight the impact of researcher wellbeing on fieldwork. Others raise unsolved dilemmas, such as what to do when you're in the field and realise that the requirements for consent set by your institution interfere with interlocutors participating in the research?

Of great concern is researchers disclosing that the extent to which their fieldwork challenges are gendered and racialized are neglected, or that conversations on fieldwork experiences exclude those who are not able-bodied. These divisions must not be reproduced. It is important we recognise the discrimination inherent in our own department, disciplines and scholarship to avoid recreating any bias and silence we wish to confront.

The pace of change in many field sites may not be being met with a culture of openness across academic departments or adequate levels of support for researchers before, in, or after the field. Technological improvements and interdisciplinary engagements within and between departments have the potential to enrich levels of contact and support. An important starting point is raising awareness within early-career researchers often used to operating in silos, or small research clusters spread across distant geographies.

Whilst doctoral fieldwork necessitates independence, it does not have to produce confusion or isolation. The freedom of fieldworkers must be maintained, but alongside this, it is an important to open up avenues for students (and others) to discuss their experiences of the field. Of course, the supervisor structure is an important means of doing this, but we also recommend building strong peer networks to offer informal guidance, and opportunities to reflect during and after the field. It is our hope that initiatives such as Field Diary continue to provide platforms for discussions of fieldwork, and foster a culture of supporting our colleagues in the field.

We would like to thank the regular editors of Field Diary, the contributors to this issue, and all those who populate the field.

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WALKING WITH PASTORALISTS

retrospective reflections on the everyday and exceptional challenges of the field

Hauke-Peter Vehrs

Introduction

Imaginations of contemporary ethnographic fieldwork have tended to disregard the conditions of hardship and remoteness characteristic of long-term embeddedness in the field. The classical fieldwork approach of anthropologists has shifted from living with a defined, usually ethnic group, to encompass various field conditions. For many, shorter field trips which do not attempt full cultural assimilation have become more popular. This was not the case for my own doctoral research.

Personally, the idea of a more conventional field experience exerted a strong force of attraction. I wanted to live with a small pastoral group in North-Western Kenya for at least a year. This had been the case since I embarked upon my PhD in an interdisciplinary research cluster at Cologne University. In total, I spent 16 months in Kenya, mainly with Pokot pastoralists in Northern Baringo County. The East Pokot District extends over an area of approximately 4,500 km^2 in the East African Rift Valley, with an estimated population of 133,000 people (Republic of Kenya 2010). Pastoral ways of living are still dominant in the region, though livelihood diversification is observable in many ways (Oesterle 2007, 2008; Greiner, Alvarez, and Becker 2013; Bollig and Oesterle 2013). My fieldsite was right in the centre of the East Pokot District, away from the roads and centres, on the slope of an extinct volcano named Paka. Here, I lived in a pastoral settlement with the household head, his two wives, and their eight children. The mountain "community"[1] on the southern slopes consisted of 42 homesteads and approximately 600 people who speak the Pokot language and to a very limited extent, Kiswahili.

Besides definite means of data-gathering, such as cognitive methods, interviews, surveys, and network analysis, my research also consisted of hanging around with people, laughing about the latest gossip, dozing together in the shade, discussing important community issues, celebrating rituals, staying in each others homesteads overnight, or walking in groups around the region for weeks on end. My fieldwork was successful, and I am confident I opted for the appropriate methods. Nonetheless, the wonderful experiences I had were accompanied by intense situations I was not prepared for. In this passage, I want to foreground two very different types of challenges that I faced: the ordinary, and the exceptional. Ordinary situations included everyday occurrences and ritual participation and frequently involved dealing with unfamiliar Pokot conceptions of sickness and death. Adapting to different epistemologies involved incremental changes of myself. In my case,

1. The term community is used to define the group of people involved in the research, though it does not do justice to the transhumant mobility of people during the year (Bollig and Oesterle 2013), or the regional mobility of individual households.

everyday life in the field took months to adapt to. But while I gradually assimilated to my surroundings, my fieldwork also featured exceptional events that assailed me with feelings of surprise and helplessness. The sudden rupture of everyday life was epitomised by the 2014 execution of an army operation against some "culprits" amongst the group of Pokot I was staying with. Both everyday and exceptional experiences had profound effects on my understanding, my writing and on me personally.

My intention herewith is to progress beyond the mere description of experiences. Following Richardson and St. Pierre, I evoke "writing as a method of inquiry" (Richardson and St. Pierre 2005), as a cathartic process of reflection and a new access point to process my field data and the stories I collected. I also intend to offer some insights into the unpredictability of fieldwork, and my struggles to cope with physical, emotional, and psychological dangers during this time. Through an exploration of my personal experiences, I intend to add to the already existing body of work on "fieldwork under fire".[2] Such literature scrutinises the role of the researcher in the field and in doing so contributes to ongoing discussions of research ethics.

With reference to my own fieldwork experiences, I introduce the analogy of an inner bell, which rings when there is resonance between people, i.e. when a common understanding is achieved. In many situations, when little or no understanding is achieved, there is no such resonance and this bell fails to ring. The extent to which my own bell resonated with various individuals, and what that means for my research, is explored in the following. I begin with a description of my fieldwork and immersion into the field, and conclude with a discussion of how my inner bell was re-tuned when my fieldwork ended.

The Challenges of the Everyday Life of Insiders

> *"[…] we cannot simply walk into other people's worlds, and expect thereby to participate with them. To participate is not to walk into but to walk with – where 'with' implies not a face-to face confrontation, but heading the same way, sharing the same vistas, and perhaps retreating from the same threats behind."* (Lee and Ingold 2006, 67)

I went to the field together with my field assistant who goes by the name Kude. His birth name is Charles Lorot, and we met during the exploration phase of my research. We agreed to work together, and ended up doing exceptional fieldwork. To call Kude an assistant is misleading. After some weeks of orientation, we became partners in fieldwork, and we shared a hut during a year of fieldwork. Though Kude was born in a pastoral family, he went to primary and secondary school and did not spend his youth herding animals. Kude has something of an intermediate character between the pastoral and the non-pastoral, making it easier for me to connect with him in ways people in Paka could not, and for Kude to connect to the people in Paka in ways I could not.

At first, as is typical of many anthropologists, Kude and I set about deciphering the patterns and rhythms of everyday life. Initially, we were sleeping in a tent, until a cow stepped into it. We then asked our host for our own hut, the construction of which he graciously organised. It took some time to become acquainted with the people in the neighbourhood, but slowly we progressed with our work. This was not always easy, but that is to be expected when you are discovering a new place, and people are discovering you. After a few weeks, we had visited every one of the 42 homesteads, and neither we nor the setting seemed so extraordinary anymore. We started to make friends and gradually began to be invited to various events. Being exposed to the community all the time meant that there was nowhere to hide; we were forced to sink into community life. But fortunately, that meant that we were never disconnected from the community. Retrospectively, this exposure, though exhausting, was important for my research.

Walking in pastoral societies is an essential part of daily interactions and became an essential asset of fieldwork. Lee and Ingold (2006) explored the role of walking and its potential for research by recalling Geertz's cockfight experience. They detected various ways of interaction between people and their environment, as well as manifold relations between the researcher and an ever-unfolding field. For instance, after a while Kude and I started to use tyre shoes, mostly for pragmatic reasons. Tyre shoes are very robust, and useful in situations, such as crossing torrents and soaked roads, or walking hard rocky paths and resisting acacia thorns, that often pierce ordinary shoes. Most other shoes would have quickly worn out in such an intolerable environment.

2. See for example Nordstrom and Robben (1995) and Ansoms' (2010) approach to revealing "the stories behind the findings", which focus on emotional dangers and psychological trauma.

With every walk we did, we created new lines of encounter with people and their environment. The more paths we walked during fieldwork, the more legitimate we felt in conducting that fieldwork, and we were constantly drawing new lines. To understand the mobility of Paka people, I had to understand the way pastoralists interact with their environment. Walking, therefore, was a good way to learn how to perceive the environment from a different perspective. In a way, we all shared the same rhythm of walking (Lee and Ingold 2006, 69), and hence created a "shared space" (Wikan 1992, 463; Tambiah 1999, 122). The first time Kude and I went to the market in Kadingding, ten kilometres from Paka, we returned home soaked with sweat. Our host asked us if it had rained on the plains, and burst out laughing when we told him about our exhaustion and the climb up the mountain. After some months, we climbed up Paka chatting all the way with our companions about people drinking too much, the weather, adultery cases, or merely about the food at home.

The relatively slow speed of walking enabled us to observe our environment in a more detailed way and the extent of our observation was larger, compared to faster forms of movement (Lee and Ingold 2006, 70). Furthermore, we had the freedom to choose any route and to go in any direction we intended. During fieldwork, we had to learn how to walk the rocky paths and hidden tracks in the way that other people did, to read the landscape as they do. For instance, we could identify the footprints of certain people by recognising their tyre shoe marks, and learned to find a herd of goats in a jungle of animal footprints. But walking does not merely capture recent events, but also opens the door for "thinking and perceiving the past, present and futures, and combining them in references to routes" (Lee and Ingold 2006, 75). Furthermore, Lee and Ingold (2006, 74 ff.) identify three resonances between walking and anthropological fieldwork. The ethnographer's groundedness constitutes the first resonance. Instead of "looking out onto a research field", walking is more of an embodied experience putting the researcher into the field, through different experiences of walking. In a daily routine, we discovered important stories of the field, and walking became a vital way of gathering and exchanging information. Exceptional walks, on the other hand, were important for collecting inimitable impressions, such as the meaningful walk to secret rituals, or longer trips away from Paka. The second resonance addresses the biographical movement during walking. For us, it meant accompanying our informants along their paths of life, having "insights into their wider ways of life" (Lee and Ingold 2006, 78). Lastly, the third resonance describes "the potential for shared understanding through movement, through walking together" (Lee and Ingold 2006, 79).

Therefore, walking is highly important during fieldwork and merits methodological considerations. Firstly, pastoral Pokot mainly move on foot daily, and hence, walking becomes an instrument of sharing experiences and creating a shared space. Secondly, walking is not merely a form of movement. Walking with the people gives way to the encountering of daily interactions, basic information about the community, and gossip about people, and finally to becoming acquainted with the people and the places of fieldwork. Thirdly, exceptional walking trips create a unique, slow, and extensive moment of discovery. While anthropologists are often caught between the acts of participation and observation, walking is an activity that allows the researcher to assume a role that enables him to observe his surrounding in detail. In the following, I describe how Kude and I "walked into the lives" of the Paka people.

Food, illicit brew, and water

Immersion involves the discovery of, and dependence on, local food. Living with pastoralists, this included a lot of animal products: meat, milk (in many variations), intestines (roasted, cooked, or raw), and blood, to name a few. It was essential to share food with my host family, friends, and strangers[3], though it sometimes felt slightly unnerving to eat unidentifiable food. I had to be taught again how to eat all the food we were given.

Food also included the so-called "illicit brew". This refers to locally produced beer (*busaa*) and spirits (*changaa*). Stories about changaa are famous far beyond Kenya; the negative effects of imbibing are notorious. Knowing this, I tried to avoid these drinks wherever I could. However, busaa and changaa had found their ways into everyday life in my field site. So I adapted, and would have a drink or two, in this case a "doli" (250 ml of freshly brewed alcohol) with those with whom we lived. I decided that if I wanted to be there for some time, I could either adapt to my new environment, or confine myself and my research according to my principles. I decided to eat and drink everything that came my way, including water from local sources. We had sufficient drinking water most of the time, as the people in Paka would take any water accessible and share it with us. We only had a few health problems resulting from the water given to us, apart from one major sickness Kude caught in the beginning of fieldwork. We dug shallow wells if necessary, and the ability to drink any kind of water afforded us the freedom to follow pastoralists everywhere. It also legitimised us as equal counterparts, and helped to close the gap between "us" and people of Paka.

3. With the term "stranger" I refer to people who we met along our way. To research pastoralists on foot implies a lot of mobility along different paths, paths that are navigated by many others. We also frequently encountered new people in designated meeting places, called *kokwö*, or in places where alcohol is brewed and sold.

Sickness

Health was a continual concern for myself as much as for the Pokot. Kude and I personally coped well with the difficult conditions. However, this was not the case when somebody close to us developed a critical illness, and competing epistemologies of medicine came into play.

On one notable occasion, my neighbour Kortin told me about his sick baby boy at home. I asked him how serious it was, and he told me he did not really know. I strongly recommended that he take advantage of the newly constructed road and take his son to the nearest hospital in Chemolingot, a distance of 27 kilometres, before the child's condition deteriorated. My advice was ignored. Instead, my neighbour opted for *Tapa*, a home-based healing ritual. The next day we met again: the boy was still sick and my neighbour still refused to go to the hospital. Kude and I continued with our work into the following day during which a heavy rain shower poured down. This rain shower brought us a lot of joy, since we had been long awaiting the rainy season. The rains were so heavy that the whole area seemed to drown. That evening, another huge shower started. It was at this point that my neighbour came to my hut. He told us that the condition of his son had severely worsened. He was worried about whether his son would survive the night if he did not receive medical treatment.

I was flung into a horrible situation. I had the only car available in the region, and while I would have taken anybody to the hospital at any time, it was pitch black outside and the sandy roads were completely flooded under the deluge of rain. To make this drive now would be very risky. I still recall, sitting in the hut, thinking about the boy, the impracticality of driving under these conditions, and the foolishness of my neighbour. In the end, we decided to make the journey, even though the risk of accident was high. I was incensed, but concentrated on what I perceived to be the road. My body was full of adrenaline. We made it as far as a small homestead in Chemoril, halfway to Chemolingot, which we were familiar with, knowing there was a nurse based there providing health services for rural areas. Fortunately, she was around and agreed to treat the baby overnight. I was relieved, but still felt anger towards my neighbour. Nobody talked during the drive, nor did Kortin and I talk to each other for days. Though the journey and the treatment went well, the bad feelings persisted for some days, until we settled the issue with a small *kokwö*-like assembly[5] and some homemade beer.

My neighbour later became one of my closest friends in the field, and he accompanied Kude and me on a walking trip through East Pokot for three weeks towards the end of the research. This situation showed me how differently health care is perceived among pastoral Pokot and how fragile human life is in an environment where access to Western medicine is either restricted, or not the first treatment of choice. Over time, I came to appreciate these competing worldviews, and fine-tuned my inner bell more towards local contexts and priorities. In general, I accept the importance of Pokot healing rituals in innocuous situations. But the evaluation of risk and danger is different, and challenged my pre-existing ideas of health and sickness, and danger and security, to be sometimes right and sometimes wrong. Situations where my preconceptions were challenged were an important – if sometimes undesirable – precondition for a deeper understanding of Pokot life.

