Plans, Changes, Improvisations

Navigating Research on the Fertility Quests of Mozambican Women and Men

Inês Faria

ABSTRACT: This article addresses the challenges and reflections of a junior anthropologist while developing research on the delicate topic of reproductive health and infertility in Maputo, Mozambique. Based on participant observation notes, entries in fieldwork diaries, and interviews, and assuming the character of a reflexive ethnographic account, the article concerns personal and research challenges and opportunities experienced during the preparation and development of a research project and a PhD thesis. While reflecting more broadly on processes of knowledge production, history and colonial relations, and on the writing of a scientific account, it provides insights into the pragmatics of research in medical anthropology by detailing the everyday life of doing ethnography, including networking, bureaucratic processes, boredom, the exploration of new fieldwork landscapes, and positionality dilemmas.

KEYWORDS: fieldwork, hospital ethnography, Maputo, medical anthropology, positionality, reflexivity, therapeutic navigations

In this article, I look at the pragmatics of the fieldwork process with a focus on my own personal experience. Rather than an analytical piece, as I have developed elsewhere (Faria 2018), I opt here for a more descriptive and informal tone. I argue for the relevance of flexibility and reflexivity in fieldwork and analytical practice, but also – following authors such as Hansjörg Dilger and colleagues (2015) for the specific case of medical anthropology – for the relevance of the notion that ethnographers are in a privileged position to explore, and go beyond, the crystallised division between context, the private fieldwork experience of the researcher, and the published text of the scientist (Latour 1993). Indeed, the processes of fieldwork, analysis and writing often seem to be light years apart from one another; departing from the messiness of empirical work and its relational complexities and arriving at the ‘objective’ published scientific account.

Although these matters have been debated (Dilger et al. 2015; Latour 1993; Lewis 2012; Long et al. 2008;

McLean and Leibing 2007; Visweswaran 1994), I still feel that there is a lack of more pragmatic accounts directed at young anthropologists doing research for the first time, accounts that focus on the realm of the mundane but that nevertheless crucially support the making of science – in this case, medical anthropology. By sharing the mundane aspects of my processes of fieldwork (Eijk 2018; Latour 1992), analysis and writing, I argue for a demystification of the anthropologist and ethnography. I argue as well for the need to make these debates and reflections accessible to young scholars and adapted to specific areas, since the pragmatics of research in medical anthropology (Dilger et al. 2015; Long et al. 2008; Van der Geest and Finkler 2004) will be different from those of other fields, but may also reproduce wider socio-political operational patterns (Jobson 2020; Readings 1996; Straube 2020; Trouillot 2003).

The process of ethnographic research demands a reasonably flexible research project and method-

ological structuring (Davies 1999). Most young anthropology scholars know this, and start discussing methodological concerns early on in their training and, I dare to say, long before even touching the field terrain with their own two feet. Such was my case. I had heard various field anecdotes from several teachers, some more amusing and interesting than others, but all with the same motto, which one of my professors in Lisbon simply defined as a crucial necessity for *jogo de cintura* in ethnographic fieldwork. *Jogo de cintura* is a dance metaphor, meaning that you need to 'have the moves' to graciously manoeuvre your entrance and navigation through fieldwork terrains. This idea was later complemented by another teacher's impression of ethnographic fieldwork, this time at the University of Amsterdam, who described participant observation as 'deep hanging out', an expression first used by Clifford Geertz (1998) more than two decades ago. This was another helpful illustration, and both became useful reminders of the tensions and delicate relations that I indeed experienced in the field. But what does it really mean in practice?

Below I will share aspects of my navigations throughout my first fieldwork for my PhD in medical anthropology, which included hospital ethnography, in order to shed light on experiences that may be useful for young scholars. In the next section, I will briefly outline the background to my research. In the third and fourth sections, I will reflexively explore the practical matters and ethical issues that were present in my fieldwork experience. Finally, I will dedicate the last section to the process of analysis and writing, offering insights into the pathway between fieldwork and a thesis or article. In the concluding section, I will sum up and discuss the aspects of my experience that I find most relevant for young scholars in medical anthropology, some dilemmas and what I see as promising 'ways of doing' anthropology.

Background: The Research and the Thesis

Many anthropologists, including medical anthropologists, have written about the process of planning research and about the ambiguities and challenges of doing fieldwork (Barley 2012; Dilger et al. 2015; Lewis 2012; McLean and Leibling 2007). I too had thoroughly read and written about it before boarding the plane that would take me to my first fieldwork site. For me personally, the accounts I read were very useful, particularly the more humorous ones and those that implied a deeper conversation about improvisation (Barley 2012). Other debates, such as the

ones about historical, colonial and political tensions latent within anthropological practice, were also good sources of thinking material (Dilger et al. 2015; Lock and Nguyen 2010; Trouillot 2003).