Raiding

Raiding neighbouring communities is a common strategy for young Pokot men to accumulate livestock. Pastoral Pokot are notorious for raiding members of neighbouring ethnic groups like the Turkana, IlChamus, Tugen, or Samburu. The character of these raids has undeniably changed over past decades, transforming from organised events with hundreds of warriors, to small group enterprises. Now fewer than ten men will band together to steal livestock, acting independently of the wider community. Men now only share some of the captured livestock with friends and relatives, keeping the lions' share for themselves. The weapons used have also changed, from spears and bows and arrows to AK47s.

Inevitably, raids happened during my stay in the field. On one occasion, someone who lived at the foot of the mountain and regularly visited the uphill kokwö next to our homestead, participated. A few days before the raid, Kude and I conducted an interview with five elders on the topic of prophecies and their imaginations of the future. When the interview concluded, this friend, who later came to be involved in a raid, joined Kude, the elders, and me as we listened to stories and shared tobacco in the kokwö. This was the last time I saw him.

Some days later, I learnt the details of his demise. Three men had gone to raid a neighbouring community at the southern border of Lake Baringo. They had successfully captured some cattle and goats and had hurried back into East Pokot. But they were pursued by the injured party and engaged in combat. My friend was shot in the leg, and was unable to escape with the others. He called up his friend and said that he could not make it. He said he would shoot himself to escape torture, and that the friend must take his gun home, since it was worth a lot. So that is what was reported to us.

Only after some days did I really process his decision to end his own life and begin to grasp the cruelty of life as a young warrior. A warrior's life is always prone to violence, and death can happen at any time through sickness, sorcery, or at the

4. Later, Kortin told me the baby had caught Malaria, but in the Pokot language the word "malaria" could also refer to other diseases that result in fever and prostration.

5. Legal cases among pastoral Pokot in East Pokot are solved in these neighbourhood councils (Bollig 2006, 35; Bollig and Oesterle 2007, 24).

hands of an enemy. I questioned whether I would ever understand what it means to be a warrior, as I had never really been exposed to this life. My ability to resonate, so to speak, was limited, although I could comprehend the logic of the story. However, the full emotional extent of being in that situation was, and still is, inaccessible to me. I questioned whether I would ever understand what it means to be a warrior, as I had never really been exposed to this life. My ability to resonate, so to speak, was limited, although I could comprehend the logic of the story. However, the full emotional extent of being in that situation was, and still is, inaccessible to me.

Encountering the "slayer"

As we spent more time in Paka we began to attend and learn rituals. One of these rituals is called *Kolat*, and is performed to heal a household member from disease. Kolat involves slaughtering a goat and eating it with close friends and family members as person undergoing treatment is painted all over with a mixture of clay sand, water, and ashes. Women must be separated from men as herbal medicine is given to the afflicted individual. The men sit within the cattle or goat kraal, and one elder distributes pieces of goat meat and intestines to the group. The master of the ceremony must also embody a special characteristic: he must be a slayer, or *kolïn* (see Bollig and Oesterle 2007, 35).

A person becomes a slayer if he has killed an enemy during a fight. The slayer then must undergo ritual cleansing before he may execute certain functions such as leading the Kolat ceremony. When I learnt about this, I was astonished at how many of our friends had gained the title of slayer. Amongst the Pokot the intensity of masculine ideals and the pressures of living up to these ideals left me feeling overwhelmed. Those few men I met during my fieldwork who were freshly initiated kolïn, wearing an ostrich feather dyed bright red in their hair, always created an uncomfortable feeling in me. Such encounters made me think about my own sense of masculinity and how I have no attending desires to be a warrior or a slayer. Of course encounters such as this only challenged my worldview. Nobody else found them particularly unsettling, as it was normalised. For the Pokot it was a fact of life that people kill and die during conflicts. But for me it was not normal that killing an enemy is equated with ritual prestige. My moral compass began to shift, contours of morality and immorality perhaps becoming incrementally less different between myself and the Pokot.

Secret Ceremonies

A few months after we arrived in the field, Kude and I got the chance to observe a special ritual. It was not like any of the ceremonies we had previously attended. This time it was a secret ceremony that was performed following the advice of a local prophet to ritually avert a dangerous situation. I was very excited, I had been hoping to be included in a ritual such as this for some time. Our invitation seemed to be proof of our acceptance.

We went around with the men, from place to place, our ranks swelling as we made our way through the bush, all the while anticipating the ritual. Finally, we reached the location where the ceremony was to take place. The elders had already arrived, as had the goats that were to be slaughtered. As the ritual started, one of the younger elders who was feared because of his connection to powerful forms of witchcraft stood up. He was completely drunk. He complained about us – Kude and me – being present: two boys, who had not been initiated, and were not part of their community. According to him, our presence would cause the ritual to fail. We had to leave the scene.

Kude was enraged. I was disheartened at our abrupt dismissal. We went home with the feeling of total failure, and questioned each other as to whether or not we could continue to stay in Paka. We wondered whether we had actually shared any meaningful experiences with these people. This was a big setback, considering our enthusiasm at being invited to the ritual. Kude and I talked a lot about this situation and about the person that had dismissed us. We thought about his motives, his position in the community, and his relations to others in Paka. We avoided working with this person for some months until the dust had settled.

In retrospect, the dismissal did not have any negative consequences for our ongoing immersion in the field. In fact it turned out that other community members came to see us afterwards to offer their consolations, advising that we should not listen to this one elder, but rather to all the others who did want us to be involved. Later on, we were included almost naturally in another secret ceremony of the same kind. However, the situation illustrates that connecting with people is not a linear process and may be accompanied by setbacks that have big, if passing, impacts on the emotional state of the researcher and their assistant.

Healing

At an advanced stage in my fieldwork the people of my own homestead decided to conduct a healing ritual for a sick baby. Such approaches to healing are common. This one however brought some concern, as it went by the name of *Kisun* (blood). Only men could perform this task. During the ritual, a baby goat was tied to a tree, and I was asked to cut its throat, but not its neck. To heal the baby, the goat needed to remain alive as blood gushed out of its throat and the baby was held in the stream of blood. Both the baby and the goat were strong enough to cry out. To conclude the ritual the goat

was untied from the tree, and as it lay on the ground my host made a deep cut across the peritoneum (abdomen), to remove the heart of the small animal. The heart was then held to the front and back of the baby's chest. The ritual is performed to remove sickness from the baby. Another living thing is required to take on the bad spirit responsible for causing harm to the person. The heart was finally thrown far away from the homestead.

Amongst pastoralists killing an animal is quite a usual routine, and I had taken part in the slaughtering of many animals before. The sacrifice of the baby goat, however, was an exceptional circumstance. For one, the way that we killed the baby goat meant that it took a long time for it to die and we all witnessed its struggle as it did so. Yet the severe sickness of the baby boy put us in dire straits.

After the proceedings, the baby's condition continued to worsen. I urged my host and the parents of the baby to take the child to the hospital in Chemolingot, about an hour drive from our homestead. They agreed, but only after awhile, as the ritual healing was expected to take some time to exert its influence on the baby; if the blood is washed off too early, it cannot develop its full effect. We eventually set off for the hospital and I sadly focussed on the road. This was not the first occasion during which I had driven along the sandy road in a hurry. I knew the way in and out, but still it took a horribly long time until we had reached the tarmac road that led us to Chemolingot. We eventually did get to the hospital, mobilised the staff, and found a doctor. The doctor did some tests and diagnosed the child's illness mentioning that the baby might have to be transferred to the district hospital in Kabernet. Sadly, the child never reached Kabernet. The next morning, we met the parents on their way home, without their baby. They walked by without noticing us. It seemed we had failed. The ritual and the hospital treatment did not work. The despair of this situation seems obvious. Needless to say the events left me disoriented. I found it impossible to rely on participation, empathy, or resonance as ways of making sense of what had happened.

We did not meet the father of the baby in kokwö for some weeks, and Kude and I tried to go on with our work. We often thought about what had happened and tried to make sense of it. It did not become more comprehensible with time just less pressing.

Violence from Outsiders

I was gradually beginning to deal with the everyday challenges and violence in Pokot life when I came to realise that the scale of disruption was so much greater when outsiders and the state intervened in this fragile system. The most intense situation of fieldwork in East Pokot emerged during the first half of my fieldwork, and showed me how rapidly and unpredictably situations can change.

Army intervention

In November 2014, the historical Kapedo conflict between the Pokot and Turkana peoples surfaced again, resulting in a massacre of Kenyan Administration Police officers by Pokot warriors. Pokot conflict with the Turkana is nothing new, and for decades there have been frequent clashes and raids. Bollig (1990, 74–76) describes the history of the violent conflict between Turkana and Pokot, which began in the late 1960s. At this time, Kapedo was still a central hub along a border zone. The conflict resulted in the displacement of many Pokot from the Kapedo area in the 1970s. In the following years, Pokot communities pushed the Turkana north and continued to secure territory from them, though some Turkana stayed on in Kapedo under police protection. Tensions between these two groups have remained high in this region. In November 2014, the situation again escalated when the Pokot controlled all access to the town and ordered the remaining Turkana people to leave. The Kenyan government responded to the call for the removal of Turkana people from Kapedo with an elevated police presence. The resulting clash between the Pokot and Administration Police left 22 officers dead.

Having had been in the field for some time now, I was realising that the killing of people, especially during raids, had become a sad reality of life for the Pokot, repeating itself on an almost cyclical basis, every few weeks. But the intervention of the Kenya Defence Forces (KDF) in response to the killing of the Administration Police officers created an entirely new environment of anxiety, different from that associated with omnipresent cattle rustling. While the raids mainly take place in the border zones between Pokot and neighbouring groups, the army intervention struck right in the heart of East Pokot. Furthermore, the presence of tanks and helicopter gunships imposes an elevated magnitude of threat. From my field diary entries, I briefly try to reconstruct the influence the KDF operation had on us during this highly tense period, which forced Kude and me to leave the region for a week.

October 27th, 2014

Two days ago, we sat together […] in the evening and a lot of bad news was exchanged. The Kapedo conflict flares up again and no vehicle is allowed to reach the town. Even the Chief [of Kapedo] was under fire when he tried to reach Kapedo with a lorry and a Landcruiser. The situation is serious, when Pokot start shooting at Pokot. Yesterday, Kude texted me that rumours go around that talk of three soldiers and two Turkana left dead, and it appears that the situation grows more acute every day.

November 5th, 2014

The situation escalates! We had to leave Paka helter-skelter yesterday, when the Deputy County Commissioner announced army interventions all over East Pokot on the radio, if the ultimatum to return the weapons of the soldiers killed in Kapedo is not met. So far six out of 23 G3-guns have been returned. Therefore, I called the Deputy County Commissioner and asked him about the situation. He mentioned that I should drive to Chemolingot [which, in his view, is the only safe place in East Pokot for the moment] at latest the next day, since it is not yet determined where the army interventions will take place.

November 6th, 2014

The sudden departure from Paka yesterday still gives me collywobbles. We just left and eluded the coming danger. All the others we left behind, merely telling them about the possible army intervention. The drive to Chemolingot was one of the longest of my life. My thoughts went in circles and I tried to imagine what could happen. Sadly, so much is possible. One tank is already in Kapedo and came into operation two days ago. During an interview, we heard four detonations, though Kapedo is relatively far off. Even the operation of helicopter gunships does not appear to be exaggerated. […] With the appearance of the army and the threat of violence, the situation completely escalated and 22 soldiers died so far. Hence, the president came to Kapedo to demand for disarming the culprits, and he set a new ultimatum.

November 8th, 2014

It makes me sick! The situation is acute! I was in Naivasha for two days to deal with some car issues and in the night from November 6th to November 7th the unimaginable has happened. Apparently, soldiers or police set shops in Chemolingot alight, beat people up and imprisoned others. This is what people report, who we picked up yesterday. Chemolingot was a pitiable sight. Though we knew about the situation – and the army vehicles on the way into Pokot strengthened our impressions – the view of burnt houses and streams of people that flee the region left a feeling in me which is difficult to describe. These were merely the obvious sights of the terror of last night. I shortly picked up [a friend's] family, went on to the church to pick the "lost" missionary from Kapedo and his family and moved on to find Kude, who hid with his family in the bush.

The conflict between the government and suspected Pokot culprits went on for a few weeks, until intermediaries returned most of the guns to government officials. During this time, the people in the small urban centres of East Pokot lived in fear of violence from government forces and spent their nights outside the town. The bad news spread all over Kenya.

"Soldiers hunt bandits over AP [Administration Police] massacre."
(Daily Nation, November 4th, p. 1)

"Thirty injured as the military hunts for guns and warriors."
(Saturday Nation, November 8th, p. 1)

"Residents flee as military hunts for guns and bandits."
(Saturday Nation, November 8th, pp. 4-5)

"Pokot leaders declare 'cold war' as military asked to halt arms mop-up. Legislators allege civilian harassment by security officers conducting the disarment drive."
(Sunday Nation, November 9th, p. 11)

"Court asked to stop arms mop-up. Law maker accuses soldiers of brutality against innocent civilians."
(Daily Nation, November 12th, p. 10)

The situation did eventually cool down in the following weeks. Surprisingly, the anxiety we felt seemed to disappear a few days after the incidents in Chemolingot. People quickly returned to their homes and did not hide in the bush anymore. After this incident, I was glad for each day that no news about Kapedo reached us. In retrospect, it surprises me how quickly we once again felt safe in the field, and how little violence ended up reaching us. We were lucky.

This situation differed from the other "everyday challenges" described above as Kude and I were forced to immediately vacate the field and leave the people we dwelt with behind. I completely fell back into my more familiar worldview and did not undergo the same experiences as those who stayed in Paka. When I recall the story of Geertz's cockfight (Geertz 1972), in which the renowned anthropologist hid from police in the courtyard of some Indonesian villagers, I cannot help but think that perhaps I should have tried to handle the army intervention as everybody else did. But really – from my point of view – this would have been impossible and extremely dangerous. I was extremely fortunate to have the option of getting in the car and driving away, though not easy to leave the behind the place and the people I was attached to. It was a disruptive feeling to be safely situated in the next town but disconnected from those who stayed behind. These feelings led me to decide to return to Paka as early as possible.

When we finally did, nothing bad had happened in Paka. The new challenge we had to face was organising the transportation of food (especially maize flour and sugar), since the women had been avoiding the markets in the town centres for some weeks. In this case, the car offered us the possibility to support the Paka people to a large extent.

The adjustment of the inner bell

Kude and I experienced some intense situations as well as many ordinary situations, all of which shaped our time in East Pokot. To every researcher, the memories of fieldwork generate certain feelings – in my case many good ones: warm welcomes; drinking chai-tea; milk and meat; and hours of walking together, aiming in the same direction, and reaching a destination as one. In these ways we became part of the field and the people, as it became part of us. Moved by the urge to understand people and to create some resonance between their perspectives and ours, I tuned my inner bell according to the vibrations of people in East Pokot.

Though fieldwork is often perceived as a time of data collection, here I want to highlight the personal experiences. As anthropologists, immersing ourselves in other people's lives gives us the opportunity to create meaningful ethnographic accounts (as well as in some cases actively changing the course of events). Our sensual experiences, both bodily and mental, give way to a deeper understanding of the local context. Therefore, we use our bodies and minds as instruments of resonance, which must be fine-tuned over time. The more I tried to comprehend the lives of the Paka people, the more I had to leave my prior understanding of the world behind – and it took many months in the field until I understood things better. The process of understanding was accompanied by something I perceive, ex post, as an alienation from myself, at least to some extent. As the researcher adjusts, however, they may later find themselves being out of tune when they return home.

Epilogue: the alienation of the self

When my fieldwork ended, I was not able to switch easily back to the life I had lived before fieldwork. The return to Germany was followed by months of coming to terms with an environment that was so well-known, but now inaccessible to me. Though all my friends and family members supported me, I lacked somebody who could resonate with me. My wavelength could barely ring anybody's bell; nor could theirs ring mine. As this was the case, I put my fieldwork experiences aside, just as I have done with all the situations of hardship I encountered in the field. The stories from the field I narrated became superficial. Killing the snakes, drinking the blood, drinking water from the dams, and walking for 30 kilometres under scorching sun just to have a soda – these stories were easily told, and distracted attention from the hard underlying facts that were more difficult to disclose: the friend that did not return from a raid and who shot himself to elude enemy torture; the sick people brought to our homestead in critical condition; the feeling of driving through the night unable to see the road; and finally leaving everything behind in fear of the army intervention. These things I could not truly share with anybody. The life of an individual person, so highly valued in western cosmology, is constantly under threat in East Pokot. The stories I told at home did not relate the daily struggles of the field.

In other ways, I somewhat unconsciously perpetuated aspects of my Pokot life back home. For instance, my flatmate recalled to me months later that I was somehow alien after my return home. She noticed that when we were sitting in a group of people and I would talk to her, my non-verbal communication would never address her, but only the men in the group. When she told me about this, I reflected and realised that I was dismissing her, as having done research mainly among men, I had adjusted to Pokot social norms wherein women are not so readily included in all aspects of social life.

Lost in translation, I decided to go back to East Pokot for a month in December 2015. I had spent three and a half months in Germany, and had the feeling that I was slowly returning to my "normal" self. But I also had the feeling that I had to re-

return to Kenya to complete my research.

Fieldwork in East Pokot was a paradoxical situation and I have outlined the events that challenged me during fieldwork. Therefore, the question that arises is why was it that I was not totally relieved to return home after such a challenging time in East Pokot? In retrospect, the daily interactions seem less spectacular than the unique hardship events I described earlier. However, these quotidian routines constituted the majority of time in the field. Hence, both the times of hardship and the good and the ordinary moments usher in a very intense relationship between the researcher and the field, comparable to a love affair that captivates us with all the feelings involved.

Back in the field, I felt at home immediately. I arrived together with Kude, our hut was still there, and much to my satisfaction everything was the way we had left it a few months earlier. Our host warmly welcomed us, and we exchanged news, just as we had always done. We lay down in the grass under the Tuwit tree next to my hut and talked and rested. In this moment, I recognised how much Kude and I had become part of this place and its people, and how much it had become part of me.

My host told us about the rains that had started a few weeks previously, and about the circumcision of the girls, who were already released from seclusion. The *Lapan* ceremony (the last ceremony to take place before circumcised girls are released from seclusion) had taken place some weeks earlier. The next important thing was the *Sapana*, the initiation of men, where young men become full members of the community, with all the associated duties and obligations. My host asked us to join in. We agreed, since we had earlier talked about it, but had never before met the right conditions for the execution of the ceremony. For me it also was the chance to gain some ritual closure on my doctoral fieldwork. I decided to become a full member of the community in which I had dwelt for a year. This decision felt right.

For the next two weeks we prepared for the ceremony, organising the oxen that had to be killed, brewing beer, and buying beads for our adornment. We could not wait for the celebrations to start. The day before everything began, elders from the whole area gathered at our homestead. The next morning the ceremony started. It lasted for three full days, and killing the oxen, preparing the meat and serving the elders, receiving the clay head decoration, and all the other procedures, accompanied by very little sleep, proved tough. I must admit that I have rarely experienced something that physically challenged me so much. I was relieved when it all came to an end.

For Kude and me the Sapana ritual also constituted an important stage in our friendship. For over more than a year we had conducted fieldwork together and went through many hardships. During this time we became close friends and found a good expression of it with this ceremony. We officially became age-mates and took on the names of the dead oxen, which are our official names now. This extremely exhausting ritual also served as a proxy for our research, which was ended with one blow.

Within a few days, I left East Pokot, travelled back to Nairobi and then back again to Germany. It was over now. Fieldwork had come to an end and I felt sure I had concluded my work in an adequate manner. The ceremony had performed its task: I became a Pokot man and was now fully accepted into their society. Perhaps, this was what I had been looking for during the last year. However, with the execution of the ceremony I was also relieved from the burden of fieldwork. Our immersion in the life of the people of Paka went along with many rights we obtained over time, but also with many responsibilities that we had to accept.

Retrospectively, my experience of immersion in the society of East Pokot brought about the alienation from myself. I became disconnected from my people back home. Before starting fieldwork, it would have been impossible to imagine that this process could have happened to the extent that it actually did. From my experiences, I learnt that we must reflect as much on our way back from the field as on our way into it.

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FIELD EXPERIENCE IN THE SURVEY AND EXCAVATION OF THE UGANDAN UPPER NILE CATCHMENT

Elizabeth Kyazike

I undertook my PhD in Archaeology at the University of Dar es Salaam (Tanzania), and carried out my fieldwork in Uganda. Despite Uganda being the country where archaeology was first practiced in East Africa (Robertshaw 1990), it has since lagged behind in terms of research investment in comparison to other East African countries. I felt compelled to undertake my fieldwork in Uganda to contribute towards our limited archaeological knowledge of this region. As archaeological research is an underdeveloped area in Uganda, when I was doing my fieldwork I was unable to consult with a local network of scholars who had completed their PhDs in my discipline. While I had my professors at the University of Dar es Salaam, I had no one in my network that had undergone the experience of conducting doctoral research in archaeology in Uganda.

My thesis was entitled *Archaeological Examination of Cultural interactions in the Upper Nile catchment areas: 6000-1500 BP*. The main aim was to examine cultural interactions between the Ugandan and Sudanese sides of the Upper Nile Catchment areas during this time period. This involved undertaking fieldwork on the Ugandan side and comparing my findings with published studies from the Sudan. It took six months to write the proposal while at the same time I was auditing basic archaeology courses for the Master of Arts in Archaeology, given the fact that my academic background was mainly in history. By 2010 I had gone through the rigors of presenting a research proposal to the University of Dar es Salaam, first to the Department of History and Archaeology, then to the College of Humanities and Social Sciences (CASS), and finally to the Senate Committee.

After this I was ready to go to Uganda. Field work in the Upper Nile catchment areas was done in three phases: Phase 1 was a preliminary survey; Phase 2 involved a more detailed survey and some test pit excavations; and in Phase 3 I undertook more targeted excavations. Each phase had its own unique experiences and challenges, but solutions were eventually found in each case, and I managed to obtain my PhD in 2013. In order to show how I dealt with these challenges, I have found it fitting to explore each phase for this Special Issue of Field Diary.

Phase One

Known archaeological sites in the Upper Nile Catchment areas were targeted for this research. In the first phase of preliminary surveys I visited Entebbe rock shelter, Nsongezi rock shelter, Kikubamitwe, and Chobe/Chobi sector.

I identified the Kikubamitwe site from the work of David Kiyaga-Mulindwa (2004) who had found Kansyore pottery here. By a stroke of good fortune my surveys of the site coincided with the construction of the Bujagali hydro-power project. The project necessitated the building of new roads, and within these new road cuts an abundance of cultural materials were exposed. The wealth of exposed cultural materials helped me decide that I should return to this site for the second phase of surveys.

Phase 1 at Chobe sector was challenging. Even before going to the field I had to start with acquiring a research permit and park entry permits from the Uganda Wildlife Authority (UWA) as Chobe is located within Murchison Falls National Park. Permits from the Uganda National Council of Science and Technology (UNCST) were not enough; since it was a national park I also needed park entry permits and had to pay ranger fees.

When I arrived at the UWA office in Murchison Falls I introduced myself, explained the purpose of my journey, showed them my receipts and requested a ranger escort. I had diligently done all of the payments for the park fees in advance from the UWA Kampala office. Unfortunately, when I arrived at the park, I discovered that additional funds were required for the rangers. I was in a dilemma because my research money had been budgeted to the coin, but I needed the rangers to safely enter the park. Murchison Falls National Park is one of Uganda's largest, harboring the 'big five' animals, including elephant and buffalo. With all of my options exhausted I ended up using my own upkeep funds, and off we went into the park.

My first task was to look for a place referred to as Kampala, a place Kiyaga-Mulindwa (2004) suggests was an exchange center for traders from the north. This was a key area since my research entailed examining cultural interactions in the Upper Nile catchment areas. I was totally dependent on the ranger to find the site as Kiyaga-Mulindwa did not publish its coordinates. The ranger assured me it was no problem, and that he knew the exact spot where Kampala was. Unfortunately, our field visit was preceded by the rainy season. The grass towered over my head but the ranger and my field assistants were determined to move into the bush off the main track. After driving into the dense vegetation for over a kilometer without success, I had to make a decision as the principal investigator. We ended up turning back that day. Up to now I am still looking for this place called Kampala - a renowned market center in the north.

Survey along the Chobe airstrip

I had more luck at the Chobi airstrip. Fortunately the airfield had just been graded, exposing a lot of cultural materials in the cuts. Artifacts exposed in erosion gullies provided further clues as to the nature of the cultural materials at the site. The main difficulties here were the wild animals, especially buffaloes and elephants. We were always on alert as they could appear at any time and from any direction, their presence often interrupting our on-foot surveys. The trick we developed was to have our field car close by. When the buffaloes approached we would use the car horn to scare them away, and when the elephants were near we took cover in the vehicle.

The challenge at the Entebbe rock shelter was not impenetrable vegetation or aggressive wildlife, but the site managers, who were using the site as a shrine. Despite our explanations of the purpose of the research, they were very reluctant to grant us access to the site. But then in the middle of our negotiations, a group of people approached the site managers and my field team. These visitors had come to appease the gods at the shrine. In Uganda, traditional worship is usually linked with witchcraft and many people avoid associating with it publicly. The site managers decided they would rather we leave their company than continue discussing permits so as to allow the group of worshippers to continue with their ceremony, away from prying eyes. But before we were allowed to proceed, we were instructed to remove our shoes. This was disheartening as the rock shelter was very dirty, and as there were no toilets, it was not uncommon to encounter feces. I am not a quitter though, I complied, and we managed to conduct a survey. Unfortunately for me though, during the survey we only found Entebbe ware (a pottery style characteristic of the last c.1000 years). Entebbe rock shelter did not appear to have material culture dating to my time period of interest (6000-1500 BP) and it was dropped from my list of sites for further excavations.

Finally we journeyed to Nsongezi in an attempt to identify the Nsongezi rock shelter and Kansyore Island. Nsongezi is adjacent to the Kagera region, close to the border of Uganda and Tanzania .I was concerned about being able to find Nsongezi rock shelter, but I was helped by the local people. Just the mention of Stone Age was enough to get a local person to direct us right to the site. The next challenge was figuring out how to cross the Kagera River to go to Kansyore Island. There was no canoe in sight so we were stranded on the mainland. But I was informed that a man named Kafureka and his family lived at Kansyore Island, and that he was a strong swimmer, so strong that he didn't use a boat to cross. We managed to find Kafureka and I explained to him the purpose of my research. He said there were indeed potsherds on the island, and that he could bring me one to see. Kafureka promptly swam out and returned to us with three potsherds, three beautiful Kansyore ceramics similar in style to Sudan pottery. At this time he also told us about a person called Byaruhanga who has a canoe, and I took his contact information so that when I went back for intensive surveys I could get him to take us to the island.

Phase Two

Phase 2 mainly involved more surveying, collecting some archaeological materials from the surface and limited excavations. We began at Chobe. I had to undertake intensive surveys and excavations at this site in one go because my permits to work in Murchison Falls National Park were close to expiring. In the process of surveying, more than ten sites were identified (see Kyazike 2016). It was archaeologically a success, but not devoid of logistical difficulties.

This time round, the initial challenge was accommodation. We decided to stay at the Karuma ranger's camp. The rangers camp is not, however, enclosed, and animals are free to move in and out. It was decided that male team members were to be put out in tents while the women stayed in the ranger's storehouse. The storehouse had previously been used to keep cement, but was converted into our kitchen/living quarters. Having settled that, we then had to take on the challenge of daily movement into the park, some 16 km from the campsite. The commute was made longer when elephants decided to block the roads with trees. It was not unusual for our workday's to begin by cutting up trees to clear the roads.

Another site that I ended up visiting during Phase 2 was Kikubamitwe. Initial surveys here during Phase 1 were promising, but uneventful. However, I got a shock when the person who was supposed to assist with preparing meals in the field pulled out. This really disorganised me. Fortunately my brother's wife came to my rescue and recommended to me a lady whom we referred to as Mumbejja. I did not know at our first meeting when she was hired but discovered later that Mumbejja was even a graduate. Mumbejja was a blessing to me, doing her work very fast at the camp and then later joining us in the field, eager to learn and assist.

Kikubamitwe camp at the school

But we also had trouble finding accommodation at Kikubamitwe. I started by contacting the defense secretary of the Kikubamitwe village who eventually introduced me to the local council chairperson. We debated about using the chairman's compound as it was so close to the bars and also had a lot of other tenants. Instead we decided to set up camp at Kikubamitwe Primary School. The school compound is just by the main road from Njeru to Kayunga, a very public place. People in the area and passersby's always asked how we could be so courageous as to sleep outside in tents by the roadside. As it turned out the tents we were sleeping in were army green in colour, they looked like they could have belonged to soldiers. This coincidence was a blessing, scaring away potential intruders. I think our 'army camo' helped assure our security.

Herman Muwonge and the author in conversation with a local person at Kikubamitwe camp site

While the campsite was probably a bit too close to Lake Victoria, giving us chilly nights in our tents, the location of our Kikubamitwe accommodations was so central, that we received daily visitors. We regularly conversed with curiosity filled people who came to see what we were doing, allowing us to engage in a community archaeology approach here. Our visitors were always interested in knowing what exactly we were doing, sometimes they would come in to see the artifacts we were washing asking to know what they were, and we would happily explain.

As far as the archaeology at Kikubamitwe, our work was also limited by a big chunk of the site being a no-go zone, sealed off for the Bujagali hydropower project operations. But all in all, the excavations of the three test pits here were not very problematic, especially as the stratigraphy was not too deep.

Next was Phase 2 in Nsongezi. Accommodation was sought at the Nsongezi police post and this time we caught up with Gonya the canoe rider. We set up one test pit on the main land and another on Kansyore Island. It was not always easy to travel to the island in the canoe, under the steam of Gonya, but we managed to complete the survey and mapping and established one test pit. The survey results revealed very interesting finds that contributed to answering my research questions, and this is where I decided to direct my focus for the last phase of my research.