My selected fieldwork site was the city of Maputo, the capital of Mozambique, where I arrived in the summer of 2013. The research project I was working for was about infertility and assisted reproductive technologies (ARTs) in sub-Saharan Africa, with the title 'Dynamics and Differences of Assisted Reproduction in Sub-Saharan Africa'. It was a comparative study between Uganda, Mali, Ghana and Mozambique; I was collecting data for the latter. This umbrella project determined my individual study's subject and direction, but gave me flexibility in terms of possible changes stemming from the empirical research in a place where social scientific work about infertility and the use of ARTs was scarce (Mariano 2014). In the city of Maputo itself, there was a single private clinic, where some assisted reproduction techniques were performed. However, when it came to the use of more advanced medical reproductive technologies, most of the women I met resorted to reproductive travel to fertility clinics in South Africa close to the border.

These South African clinics assembled state-of-the-art global biomedical technologies, but due to the costs of the services and of travel they were not accessible to everyone (Gerrits 2012; Inhorn and Patrizio 2015). Most of my respondents in Maputo were unable to finance a journey to one of these clinics, and this inequality, alongside the familial, gender, social and personal issues that women experienced stemming from a situation of involuntary childlessness in a relationship, were central subjects of my thesis. I approached the uncertain and winding therapeutic pathways that I encountered using the concept of social navigation (Vigh 2009) and the notion of fields of possibility (Schütz 1972), as people tried to implement reproductive projects that faced disruption and transformation at different levels (Velho 2003).

My research lasted one year and consisted of multi-sited ethnographic fieldwork (Marcus 1995) based in Maputo, as well as travel to South African fertility clinics. The empirical phase was divided into two fieldwork periods: the first period of six months, from January until mid-July 2013, and the second period of three months, from September until December 2013. Parallel to semi-structured interviews with practitioners, with women who had used ARTs and with women attending one private clinic and one public hospital in Maputo, the empirical research also included ethnographic observations at those clinical sites.

Although I received theoretical preparation for the fieldwork, and my supervisors provided me with adequate support, I had scarce assistance and help from my institution in terms of this preparation. What was there came in the form of informal rather than official contacts, and most institutional connections were left for me to do. Luckily, the informal connections I made became the ones I needed. I embarked on a plane to Maputo with some contacts, documents and advice, and there I went, on my first ethnographic endeavour, about to discover what ‘deep hanging out’ and *jogo de cintura* meant.

Preparing Fieldwork: Innocence and Enthusiasm

My story began in August 2012, when I was hired as a junior fellow in the above-mentioned research project, which was based at the University of Lisbon, and became excited with the prospect of going on my first long-term fieldwork mission. As I will show below, this excitement and belief in the work I was about to pursue were important for the development of my research, as were all the comments I received on it and the maintenance of a certain openness when considering what could actually be possible empirically.

Before embarking on the fieldwork mission, I did an extensive bibliography and document review, conducted an inventory of fertility clinics in Mozambique and South Africa, and outlined the research and analysis process. I also began making potentially useful contacts. The exploratory contacts I made included multiple e-mails and phone calls to clinics, both in Mozambique and South Africa, but also included multiple contacts with the Eduardo Mondlane University, with the local National Bioethics Committee, and with people who developed research in Mozambique. I also made informal connections with friends of friends and family living in Mozambique.

It was at this stage that I started receiving advice and ideas from different people: some solicited, some not. I also received further advice from other anthropologists and from my own PhD supervisors on how to manage fieldwork, on the delicacy of my research subject and the relationships and tensions embedded in it. I often identified with some of the episodes and stories they shared with me during fieldwork.

In these dialogues, I learnt about the importance of discerning constructive criticism from positionings I wanted to distance my work from. Although this became easier with time, at the beginning the various inputs were a bit difficult to process. Believing

in your project is an important part of fieldwork preparation; you should consider advice but always while taking into consideration what you wish to do as a researcher. Awareness of these dynamics is important, but so is humility in considering others’ opinions while thinking about your proposal.

During the initial process of networking, I also had to submit a protocol for ethical clearance to the National Bioethics Committee of the Ministry of Health of Mozambique (CNBS/MISAU). It has been mentioned elsewhere how obtaining research clearance and authorisation from local research gatekeepers can be a herculean endeavour, posing many challenges to the researcher, especially a junior one and particularly when the study involves research in clinical settings (Davies 1999; Van der Geest and Finkler 2004). My efforts to obtain such clearance were no exception. However, my experience contrasts with other accounts of the difficulties of getting ethical approval to do hospital ethnography. Sjaak van der Geest and Kaja Finkler (2004) explain how obtaining approval in Europe was for them much harder than in sub-Saharan Africa and Asia. My experience was the opposite: I had to make more efforts to reformulate my ethical protocol following the recommendations of the National Bioethics Committee in Mozambique in order to gain access to the hospital there than I had during my MA research in Portugal, which had also included clinic ethnography.