Phase Three

In order to avoid the daily canoe commute to the Kansyore Island, I decided that we should camp on the island instead of the mainland. We were strongly warned by the police that we could not stay on the island because it had wild animals like hippopotamus and crocs. But I was determined to camp on the island since I knew there was already a family living there. How bad could it be?

Well at night we had to put up a campfire to frighten away any wild animals. The fieldwork was also unfortunately preceded by rain, and the Kagera water was like mud. We ended up having to collect water daily from the mainland wells for drinking and cooking, and could only use the Kagera water for showering. The campsite had nothing in the way of convenience. The first day involved setting up a pit latrine and washrooms. The island is also surrounded by rapids and these made me have a constant feeling of water moving through my head. When I left the island the world felt strange without the noise of running water.

At one point the research assistants also became difficult to manage, especially when it came to working on Sundays, which they regarded as a day of rest. Despite my pleas that we should use the day for sorting artifacts, not all team members were in agreement. I also had problems in the evenings when some camp members wanted to engage in risky behavior, like leaving the island to drink. I came up with a curfew of 10:00 pm. Sometimes I would persuade the canoe rider not to take them at all, but since he wanted money, they would convince him in turn to take them at a fee. None of this was in line with my schedule, given that I had limited time and funds for fieldwork.

Traveling with Byaruhanga to get clean water

There were also some issues with who owned the land on which our sites were located. Ownership of Kansyore Island was claimed by the area member of Parliament who threatened to throw us off. The owner of the Island of Deserters, another location we surveyed and excavated at (see Kyazike 2016) was also difficult to deal with. Tensions were resolved when I presented my research introductory letters from the Office of the President to the Residential District Commissioner of Isingiro and also helped by the interventions of the Local Council chairperson.

Finally, improper recording of materials became a problem in one instance. I was heartbroken, until I sorted it out. There was an important artifact that had not been bagged properly, and I could not find it in our collections. I could not rest until I had located it in a bag where it had been misplaced. From then on I was very keen on doing all the bagging and taking all the notes myself during excavations.

Despite all of this, I am grateful to my team. I was fortunate to be able to rely on the assistance of two postgraduate students from UDSM, Herman Muwonge and Godfrey Wamutu. Herman, Godfrey, Ssemulende Robert and Fatumah Mirembe were with me during my fieldwork and witnessed all of the associated turbulence. But I'm happy to say the work was accomplished and I finally graduated in 2013. The struggle was worth it and assured me of my own personal determination and resolve.

The author at Murchison Falls

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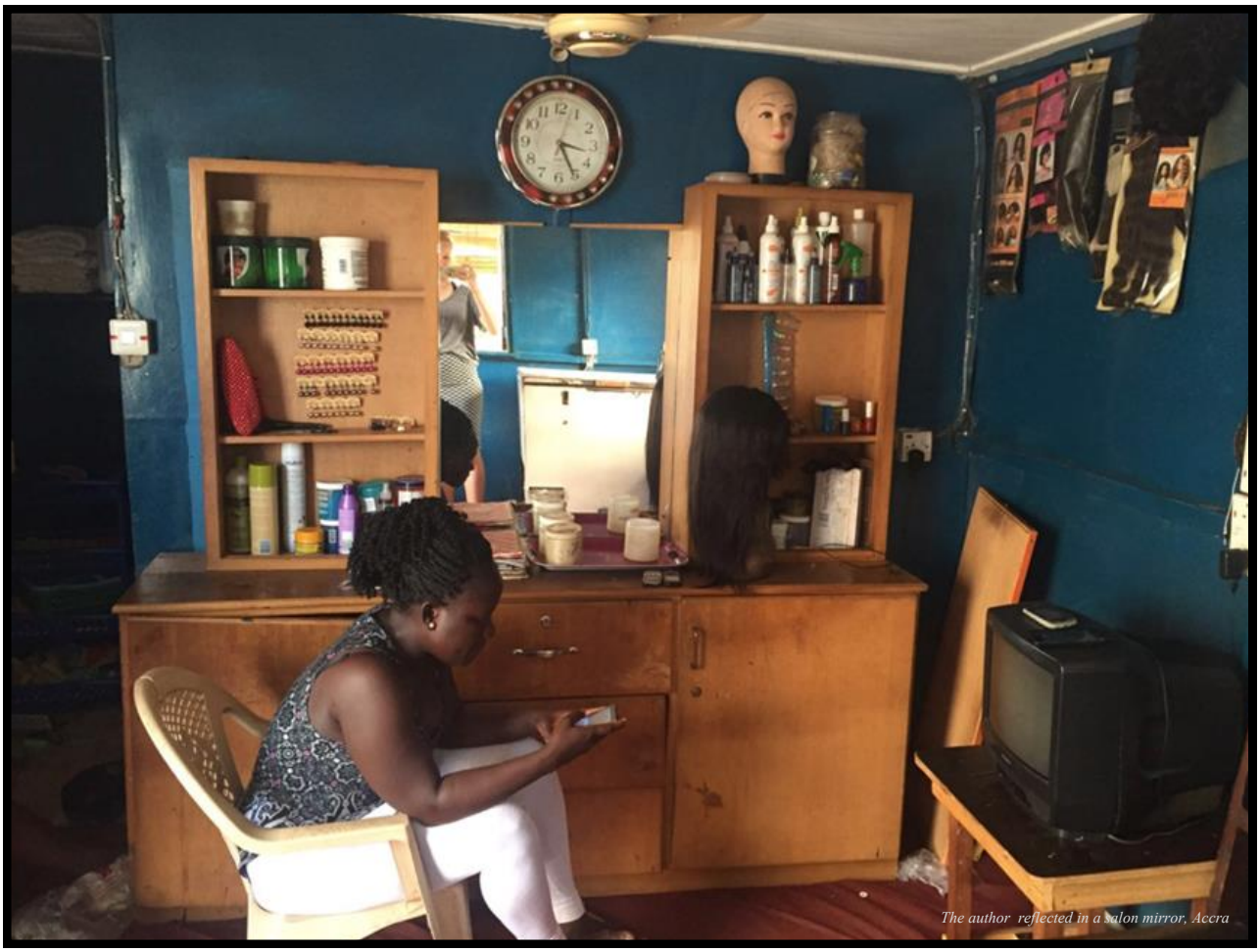
RESEARCHING BEAUTY SALONS IN ACCRA

the difference between reflections and reflexivity

Kate Dawson



The author reflected in a salon mirror, Accra

As researchers, before reaching 'the field' we are told to think through a clear methodology. As part of this methodology, we are told to organise our thoughts about how we might understand our own presence. This is often guided by feminist epistemologies that theorise the field as an embodied space, where the multiple identities of ourselves and our interlocutors shape the nature of all our interactions and thus shape the kind of knowledge we produce about the social world. Much of this begins with tracing out how we imagine ourselves in the grand intersecting structures of race, class, gender, ethnicity, sexuality among others, and how these may play out in particular locations.

As a geography student interested in the spaces of beauty salons in Accra, Ghana, I have followed all such rules. As a young, white, female researcher, I have tried to imagine the history and present-ness of my whiteness and what this might mean in particular beauty salons. In a place where skin bleaching and chemical hair straightening products are advertised on billboards throughout the city, what does my physical appearance mean in the salons? And how do these kinds of questions shape my own movements in these spaces? These are often uncomfortable and complex questions and ones that we may feel able to 'side-step' in other kinds of research – despite the fact they may be incredibly important in trying to understand our relationships in the field. While these may be implicit in the intersecting categories discussed above, maybe this isn't enough.

The beauty salon is a place of mirrors, a place where we are confronted with our own physical presence. In some ways then, it is a place of reflexivity. But what we see in the mirror is of course, how we see ourselves, and not how others see us. For me, this is the biggest challenge of reflexivity. While there may be a mirror that reminds us of our gender, and one that reminds us of our race, what these reflections report back to us cannot reveal what our presence may mean in particular moments to particular people. As researchers in any field of study, we still have a long way to go in seeing ourselves through the eyes of our interlocutors and part of this this may involve accepting the unknowability of such 'seeing.' A good starting point however, may be the continued multiplication and fracturing of the mirrors that shape our understandings of ourselves.

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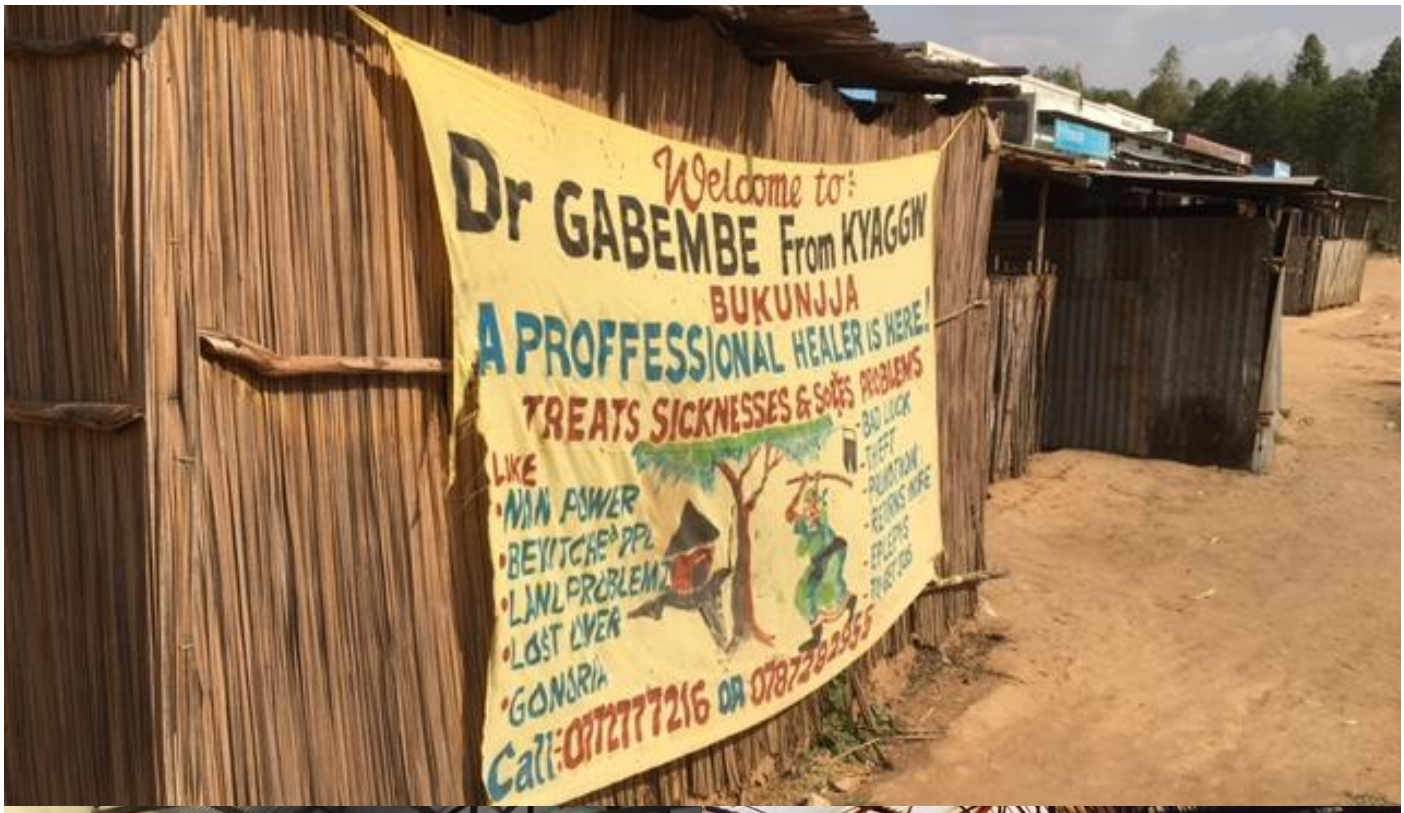

ON INVISIBILISING ASSISTANCE IN THE FIELD

Liz Storer

Research assistants are the unsung heroes of research. Too often this crucial relationship in the research process remains virtually unacknowledged and unwritten. When research assistants are mentioned, often briefly in a methodology section of a write-up, they are spoken of in purely functional terms. The sanitised language used to depict their role suggests a performance of an almost clerical duty, apparently interchangeable across any context. Yet the work of research assistants is not mechanical. Assistants are nearly always more grounded in the social environment of the field than the researcher, and better aware of the moral limits in their communities. As well as being intricately embedded in the knowledge production process of research outputs, these individuals also have real practical needs and will very often remain in the field a long time after we, the mobile researcher, have left. As such, our assistants may have to account for our actions long after our departure. It seemed fitting to me to acknowledge the responsibilities researchers have towards their assistants, rather than 'invisibilising' their roles.

I am currently carrying out my PhD fieldwork, living with Lugbara communities in West Nile. I am lucky to have the opportunity to spend 18 months here to acclimatise to local people, landscapes and rhythms of life. But I am acutely aware that much of my progress to date has relied on the insights and patience of my two research assistants, Osuta and Patricia. My research in part builds on the work of John Middleton, an ethnographer who documented the social structure of the Lugbara in the 1940s, focusing on cosmological beliefs and what he interpreted as accompanying religious practices. While

he conducted research in an era when university departments arguably still respected the timescales of anthropological fieldwork, even Middleton relied on multiple research assistants. Characteristic of his contemporaries, their presence is not particularly visible in his widely-read books (Lugbara Religion and The Lugbara of Uganda) or his many published articles, though reference is made in his less-well known Expectation and Paradoxes of Anthropological Research. Since embarking on my PhD fieldwork in Lugbara communities I have thought much about Middleton's research experience, though I am acutely in want of details on his research assistants.

Epistemological Mis-understandings

My own work focuses on local conceptions of justice across Arua and Maracha Districts of West Nile. My methodology involves tracing events and cases as well as in depth interviewing and general participation in daily life. To date the research process has drawn on many facets of Lugbara existence, which go far beyond normative ideas of 'justice' and its associated apparatus, incorporating aspects of religious experience as well as various forms of alternative healing and curing. Understanding what might actually constitute 'justice' has made it necessary to speak not just to those representing the accepted (or at least expected) moral authority of the court, elders or church, but with figures whose legitimacy is questioned by many Christian-Lugbara, including local prophets (*ba eyo ondri piri*), herbalists (*ba aro nyakuni fe piri*) or witch-doctors (*ojo*).

The latter has proved particularly problematic. From my reading of Middleton and others, I understood that historically the ojo in West Nile used to occupy a somewhat ambiguous, but nonetheless important role in Lugbara society, divining the causes of sicknesses that could not be treated through the rituals of elders. Yet today, one of the legacies of different Christian factions vehemently campaigning against traditional or pagan practices is that ojo's (now translated to witch-doctor rather then diviner) are generally outsiders (often from Congo) who are highly feared and thought to practice powerful forms of witchcraft. The ojo of today may advertise on public banners or in local papers, and are frequented in times of incurable sickness, destitution or need for protection, though being associated with these individuals is stigmatised by Christian communities.

I learnt about the inhibitions inspired by the ojo early on in my fieldwork, through the reactions of my research assistant Osuta as we followed up on the resolution of a mysterious fire. Osuta refused to accompany me to speak to, or even to contact, the ojo drawn into the arson inquest by the community. For several days he brushed off my requests to interview the ojo and we continued with visiting other people. This happened early in the research process and I found his avoidance frustrating. Finally I insisted on knowing why Osuta was making it so difficult for me to speak with the ojo, and he eventually told me his underlying reasons: as a Christian, Osuta believes it is sin to visit a witch-doctor. Of course, confiding in me on this issue made him feel awkward at a time when we were developing a working partnership, and he was understandably hesitant to discuss the stigma Christian Lugbara attach to ojos. Yet as we have built trust in each other we have continued to discuss my initial demands to meet the ojo, as well as his reaction at length, allowing me insights into this sensitive area. Since then, it has been as productive to speak to those who have visited these settings, and to interview healers whose roles are less controversial.

At this early point in the research process, I was unaware of the fear that these individuals invoked locally. I had failed to take account of the cosmological repercussions, and social concern that Osuta believed would follow a visit to the ojo. But discussing this process and working through the meaning of Osuta's reluctance brought deeper understandings of these forms of curing than visiting such a healer at a point when I had little grasp on life in Arua would have done. Similar situations of epistemological difference have continued to confront Osuta and I after speaking with people accused of poisoning (enyata) and various other forms of witchcraft. Often these have come up spontaneously and are entangled with other disputes. But now, I am starting to understand and respect, if not fear, the spiritual power the oja have in these communities. Beyond stigma, the repercussions of visiting these healers are also feared. According to those I later consulted, visiting through research, it was anticipated that we would be in a position for the ojo to make particular demands. If these were not met, death could even result. I have learned to always negotiate the direction of the research together with my assistants.

Repercussions of Invisibilising Assistance

Alongside these epistemological confrontations, there are also practical repercussions that accompany the tendency to invisibilise the central role assistants play in research. In one of my first meetings with Patricia, she relayed her concerns about my offer of employment owing to her experience of being treated badly by NGOs for whom she had conducted data collection for in West Nile. Being an outsider and a white person (mondo), understandably Patricia initially associated me with this type of employer. Though I won't name names, Patricia lamented the treatment she had received from the NGOs

she had previously collaborated with. In one case she had been dismissed for missing a few days of work when she had to accompany her son to hospital for urgent treatment. In another she had been received badly when she questioned the criteria of her research mandate with reference to local circumstances. More generally she was worried about the uncertainty that short-term research work brought to her, not knowing when a contract would or wouldn't be renewed, fully aware of the limited support she had previously received whilst being asked to travel regularly to remote corners of the district.

Throughout my research, I have endeavoured to provide as much clarity and certainty as I can to Osuta and Patricia. I drafted contracts detailing my expectations of their roles, and have been lenient when either of them were sick or had to attend the funerals of relatives. Illness and death are not infrequent occurrences for those living in West Nile. Furthermore I have committed to pay them monthly (rather than daily) salaries, representative of a generous local wage, although much lower than the equivalent in any UK context. When I leave the field later this year, I will try and connect them to other research students, or to the few NGO contacts I have in the region. Of course I may not be able to secure them a longer-term position, and to some extent my actions may serve more to assuage my own conscience. Nevertheless my assistants have communicated their appreciation for my efforts.

Whilst some PhD students do receive generous grants to cover the cost of fieldwork expenses, many of us cover these costs through our own research scholarships, and individuals who are self-funded even doing this out of their own pockets. Being awarded a research scholarship for the duration of my PhD, I am in a fortunate position. Though I am uncomfortably aware that the reason I, and many others, are able to gain from the insights of dedicated research assistants is because of differences in local economies. People here are cognisant of the existing financial imbalances between myself and the average resident in West Nile. As a student people can conceive of my limited means, but as a mondo I am inextricably associated with other Western organisations and individuals who have visibly indicated their wealth to the population here through their material possessions and the places they frequent. To an extent, during my fieldwork I have shifted towards adopting Lugbara conceptions of value, but I will never be completely divorced from my home context, and this is the economy into which my research will be placed.

Accepting the contribution that research assistants make should create space for a conversation to begin about how researchers can ensure employment standards that are not so far removed from Western norms. The conversation must be broadened so that we can address how we, as researchers, can provide as much security and certainty for people upon whom our understandings, which shape our writing and careers, depend. In visibilising the work of field assistants, we can at least endeavour to avert being part of a global system which exploits people in contexts lacking codified regulations on wages and employment, and as such take responsibility for those with whom we work alongside.

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A BUDDHIST, A CHRISTIAN AND A HINDU ON THE ROAD

lessons learned from an unusual fieldtrip to the Cambodian borderlands

Anne Hennings

Like many other researchers, I encountered various challenges and dilemmas during my doctoral fieldwork, but also collected anecdotes that today easily provoke laughter or, in some cases, probably concern. Research in post-conflict and post-genocide societies, such as Cambodia, inevitably yields unpredictable dynamics and many opportunities to unknowingly contravene cultural norms or expectations. My emphasis on land conflicts, ex-combatants, and contentious politics exacerbated the likelihood of doing this. I spent most of the year 2016 living and researching in northwestern Cambodia, in proximity to the former Khmer Rouge strongholds and ongoing land conflicts. Using the 'ethnographic peace approach'[1], I conducted interviews with communities, former rebels and monks, participated in informal discussions, and engaged in talks with government or military officials. Wherever possible, I observed everyday practices of resistance and advocacy politics.

Researching ex-Khmer Rouge in Battambang Province

Yet, under the watchful eye of the authorities, I could not stay – as initially planned – in the communities for longer periods of time. Instead, I chose a flexible approach and conducted extended field trips to certain regions on a regular basis, accompanied by my translator, let us call him Chan and, initially, also by my second field assistant Samnang[2] who mainly functioned as a fixer. Having two assistants came along with challenges I could have never imagined. Apart from age differences, gender, religion, and varying experiences of war and violence also played a key role. As little attention has been paid to these issues in the literature, I want to take a closer look at the challenges of triangular relationships in the field and the dynamics of working with two field assistants of different ages and religions in a post-conflict context.

By referring to my ethnographic experience in Cambodia, the essay addresses relational challenges between researchers and research assistants that may unfold and how this affects, for example, interview situations or the legitimacy of the research team.

1. See Millar, GM. (2014). An Ethnographic Approach to Peacebuilding: Understanding Local Experiences in Transitional States. Routledge, Abingdon; and the Special Issue on Ethnographic Peace Research in International Peacekeeping (forthcoming)

2. Both names changed for security purposes

I will start by briefly introducing the three of us and pointing out some of the advantages of working with a diverse team, followed by a reflection of what went, and what could have gone, wrong. Finally, I suggest potential ways of coping with emerging quarrels within the research team.

The gift of diversity

Finding a research assistant in Cambodia who speaks English well, is willing to work on politically sensitive issues, and is fully committed to frequently traveling to the provinces with a female researcher in her late twenties was an exhausting and time-consuming undertaking. Eventually, I came across Chan. Chan is the same age as me, a masters-student, a father of two, and most importantly, was available for an extended period of time. In addition to Chan, Samnang accompanied us in the early weeks of my fieldwork to help us secure access to resisting communities and local NGOs. Well in his 70s, Samnang is well-connected within the opposition without being partial, which is rare to find in this context. In contrast to other research teams with multiple assistants, who are in most cases students or graduates for a number of pragmatic reasons[3], the age differences within our team proved to be remarkable. Although glad I found two promising field assistants that I got along well with, I was at the same time concerned that the age (and gender) gap between Chan, myself, and Samnang might pose certain challenges. Adding to my apprehension was the fact that Samnang and I could only really communicate through Chan. Yet, in a disrupted post-conflict society, such a diverse team can also be an advantage in terms of building trust with interlocutors.

Age plays a pivotal rule in the Cambodian society, particularly underscored by differences between people born before or after the Khmer Rouge regime (1975-1979). This is not to say that survivors are treated more respectfully. It is rather a question of knowledge and personal experiences of violence, fear, and trauma that set them apart from following generations. As a genocide-survivor, Samnang has lived through 30 years of civil war and volunteered in one of the refugee camps in Thailand. In contrast, Cambodians not yet in their late 30s (such as Chan) have often only limited knowledge of what happened when the Khmer Rouge were in power, as this part of Cambodia's recent history still hasn't been included into school curricula. Many of Cambodia's youth even deny the existence of the genocide.

I also had to pay special attention to the religious diversity of our research team. Being socialized in a predominantly Buddhist society, Chan converted to Christianity in Phnom Penh where he used to live in a Christian student dorm. I grew up in an atheist environment, and feel at home with Vedic philosophy. Samnang, however, had been a Buddhist monk for many years until he was forced to leave the temple by the Khmer Rouge in the 1970s. He is still a very spiritual person and pays the highest tribute to monks. The three of us often had passionate discussions about spirituality and the hardships and hopes of converts. Our religious diversity apparently also played an important rule during the fieldtrips, as I noted in my field diary:

> *After a two hours ride on bumpy roads we finally arrive with packages of noodles, Ovomaltine and sweet milk at the Monks Community Forest in the far north. The charismatic head monk of Oddar Meanchey province has been protecting this forest since 2002 and I heard various heroic stories but also critical voices that he acts in an increasingly authoritarian way – these contradictions make it even more exciting to meet him in person. However, I'm nervous as we arrive and concerned about the rituals we are expected to respect and follow. Chan told me earlier that he doesn't feel comfortable to approach monks according to the customs. Addressing a monk in Khmer translates as "God", which is inacceptable for Chan who says "he is simply not God". Instead, he addressed him with a casual "bong" which is neither appropriate to the monk's age nor his position. I was glad Samnang was with us, who knew the head monk very well and compensated for Chan's relatively disrespectful behavior and my limited knowledge on respective customs.* [June 2016]

That field diary excerpt exemplifies many other situations that occurred during my research because my two research assistants represented entirely different faces of contemporary Cambodia. While Chan stood for a modern Cambodia, where young (and largely urban) people have the right to choose their religion or way of life, and increasingly have the courage to speak up on various issues; Samnang represented a generation still trying to come to terms with the past, and continuing to demand respect according to the customs of a certain hierarchical society he ascribes to.

3. Barrett, CB., Cason, J. (2010). Overseas Research II. A Practical Guide. Routledge, Abingdon, 84

Visiting the Monk Community Forest in Oddar Meanchey Province

My research was inspired by the insights gained into both living realities, realities that are far more complex than I can outline here. I was optimistic that our internal diversity would be an asset in the end, such as it proved to be in the Monk Community Forest. There were many times when Chan too was of great help, especially when he interacted with former Khmer Rouge cadres without any prejudice. Although his family had also suffered under the regime he hadn't experienced any violence directly and thus felt comparatively relaxed when dealing with former rebels.

The pitfalls of diversity and potential hazards

As it happened though, the *disadvantages* of our research team's composition outweighed the benefits despite various team building and bonding attempts on my part. Whilst fellow researchers and friends often reminisce about the good times with their assistants, I genuinely wondered during this two-week fieldtrip if that would ever happen to me. For Samnang, it was difficult to accept a female researcher in her late twenties and even more challenging to show respect towards Chan. At various times, we ended up in awkward situations where we all lost face. More than once, Samnang yelled at us in public, openly questioned my authority, accused me and Chan of offending him, and even secretly recorded interviews without asking for permission from me or the interlocutors. Nonetheless, I continued to encourage Chan to remain sympathetic. However, his patience running out, one day Chan literally dropped his spoon - a serious gesture of disrespect in Cambodia. It proved to be difficult for me, a cultural outsider, to intervene or initiate dialogue when conflicts amongst my field assistants emerged or rumbled on – especially when I too was 'under fire'. Disputes such as this may be too subtle for the foreign researcher to notice, or too embedded in cultural dynamics to be fully comprehended. Interlocutors, on the other hand, often easily sense these internal conflicts and may take sides or feel bewildered. Hence, at a certain point, I became increasingly concerned about the impact of these tensions on our external legitimacy and the quality of the interviews.

Finally, while preparing for the next fieldtrip, Samnang's and my own contradicting opinions on the legitimacy of paying interlocutors money was the metaphorical match that lit the kindling. By then, I had established a proper network so I decided it was time to part ways and take the risk of being denied access to some communities. Luckily this never happened. As Barret and Carson have written, researchers are often surprised about the lack of negative consequences after dismissing a field assistant. In fact, in case of multiple assistants it may not only ease tensions but also the concerns of other assistant(s) that one doesn't 'really care about the work enough to set and enforce reasonably high standards'.4 After parting ways, Chan's mood indeed lightened, his performance improved and from a logistical perspective it also made traveling eases

easier. Reflecting on this later, Chan pointed out that he learned a lot about how to deal with and respect people of different ages, views, and lived experiences. Moreover, he emphasized his appreciation for my leadership style as I demonstrated to him that I continuously 'try to understand the people'.

But if I am honest, it took me a while to reflect and readjust my expectations towards my role and to dissipate doubts about my own capabilities. While sensitive to power discrepancies in the communities I work with, I did not anticipate such disputes in my own research team. Under these conditions field research in an already sensitive context rather resembled a form of 'disaster management', which I felt little prepared for. Academic institutions hardly address these kinds of issues or provide any guidance. With a background in Peace and Conflict Studies and respective training in mediation I had some tools that helped to prevent the further escalation of conflict within the research team. Other than that, I could only follow my intuition, and the advice of other researchers in Cambodia whom I consulted with. Although most of them had conducted only short-term research with just a translator at their side, sharing these issues eased my worries and provided comfort.

Each research context is unique, but employing multiple assistants may result in challenges that other researchers find relatable. Although tiring, I took a lot from this experience in the end and identified coping strategies that helped to prevent and navigate these hurdles. First, one needs to find a balance between addressing certain issues and avoiding situations where people may lose face. The setting, for example, can be a decisive factor, i.e. it may be advantageous to plan to have conversations with your fieldwork team in safe but private places. Second, whereas one may have an understandable urge to sympathise one must also be aware of alliance building dynamics within one's research team. These may be directed against another field assistant or the researcher themselves. Third, and this cannot be stressed enough, the researcher needs to make the responsibilities of each team member as clear and detailed as possible at the very beginning. Finally, as fieldwork tends to be exhausting, frustrating and is anything but predictable, it is also up to the researcher to nurture good team morale and be patient – after all, everyone has his or her individual strengths and peculiarities. Yet if people's behavior is negatively affecting the research process or the internal team dynamics one needs to intervene quickly and appropriately.

Takeaways

My aim was to reflect on the often unexpected and underestimated relational challenges of fieldwork when working with multiple research assistants. While generally unpleasant, internal quarrels also pose certain risks in difficult or sensitive environments. For example, interlocutors may lose trust in the research team and its purpose. In extreme cases, internal disputes can even jeopardize the confidentiality of data when assistants leave the team not feeling bound to earlier agreements or looking for revenge. Thus, for the benefit of all involved it is important to be sensitive to internal disputes, to address these at an early stage, and to not be hesitant to take appropriate steps.

Diversity can both benefit and exacerbate the research process. For instance, it can help the researcher gain access to villagers, but also pose difficulties in terms of trust building. Prominent in the title, I discussed how religious diversity proved to be an advantage for me, and how multiple field assistants were an asset in my research process. Yet, it depends on the research context if differences within the research team have an impact at all. Religion or faith wouldn't have played a role, for example, if I hadn't intended to interview monks. Likewise, personal experiences of war and genocide are probably of less importance when conducting research on fishing communities, for example. I also do not want to overemphasize the struggles of young female researchers – there are clearly certain advantages, too – but gender still plays a pivotal role in many research settings. Originating from and doing a PhD in an achievement-oriented society, I hope that this essay contributes to a more inspiring exchange and open debate on the various difficulties that doctoral and post-doctoral researchers alike encounter. Only by sharing lessons learned – however painful those might have been – can we genuinely attempt to establish responsible best-practices for research.

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4. Ibid, 88

A BRIEF WINDOW OF ~~OPPORTUNITY~~ RISK

research in Northern Bahr el Ghazal, South Sudan, 2013-2015

I've been extremely lucky with my doctoral research in South Sudan: both lucky to have had the opportunity to visit, to make friends and explore local histories, and lucky to have avoided the obvious dangers. There was a brief window of opportunity, and risk, for conducting research in South Sudan that was open during my doctoral period – from around 2005 to last year. Durham was the only institution I found willing to support local research in South Sudan when I applied in 2010, before the South's referendum on secession and five years after the end of the previous civil war. Thanks to the current state and economic collapse, and horrific regional wars, this window is now closed.

In retrospect, my research was uneventful and productive regardless of (or even in spite of) my attempts at planning, risk assessment, political analysis and self-examination. My tactics were possibly slightly counter-intuitive. I fixed on Aweil, and what was then Northern Bahr el Ghazal state, as a research area because of the kind of 'positive negative' of its political circumstances: the state was then governed by Paul Malong Anei, locally (quietly) called a dictator, and who has been the military-political ruler of the area, with powers and connections spreading into Darfur and the Central African Republic, since his days as a rebel commander in the Anyanya 2 and then Sudan People's Liberation Army from the 1980s onwards. Malong has now gained further notoriety since the civil war restarted in 2013: as Chief of Staff of the ruling SPLA faction headed by President Salva Kiir, he has been described by both academics and commentators as a war criminal, responsible for organising the ethnic militias (known as Mathiang Anyoor) that carried out most of the massacres in Juba in December 2013 and across Central Equatoria this year. But over 2012 and 2013, I persuaded Durham that a relatively stable dictatorship is a form of security, and this was true at least over the course of my research to December 2013.

The most useful preparation that I did for this period were days of Googling, reading South Sudanese political gossip on forums, and old news reports on local chiefs and other personalities, and building a social network in Juba over nearly a year, while I worked on the South Sudan National Archive project. One of the friendship groups I made happened to mostly be from Northern Bahr el Ghazal, and these young men and women were my security advisors and research mentors over the course of the project, became my landlords in Aweil. I also made friends with researchers who had worked in Aweil before me, most particularly Martina Santschi, with whom I would be mistaken throughout field research.

These international and local friends were crucial to my sense of self and my understanding of what I was doing. They sat on my ego and helped me avoid an Indiana Jones complex while I worked and lived in Aweil and surrounding villages over ten months with my leopard-stickered motorbike: I needed to put myself in a wider community, and see my concomitant responsibilities, at all times.

Scenes of South Sudan

Abuk's house

Even so, my research was in retrospect (and I'd say is generally still) remarkably ignorant. Likely this is a feature of all social research by abstracted white researchers in Africa, and part of the reason why our academic colonialism should be superseded by local intellectuals. But I came to so much very late: the fast-changing local power dynamics of Malong's regime and ambitions, the corruption and quiet political violence of Aweil, and – most significantly – the growing recruitment of under-employed young men into Malong's part-time paramilitaries that became the basis of Mathiang Anyoor. But at the time, before the war ignited at the end of 2013, this military service was generally understood among the chess group I played with, the families I lived with and the friends I drank tea with as a natural route to at least some income, and part of local men's obligations to the SPLA, if not to the generally disliked Governor Malong. As one of my friends, Dhieu, commented when I returned for a visit in mid-2015: when I was in Aweil in 2012 and 2013, 'we were already in war – we just didn't know it.'

I've since been working as a researcher on various projects within South Sudan, and continue to struggle with the emotional and ethical legacies of this research in Aweil, including my continuing responsibility towards, and fear for the safety of, my friends as many of them move back to Darfur and Khartoum to escape famine and forced recruitment. My inability to protect, defend and provide for the people who did so for me is painful, particularly as I publish research based on my time with them.

Scenes of South Sudan

Train line at Wadweil

There's likely no resolution to this, other than the consolation of reading the work of Wendy James, as she worked through the same personal and professional pain during the previous civil war over 1980s and 1990s, as I move through the long-term experiential and ethical legacies of pursuing work as a white researcher in East Africa.

Scenes of South Sudan

Chiefs court Mayen Ulem

THE GOOD, THE BAD AND THE UGLY

Stéphanie Perazzone

This text discusses three key challenges I have encountered during my doctoral fieldwork conducted in the Democratic Republic of Congo (DRC) for several months spanning between September 2014 to April 2016. My doctoral dissertation seeks to provide a set of ethnographic explorations of daily state-society relations in three key urban centres of the DRC: Kinshasa (the Congo's capital), Lubumbashi (it's industrial mining hub), and Goma (one of its most conflict-affected cities). My approach hopes to draw attention to processes of state formation at the 'meso-level'. This level of analysis differs from that employed by conventional scholarly and policy perspectives, which focus on non-state actors, international and domestic elite politics and grassroots civil society organisations when channelling external development assistance to the 'fragile states' of the global south in the design, programming, and implementation of their policies on the ground. My project aims instead at dissecting and analysing the multiple sites where the 'state' is routinely re-enacted, re-created, and re-legitimised, in particular within the daily interactions between generally overlooked individuals such as ordinary Congolese residents and street-level bureaucrats. The activities of the latter, in particular, continue to be perceived in international relations (IR) as irrelevant at best and parasitic at worst. But their sidelining in this way—I argue—works to relegate 'real' and 'everyday' state practices to the background of research[1] agendas in comprehending 'the state' as a transnational phenomenon. Borrowing from an eclectic set of theoretical, conceptual and analytical tools from history, IR, postmodernist thought, and anthropology, my work incorporates various methods from archival research (at Brussels's African Archives) and ethnographic interviews[2] to 'grounded theory'[3] and a strong concern with Clifford Geertz's 'thick description'.[4]

In this short essay, I reflect on what 'doing' this kind of unconventional (within IR) research entails. I hope – in particular – to honestly unravel a few practical issues that one may encounter while engaging with a 'sensitive' research terrain characterised by long-lasting conflict, extreme poverty, constant external interference and a toxic colonial legacy. In doing so, I begin with the title of Sergio Leone's 1966 Spaghetti Western film – The Good, the Bad, and the Ugly – not in order to narrow the diversity of experiences 'in the field' into such labels but, rather, to provide a framework for considering what 'doing' fieldwork in the Congo entails. Work that, yes, is sometimes ugly or bad, but also – I hope – good.

I started my research in the Congo as a doctoral student in a quite unique position. Not because I am gifted with any particularly spectacular research skills – I am not – but because I happen to have nurtured long personal and emotional ties to the Congo, its history and its inhabitants. Although I was born in France, I grew up in Kinshasa for many years, on and off as the security situation dictated. My point in bringing this up, however, is not to navigate my personal or phenomenological experiences of the countries I have researched so far, but to dwell instead on the fact that, unlike many other [western] researchers, practitioners and scholars who may experience 'developing' countries as intensely new and foreign, I picked the Congo almost naturally due to my familiarity with the site. Although I learnt a lot more from the Congolese than from my political science books, I did not 'land' in the Congo as a complete stranger. I benefitted from my previous experiences there while doing such things as arranging swift travel plans and data collection, and finding my informants. Though there were challenging moments, it also just felt like home. I have most likely not been challenged in the same ways as other researchers who may have experienced difficulties in adapting to a new environment and conducting solid fieldwork. Not only did I naturally benefit from the assistance of many of my friends and family, I also knew how to 'navigate' the Congolese urban terrain. I already knew how people – expats and Congolese alike - would react in various situations, how to negotiate appointments, as well as access to contacts and information; I anticipated many issues and I moved fast.

But though I was then indeed lucky, not everything went smoothly. It was quite hard at times and I often wondered what to do ethically, for my security and for that of my informants. I didn't know because I received limited training for this while completing my Masters' degree and as I started the PhD. I attended compulsory 'research methods' courses but learnt little about the 'hows' of conducting one-on-one, focus group, ethnographic, or semi-structured interviews. This got worse with regards to providing useful advice on how to properly handle certain issues such as direct exposure to violence, ensuring sources' anonymity, war, widespread poverty, colonial legacies, white or western privilege, or being a female researcher in patriarchal societies where codes and norms differ from one place to another. Choosing the right in-

surance package, handling painful periods, as a female researcher, where there is no bathrooms or medicine available, having to lie about your marital status to avoid sexual harassment and other uncomfortable situations, or how to cope with loneliness and stress were other daily concerns I had to adapt to. Of course, there is only so much a teacher can do in classrooms where only a handful of students are planning on taking a career path that will lead them to conduct research in non-western, sometimes unsafe, environments where white privilege is not only striking, but embarrassing. But appropriately covering 'the basics' – insurance companies, anonymity, ethics, research methods - and engaging openly with delicate ethical, security and female-related issues as well as intellectual discussions over self-reflexivity and decolonising research would have been helpful. **The bad.**

Disturbing things did indeed happen in the field. With a history of colonialism in my own family and the eternal looming ethical and moral danger of developing a personal sense of the 'White Man's Burden', decolonising research methods[5] and the 'reflexive turn' of critical ethnography[6] have not merely occupied my thoughts, but transcended my everyday encounters with Congolese friends, acquaintances and interviewees. Coming to the Congo in a bid to narrate and interpret my informants' 'life stories' while investigating the smallest nooks and crannies of their everyday life, made me uncomfortable on many occasions.

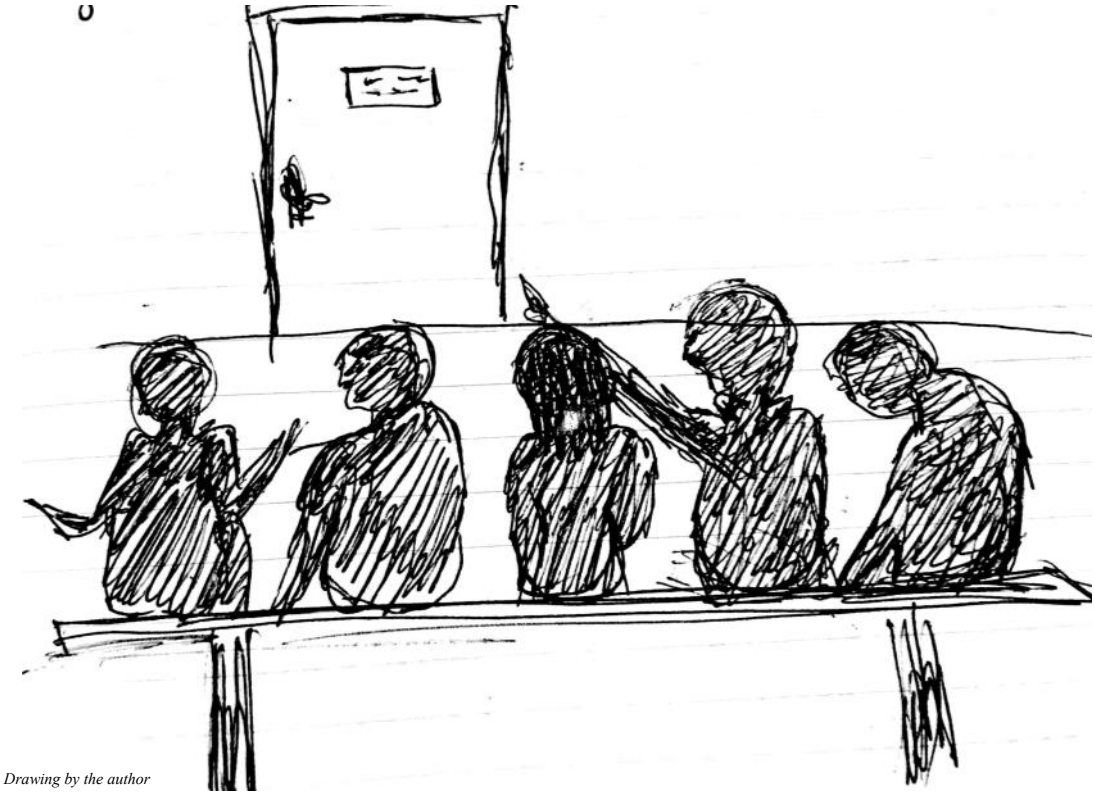
Take Théodore[1]* for example, a local neighborhood chief in Kinshasa I visited a few times in 2014 and 2015. Sitting at his office, right across is desk, I politely introduced myself and asked 'whether it'd be ok to discuss' his job, his routines, his everyday life, to which he replied, not without a hint of sarcasm: 'Right, but… shouldn't you [as a westerner] be teaching me [a local Congolese civil servant]?' This exchange perfectly encapsulates both the ironies and ambivalences of a typical postcolonial 'Self/Other' encounter and the very issues my thesis grapples with: the legacy of a long history of international intervention where key actors have circulated the idea that the 'locals' do not know what they are doing. The latter is something that also emerged from the expat community's narratives about the Congo and its people. Large numbers of expatriates working in the humanitarian, development or business sectors are often undeclared racists and hold a strong sense of self-importance, while failing to question their own ignorance and prejudice. Disturbingly, much like the Belgian colonists in the 1940s declared 'Le noir du Congo exécute avec bonne volonté et souvent avec enthousiasme les travaux ordonnés par l'Européen […] En résumé le noir du Congo est très sensible aux efforts que l'on tente pour le civiliser'[7], I heard too many Europeans complain that the Congolese were 'uncivilised', 'lazy', 'thieves', and being 'all corrupt' they were destroying what little civilisation the colonialists had ever brought to them. Taking the absurdity a little further, I once heard, in a popular bar in Kinshasa, that the place had been 'Congolized'. According to this – obviously white person – this meant too many Congolese people were hanging out there, therefore outnumbering the 'white' crowd. And not in a good way. But then again, here I was, a white privileged European studying and scrutinising the lives, thoughts and actions of the foreign 'Other'. **The ugly**.

Quite self-aware of the situation, I thought about 'decolonising' my own research process. As I sat down with my many interviewees, I purposely attempted not to monopolise the conversation nor to direct it too rigidly around the specific topics I was aiming to discuss. The point, instead, was to let people talk as much as they wanted about anything they felt was relevant, without interfering, unless I knew I needed to dig further into an idea they themselves had developed, or in cases where it'd be critical to gather specific details on a certain set of practices, texts, or habits. Because I had chosen to conduct ethnographic interviews, this proved easy enough as the method does not necessarily require showing up with a list of prepared questions, but rather to allow the conversation to unfold as interlocutors wished. In the same vein, other ethnographic methods include 'following' things, people, and stories, thereby leaving space for my informants to lead the discussion, ask me questions, or take me to places and people they offered to introduce me to. By way of example, here is a short excerpt from an ethnographic interview I conducted with a young college student in Lubumbashi:

> *Him: 'I wake up every morning at 4.30, then I pray from home, or if I have time, I go to the church. After that, I bathe, and then I took the habit to drink 4 glasses of water before I leave. If there is some bread, I will eat the bread. That's it. I walk to the nearest bus stop, and take the public transportation to come all the way up here. […]*
>
> *Me: Let's pause on your morning routine, when you say 'I bathe', does it mean you have access to running water?*
>
> *Him: No. Because we live in a peripheral neighborhood we don't have running water here, so to shower we use the water from a well. We took the habit to build wells in our own yards. […] The well is ours, but we help other people who can't afford to build one. Just in our parcel, live three families […] we share that water with them for free. The Régideso [the state-owned water company] has not connected our street to the*

*Name changed for purposes of anonymity.

municipal water network yet, but we have hope because we saw them do stuff with Chinese workers around here. They work together with the Régideso by providing PVC water pipes. They already installed a few of them around here, because some of our neighbors now have faucets in their yards. We use them too if we need drinking water because the wells do not give us clean water.'



Drawing by the author

In addition to these formal and informal interviews, discussions, and participant observations, I decided to start drawing a few sketches from the various locations I visited. My drawing often coincided with long periods of time spent in offices and corridors waiting for civil servants to meet with me. The 'politics of waiting' in the Congo has long become something of a lifestyle. This served various purposes, from killing time and remembering routines and atmospheres to replacing photography when it became too tricky. These visual tools helped me recollect, especially in retrospect, the details of certain situations and contexts, including sounds and odours and the general atmosphere of the spaces I encountered.

Police officer at his desk in Kinshasa. October 2015

As I was once drawing a dynamic scene at the Mayor's Office, one of the employees invited me to take a picture instead. He posed at his desk, along his other colleagues and his secretary and the result was absolutely stunning; not for its artistic quality but because it both encapsulated the image of that 'meso-level' I sought to document and came to usefully complement the set of unconventional methods I attempted to put together.

Contrary to what Théodore told me, it was me who had everything to learn from him.

This does not mean however, that our – and other western researchers' – views stand completely at odds with 'theirs'. My preliminary findings show instead that many aspects of the western registers of 'modernity', processes of territorialisation, securitisation, bodily controls, and procedural approaches to state-society relations, all tenets of the Weberian state ideal-type, occupy a central role in the discourses, narratives and actions of the Congolese. Having to cope with logistical, infrastructural and financial scarcity and a political elite that turn a blind eye on them, citizens and street-level bureaucrats alike come up with creative solutions that bend time and space, and, echoing Zygmunt Bauman's concept of liquid modernity[8], may have catapulted themselves somewhat ahead of western societies in many ways. **The good.**

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Archaeology department

National Museums of Kenya

Working through monotony

Nik Petek

During my last fieldwork from January to May 2016 I had a ruthless case of monotonitis. Although not a real disease or infection, it was an annoying affliction. And getting rid of it, as the work went on and on (and on and on), became a daily struggle and a fight that was in itself exhausting. I doubt that I am the only researcher experiencing monotony, as going through masses of data is for the most part unexciting. In fact, it is not the first time that monotony got to me, and I don't know anybody who would find their work interesting day in and day out. But this time it was different, for it happened while doing something I love.

I arrived in Kenya mid-January to go through the material I had collected during my last two years of archaeological fieldwork. There was a lot of material, and a lot of details to record from each piece of pottery, bone, and stone. The intended goal of this lab work was to compile a large database which would serve as a pillar around which a PhD thesis could be built. I set up my workspace in the archaeology lab, which would turn out to become my hermitage, at the National Museums of Kenya. I admit it is a marvellous place for an archaeologist to be, though the storage halls are slightly dim, making the Nairobi sun that much more inviting. My plan was to work every day from 8am to 5pm, and use the evenings for a bit of writing or socialising. Expecting I would wear myself out and would need to give my brain a breather, I even planned week long holidays and made promises to start salsa dancing again. My aspirations for work/life balance didn't quite materialise though, and as the days passed, my motivation dwindled. Monotony got to me.

I'm defining monotony as that feeling of struggle that comes from doing the same thing repeatedly with little to no variation. You strain yourself to start your work each day seeking any distraction, because anything is potentially more exciting. Time passes immeasurably slowly, and filling your waking moments with random pointless endeavours becomes your hobby. I should have anticipated monotony would become an issue as I entered into a hermit like state, sorting through some 3000 pottery sherds, 3000 lithics and 900 beads. I had hoped I would be guarded against the threat of monotony with music at my desk, and the possibility of socialising and salsa. But I wasn't really prepared.

The music I brought with me was of the wrong kind and there wasn't enough. Most of the tunes were calming piano instrumentals, songs I hoped would stimulate my intellect while working. As it happened the work became so unengaging that I turned to loud upbeat music to keep me entertained. Eventually, I bored with that too. A Kenyan radio station I found later was a great discovery… for a few days. It mostly played Euro-American music and was refreshingly lacking in aggressively heteronormative DJ chatter. But like most radio stations these days, it adhered to a formula of repetitive playlists featuring top 10 songs. Needless to say, I stopped listening to that too.

Enter the podcast - a consistent and abundant source of amusement. I listened to them intensely, going through more podcasts in that first month than I had consumed in my entire previous life. I learned interesting and obscure facts (thanks to the Guardian and the BBC), I enjoyed exquisite story telling (kudos to you Serial), and was rocked with belly laughs (courtesy of Ricky Gervais). There was also the news. New news each new day. In podcasts I found salvation from the monotony of my work.

Then, big moment. The museum lab finally got WiFi. I decided to subscribe for a free month of Spotify Premium and it was a great decision. This was in the last month of my lab work and I was scraping the barrel to find any motivation to do work. Being able to listen to my playlists, to feel the groove, and to access a range of artists including the Bee Gees contributed to me 'staying alive'. You could tell by the way I did my walk that I was happy. This music streaming service was a huge productivity booster and restored my will to complete the finds analysis. Music comprised a large portion of my strategic defence against monotonitis. I armed myself with music that was varied, upbeat, and engaging, as the work itself was not.

I also found sports activities hugely important. With exercise I would feel more energised and focused, I was more productive, and decisions were somehow easier to make. I specifically remember an increase in the amount of work accomplished and an increase in the speed at which it was done while I was staying at a friend's apartment for a few days. I even had energy to write in the evenings, which was a rare occasion. My friend's apartment compound had a gym that I would use every day. I tried to maintain an exercise regime, and on the days I did I felt more accomplished, my mood was elevated.

My outlook was a lot more positive at the beginning. When I first arrived I had plans to be really active in Nairobi, I was going to go running in the arboretum (local park) and take up salsa dancing. However, it ended up taking me several weeks to get organised and figure out where and when the dancing events were being held. Much to my dismay, travelling there at night in a taxi turned out to be very expensive. I also didn't run in the arboretum until my last month in Nairobi, as I was always returning home too late from the museum. Nairobi is a city that does not cater to pedestrians, and where traffic jams will make your way home three times as long in a vehicle. I also did not find it easy to access green spaces where I could comfortably exercise while in the city. They are few and far between. My time in Nairobi showed me how important exercise is for me and for my thought process. Once I managed to start moving more regularly though, I became more productive.

Another necessity to fight back the monotonitis were breaks from work. Holidays served to dissipate the dreary feelings that accumulated with the analysis of thousands of artefacts. They allowed me to forget about work and feel invigorated. Returning back to work was easier after a holiday, but that good mood always waned. While the first holiday revitalised my motivation for over a week, my third and last holiday saw the monotonitis return within 24 hours.

I also bribed myself with smaller breaks throughout the day to keep me going. Towards the end, I was operating on a schedule of one hour of work followed by 15 minutes of something else. During 15 minutes I might treat myself to chocolate or read a news article. This would make me use time more efficiently, as I would spend less time in the evening and the morning trawling through the news, getting to work earlier and getting to sleep faster. This would also break up my day into smaller "tasks", giving me a feeling of accomplishment after each hour.

Finally, socialising was imperative to my survival in the dark and quiet museum. Days spent working with friends turned out to be more light hearted with the occasional conversation and banter entering the museum space. A drink after work made for a pleasant change of pace. No matter how much I tried though, by the final two weeks in Nairobi, I struggled to do any work. The well of motivation and willpower ran dry. I would get to work early, but spend the first half hour on my phone and computer avoiding what had to be done. I extended my 15 minute breaks to 20 minutes, and then to 25 minutes. My apathy towards my work was palpable.

I find that many young researchers underestimate the importance of motivation and will-power to keep oneself going through an enormous task such as writing a thesis. Although I tried to keep my motivation and productivity in tact by listening to music, exercising (occasionally), and socialising, I still managed to run out of gas. That was surprising to me. No matter what, it became monotonous. So lessons I'm taking away from this are to plan shorter lab trips or to intersperse lab work with other things, like writing. I also know that I can't neglect to maintain a work/life balance. Finally I learnt that I still love my job, even when I loathe it.

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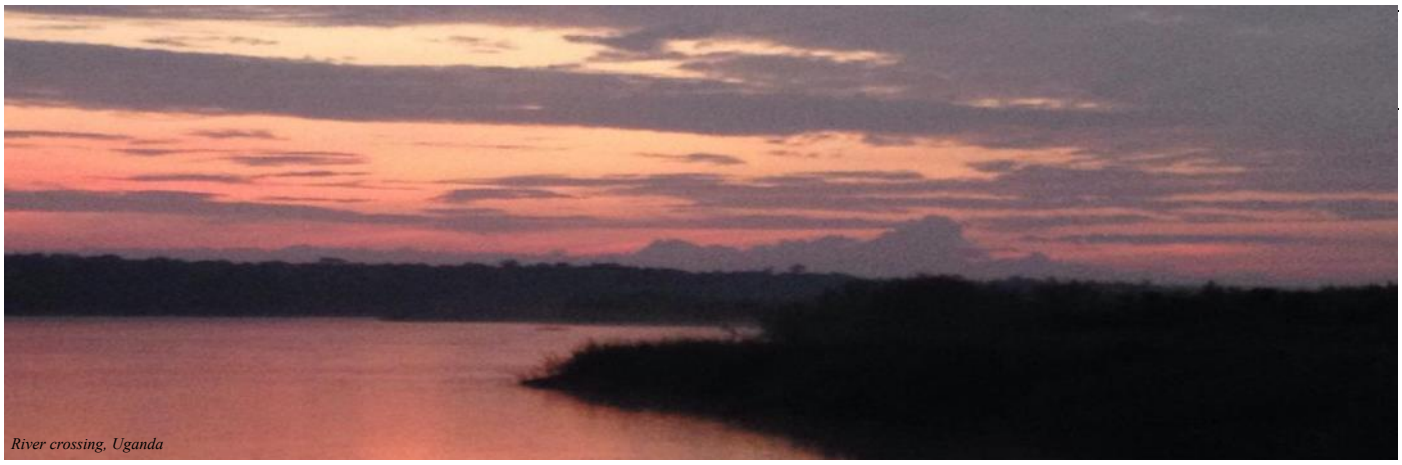

River crossing, Uganda

SECRETS OF WEST NILE

ethical challenges in the study of violence

Tom Lowman

I am several months into my first experience of real fieldwork and a pattern seems to be emerging from my interviews. It could be summarised as the following: the more integral an interlocutor is to my research, the harder it is to interact with them while still adhering to my university outlined ethics process.

My ongoing research is broadly concerned with the violence of the Amin regime in Uganda between 1971-79. One of my objectives is to find and interview men who had worked within sectors of the Amin state apparatus responsible for carrying out some of the worst excesses of the time. I arrived in Uganda with a folder full of consent forms and a shiny new recording device – a device rendered totally irrelevant by my phone – but that's a different story. In the south, speaking largely with victims of Amin era violence, I easily achieved consistent and good use of my consent forms. People were generally quite literate, with good spoken and written English, along with a willingness to engage with the formalised consent process I had devised before entering the field. Indeed, participants often articulated well thought out conditions of their own.

My fieldwork has since taken me to a district in the West Nile area. Many of the districts inhabitants once served in Amin's regime. Here I am conducting life-history interviews with a number of older men who have crucial memories of serving in the army under Amin.

In contrast to my research in southern Uganda, by the end of my third day of interviews in the West Nile region I was starting to wonder whether anybody was going to agree to signing a consent form. In many ways the reticence of men in West Nile to go 'on record' in any official capacity is understandable. As one former state research agent told me '*history is not finished up here'*. Fear of being retrospectively punished for past crimes remains very real. One gets the sense that even amongst their neighbours, former Amin agents don't really discuss their Amin era pasts. Many served away from home, and as such it is entirely possible that surrounding communities and even family members do not know the specifics of these agents' service histories. Further exacerbating anxieties is the fact that conversations during interviews often turn to the present day government and it's own far from glamorous record. While I can promise anonymity and that the information divulged during my research is to be used purely for academic work, people have to weigh the risks of trusting me, taking into consideration that I am a relatively unknown academic who has been in the field for just a few weeks.

Compared to my first few days, I have since found many former agents have become friendlier towards me, and are conveying vital information for my project. Though I can't help but appreciate the irony that the closer I get to the most sensitive sources of information for my PhD research, the harder it has been for interlocutors to feel comfortable adhering to my university's ethics codes, codes that were designed with them in mind.

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TO NOVICE FIELDWORKERS

do not shy away from emotions

Martina Angela Caretta

Because of the white heteronormative origins of the discipline, most geographers tend to shy away from sharing the difficulties they have encountered in the field and how they hampered or modified their research process. This attitude seems to pervade in my discipline despite feminist epistemological principles of reflexivity and positionality, which have become cornerstones of human geography in the last decades. Most recently, the field of emotional geographies has emerged arguing for the significance of examining emotions that the research process triggers as a source of data and reflexive accounts to improve the validity of our work.

Fieldwork, wherever it is carried out, is often an intense absorbing endeavor. Fieldwork preparation however, does not involve reflections on how our positions as outsiders in the communities we research can make us vulnerable. Fieldwork training tends to focus on practical and logistical matters, leaving out crucial instructions on how to act and react in case of harassment, emotionally impactful circumstances or disrespectful behavior by study participants. Ethical training in fact revolves around how our work must not harmfully impact our study's participants, but never dwells on what we should do if participants, respondents and informants cross the line with us. As PhD students we are novices and particularly vulnerable to these circumstances, not only because of our potential lack of training and prior experience, but also because we are dependent on the kind collaboration of informants to get PhD degrees. This is not to say that we are not part of a privileged elite that can afford to travel the world to achieve the highest educational degree, but it is to say that our exposure to risk and challenges should not be underestimated by ourselves, our supervisors nor the discipline of geography at large.

While carrying out fieldwork for my PhD in Tanzania my field assistant and I escaped an attempted physical assault from a drunkard, I had to endure the sight and sound of corporal punishment on children, and because of food poisoning and a virus my digestion took a turn for the worse with long term consequences for my health. Usually, these instances are not shared back at one's institution where questions by colleagues revolve around data collection and validity. Emotional geography challenges us to break this trend and openly acknowledge how our state of mind and body have affected our research and our relations with those around us during fieldwork.

Getting a PhD in geography does not have to mean that we have to endure whatever emotional and physical circumstances that may occur fieldwork and keep silent about them when we are back at our departments. Although departmental cultures still have a long way to go to grasp these elements, which we are often encouraged to leave out from our thesis methods section, an array of publications dealing with these difficulties is starting to emerge. Engaging with emotions will not only provide us with more relevant data for analysis, it will also enhance the validity and trustworthiness of our research and, most importantly, it will reify the "geographies of responsibilities" that we often only grant to our informants, and very rarely to ourselves.

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Recommended Readings

Caretta, M.A., Jokinen, J.C. 2016. Conflating privilege and vulnerability. A reflexive analysis of emotions and positionality in postgraduate fieldwork. *The Professional Geographer.* doi:10.1080/00330124.2016.1252268

This article, grounded on a self-reflexive, intersectional analysis of positionality, examines emotions in fieldwork through the autobiographical accounts we gathered during our postgraduate ethnographies in the Global South. We show how we, two female early-career geographers, emotionally coped with instances that put us in vulnerable positions due to loneliness, commitment to the field, insistent questioning, violence, and violent threats. We argue that a culture of silence in regard to fieldwork difficulties and their emotional consequences tends to permeate our discipline. We contend that departments ought to provide mentorship which takes into account doctoral candidates' different positionalities, conflating vulnerability and privilege, and their embodied intersectional axis. This renewed awareness will not only help reveal possible risks and challenges connected with fieldwork, but will also ultimately enrich the overall academic discussion.

Jokinen, J.C., Caretta, M.A., 2016. When bodies do not fit: an analysis of postgraduate fieldwork. *Gender, Place & Culture* 23, 1665–1676. doi:10.1080/0966369X.2016.1249343

This paper feeds into a bigger body of feminist geographical literature increasingly examining embodied aspects of research. These embodied dimensions of fieldwork often build upon intersecting positionalities, yet studies focusing on bodily limitations encountered by feminists in the field are relatively few. In this article we explore what it is like to be bodies that do not fit easily into the context within which they are supposed to be doing fieldwork. We are both female postgraduate students conducting fieldwork in the Global South. We have encountered, many times over, instances where, because of our sick and fatigued bodies, we have not been able to continue our work. We question the normalization of able-bodied postgraduate students by problematizing our own experiences, and argue that discourses of ability dominate fieldwork, in both its expectations and its conduct. This is especially the case for those with invisible disabilities because researchers may appear healthy but are not. As a result, postgraduate students may jeopardize their health for the sake of their research.

Car repairs in Kimana Town

Preparing for the Field

Anna Shoemaker

In this entry I discuss a lesson learned on accepting that things will not always go according to plan in the field. While I hope my reflections are of some use to others, I am aware that they will not resonate with all. Perhaps the only common denominator amongst doctoral fieldworkers is the fact that we all have a supervisor. Therefore, I also here suggest an exercise that PhD students might find helpful to do with their supervisors to prepare them for entering the field. This exercise is intended as a way to open up lines of communication between students and supervisors seeking to clarify what students are expecting to accomplish during fieldwork, and how supervisors are expecting to assist while they do it.

I am an archaeologist, and my thesis focuses on pastoral livelihoods and patterns of trade and connectivity in the Amboseli basin, Kenya c. 1500-1950 CE. My PhD incorporates more traditional archaeological methods of surveys and excavations, as well as semi-structured and informal interviews and participatory mapping. While I had a lot of flexibility in terms of required outputs from the interviews and mapping, I set more rigid expectations for collecting data on archaeological materials in Amboseli. It is common for East African archaeologists to spread their efforts thinly over a landscape, surveying large areas and conducting test excavations at numerous sites in order to try and understand local variability in material culture, food production, mobility, etc. During my fieldwork I was responsible for walking over and digging up a sufficiently representative amount of ground in order to make conclusions about the nature of Amboseli's archaeology. I had to ensure we achieved tangible goals in a timely fashion, adhering to methodological standards, without going over budget or demanding too much from the team. After a few months of fieldwork trying to stay on top of the finances, permits, car repairs, survey and excavation schedule, shopping for supplies, artifact inventories, field notes, data backups, physical health, team morale, communications back home etc. there were moments when I couldn't help but feel like a failure.

This sense of failure came, not because we were not managing to do the work, on time and on budget, and I think by and large with a smile, but from the fact that I was failing to control everything. Because there were also times when I had food poisoning, when our car broke down on Mombasa highway, or when the ATM in town was out of order for a month and I had to ask my friends to wire me money so I could buy food for the team. During these moments I felt responsible for failing to prevent those mishaps from happening in the first place. Admittedly there were situations during my fieldwork when I exercised poor judgement and errors were made. However, there were also instances when things happened that were beyond my ability to control.

It was problematic for me to expend energy during my fieldwork focusing on how I was failing to control all aspects of the surveys, excavations, team dynamics, budget, etc. However, a more insidious issue was that, as I resented the uncertainty of my research process, I came to internalise a degree of resistance towards the fieldwork itself. This resistance threatened to inhibit the research. For instance, I recall that when I had begun the initial survey portion of my fieldwork I was incredibly keen to get started. However some

months in, just before starting the excavations, I was experiencing more trepidation than enthusiasm as I faced departing from the fragile routine we had established during surveys, and exposing our research team to the uncertainties of excavations. Thankfully I was able to continuously acknowledge and dispel the subtle accumulation of resistance through discussions with research assistants, my supervisors, and other colleagues who assured me that they too had similar fieldwork mishaps, and that I was still 'on track'.

All in all, fieldwork went pretty well in the sense that I had supportive personal and professional networks to help me along the way as I became a better archaeologist, generated new data, and built relationships with people in Amboseli. As I shared my stories with other PhDs I came to regret that I had needlessly expended so much energy resenting my inexperience. Finally, I let go of those feelings of regret too. Swapping stories with my colleagues has allowed me to concede that *it is ok to feel overwhelmed when doing fieldwork*.

Though it probably seems obvious to the casual observer, it took time for me to gain perspective and realise that while it was my responsibility to navigate, as best I could, the challenges fieldwork posed, I could never have anticipated and prevented all of the challenges my fieldwork was going to present. I confide here my difficulties with resisting uncertainty and accepting the unexpected in the hope that others can learn from my experiences. Yet, I know that every doctoral student is going to have to navigate their own unique fieldwork process to which my anecdotes won't necessarily apply.

Many doctoral researchers go into dynamic field sites that are essentially unknown to their institutional advisory networks, and it is by doing fieldwork that the PhD student is expected to become the 'expert' on their study topic. Of course this is easier said than done, as fieldwork is likely to pose new challenges to students, challenges that students must learn to navigate largely or entirely independent of assistance from supervisors or academic peers. While many of us have the experience of conducting research in collaboration with local assistants, whose guidance, insights, and support are often invaluable, it is ultimately the responsibility of the PhD student, and not the research assistants, to ensure that fieldwork aims are achieved. I've come to perceive that the individualistic character of doctoral fieldwork offers unparalleled opportunity for personal and professional growth. In the process of navigating fieldwork, students build confidence and learn. That being said, it is almost the rule that some fieldwork experiences will be varying degrees of difficult, dangerous, or damaging, and these situations may feel magnified if students perceive they are entirely alone in managing them.

I don't think it's possible to devise a strategy for completely readying doctoral students for the diverse logistical, emotional, and ethical issues that have the potential to occur during their PhD. However, I do think that good communication between students and supervisors during fieldwork, and throughout the PhD process in general, is important for ensuring that students can get the help they need. Towards this end, I have provided here a template for a student and supervisor alignment exercise that some people may find useful to practice before leaving for doctoral fieldwork (or at other times during candidature). Though I have already completed my own fieldwork, and thus have not had a chance to test this exercise myself, the 'issues' in the alignment template have been formulated in collaboration with my supervisors, hopefully adding a level of experience and hindsight to the template design. I recommend the supervisor and the student complete the template separately first, and then both can meet to share their views. Some may find an issue listed below is uncomfortable to discuss, not of relevance, too vague or too specific - the issues for discussion are intended as suggestions, and should be individually tailored. The goals of the alignment exercise will vary, but ultimately it should generate understanding regarding what supervisors require from their students and what students may expect in terms of support, advise, and direction from their supervisors.

Finally, I also think it's good to remember that while the supervisor-student relationship is an important one, it takes a village to raise a PhD. Students should not neglect to cultivate a variety of relationships with peers, advanced career researchers, relevant institutional actors, as well as family and friends. Doctoral students doing fieldwork should feel like they can always contact someone for support, safe in the knowledge that if one connection doesn't work, another one will. As I think we all would like to operate in institutional environments that encourage a culture of shared responsibility, as PhD students we too can consider the role that we play in the fieldwork experiences of our colleagues. I've been fortunate to feel free to discuss my own fieldwork process with my supervisors, fellow doctoral students, and a diverse array of scholars, researchers, research assistants, friends, and family - and this has made all the difference.

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PhD & Supervisor Alignment template — Available for download as a Word Doc

1. Whose responsibility is it?

Indicate your view on who should have responsibility for the following issues by circling a number. Also, add any comments or clarifications. If you have additional issues, please, add them below.

Issue	Who is responsible?	Comments
Defining the students' research topic	1 2 3 4 5 Supervisor - Student	
Organising regular meetings/correspondence between supervisor and student	1 2 3 4 5 Supervisor - Student	
Seeing that the student has adequate financial resources to fund the fieldwork and other research activities	1 2 3 4 5 Supervisor - Student	
Arranging insurance coverage	1 2 3 4 5 Supervisor - Student	
Scheduling travel and organising transportation	1 2 3 4 5 Supervisor - Student	
Developing a schedule for completion of tasks to be undertaken during fieldwork	1 2 3 4 5 Supervisor - Student	
Arranging access/collaboration with organisations and facilities that provide resources for fieldworkers	1 2 3 4 5 Supervisor - Student	
Hiring of fieldwork assistants and other staff	1 2 3 4 5 Supervisor - Student	
Making sure formal requirements of reporting after fieldwork are met	1 2 3 4 5 Supervisor - Student	
Maintaining an effective working relationship between supervisor and student	1 2 3 4 5 Supervisor - Student	
Certifying that the student's work will be of an acceptable standard when examined	1 2 3 4 5 Supervisor - Student	
Organising international exposure (e.g. conferences, study visits) and expanding students professional networks	1 2 3 4 5 Supervisor - Student	
Seeing that the PhD programme is on track and on schedule	1 2 3 4 5 Supervisor - Student	

PhD & Supervisor Alignment template— Available for download as a Word Doc

2. Do you agree?

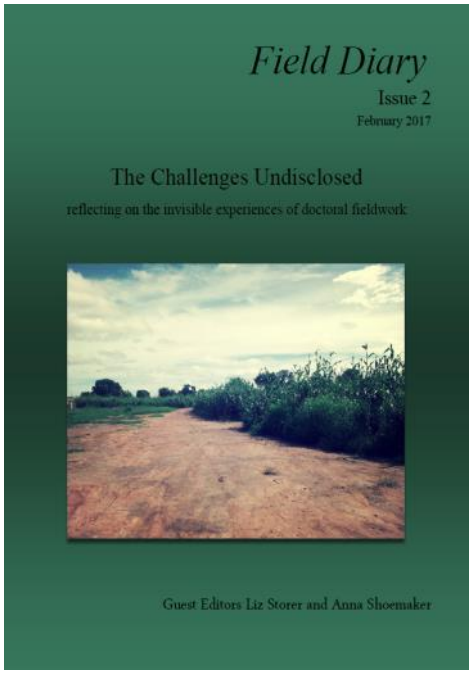
Circle the point on the line that represents your views on the following issues. If you have additional issues, please, add them below.

Issue	You would..	Comments
A strong personal relationship between supervisor and student is inadvisable during candidature	1 2 3 4 5 Agree - Disagree	
The supervisor should monitor the fieldwork process by seeing examples of fieldnotes, or data collected	1 2 3 4 5 Agree - Disagree	
The supervisor should visit the student in the field	1 2 3 4 5 Agree - Disagree	
The student is responsible for their own safety and wellbeing when in the field	1 2 3 4 5 Agree - Disagree	
The student is responsible for the safety and wellbeing of their research assistants when in the field	1 2 3 4 5 Agree - Disagree	
The student must always communicate to the supervisor if fieldwork is not going as planned	1 2 3 4 5 Agree - Disagree	
In an emergency situation the supervisor is to provide assistance	1 2 3 4 5 Agree - Disagree	
The student may have to use their own salary to pay for certain fieldwork expenses	1 2 3 4 5 Agree - Disagree	
It is necessary for the student to seek mentorship from academics other than the supervisor on some issues	1 2 3 4 5 Agree - Disagree	
The student and the supervisor must respond promptly (e.g. max. 48 hours) to all emails/communications during fieldwork	1 2 3 4 5 Agree - Disagree	

Source. There are numerous iterations of this alignment template, this particular version is based on a template used at the Stockholm Resilience Centre that has been further adjusted by Anna Shoemaker and Anneli Ekblom for this issue of Field Diary.

Issue 1

Issue 2



This special issue of Field Diary, guest edited by Liz Storer and Anna Shoemaker, is the second online publication in the Field Diary pdf series produced by the Resilience in East African Landscapes project.

Field Diary brings together fieldwork-themed stories and reflections from many parts of the world, with a particular focus on East Africa. If you would like to submit an entry, propose a theme, or serve as an editor for the next issue of Field Diary, please write to fielddiaryeditors@gmail.com.

The guest editors of this special issue of Field Diary would like to acknowledge Annemiek Pas Schrijver, Geert W van der Plas, and Colin Courtney Mustaphi as the series editors and creators of Field Diary.

Field Diary

Issue 2

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