Following the advice of different connections, I wrote the entire bioethical protocol before leaving for Mozambique: 48 pages of detailed research objectives, data collection processes, informed consent forms, questionnaires, justifications and other thoroughly described research plans. By the time the protocol was ready, I was already in contact with Eduardo Mondlane University in Maputo and had found a provisional local supervisor with whom I would collaborate during my research and who would support my empirical fieldwork.

At this point, finally, everything was ready. Shortly after obtaining my visa, with farewells said at home, I went to the airport and set off to Maputo for the first time. After all that work, I was extremely confident about my protocol and, having taken care of the Mozambican visa in a swift manner, I was ready to start my ethnographic endeavour. Luckily, at that time, I also had a friend living in the city, who kindly offered to host me for the first few days and who provided me with some basic orientation tips. Upon my arrival in Maputo, however, I was soon about to discover for myself the trials and tribulations of navigating the field.

Navigating the fieldwork does not concern only the fieldwork. It starts at home with the preparations and involves having a strong belief in your research subject. Much like the way in which ethnography demands a choreography between immersion and distancing in the field, preparing ethnographic fieldwork demands immersion in the creation of a project while accepting that a lot of things will not go as planned.

In this sense, I truly believe that a well-thought-out methodical plan for your empirical research is fruitful, even if it all pans out very differently. Contacts are also invaluable, and will surely help, directly or indirectly, with negotiating your place(s) in the field. More often than not, connections are the best path-openers both in pragmatic but also in thought-provoking ways.

Navigating Maputo: Plans and Change

Very much like my respondents' therapeutic quests, I found that my fieldwork process was also one of social navigation and situational social networks (Faria 2018; Vigh 2009). It is about trying to make the best of the opportunities that appear and manoeuvring through obstacles. How this process unfolds will be different for each researcher and each site. Below, I will present an account of my own 'deep hanging out' and *jogo de cintura*.

I arrived in Maputo on 17 January 2013. My first steps consisted of holding meetings at the university to introduce myself and the research I intended to do; asking the department staff to help me with my bureaucratic needs; meeting with researchers studying similar subjects; and participating in my project's internal workshop. It all went smoothly.

At this point, I was still confident that my ethical protocol would be approved quickly. It was not. Two months into my fieldwork, I was still going back and forth writing request letters and submitting them to different public health secretariats in an attempt to collect all of the necessary documentation to submit my ethical clearance proposal to the CNBS/MISAU. These included several declarations and terms of responsibility regarding the research; an authorisation for the research from the local health authority – in this case, the Department of Health of the City of Maputo – which took a while to obtain; and finally, a letter of institutional coverage from the Anthropology Department of Eduardo Mondlane University.

Eventually, I had collected all of the documents, improved my bioethical protocol based on the pre-

cious advice of my local supervisor, and submitted everything to the CNBS/MISAU. One month later, I got the results – it did not pass. After a day of self-commiseration and frustration, I revised the protocol, rewriting parts of it and introducing further details according to the National Bioethics Committee's review. Then, I tried to submit it again. However, the Committee met only once a month and there was a strike by the health professionals in Maputo that lasted roughly one month (from 20 May to 17 June), which meant that I had to wait before being able to submit the protocol for the second review. Unfortunately, I only obtained the official approval in late July, after I had already returned to Lisbon following the end of my first fieldwork phase.

While waiting to get ethical clearance to work at the hospital, I set my mind on collecting data outside of the clinic. To reach interlocutors, I used the method of snowball sampling (Davies 1999), beginning with some of the contacts I had previously made. I managed to interview and spend time with women and couples who had done ART cycles in South African clinics. The first two informants that I met provided me with the contacts of other women who might be willing to talk to me. They also provided me with the contacts of women who were clearly not willing to talk to me, and even less so after my many attempts talk to them and meet them somewhere. I never got a round 'no' within these attempts, but finally understood that being elusive was a polite manner of saying no. I also understood, in practice, the importance of adapting the research pace to the informants' rhythms and get to the 'point' through long conversations and trust-building, always bearing in mind the respondent's stakes in the research subject.

So there I was, navigating my way towards the bioethical protocol, towards interviewing ART users and knocking on clinics' doors that were often not 'open' for my research. I had done a survey of local clinics, 21 private clinics and 2 public hospitals in Maputo and 12 in South African cities close to the Mozambican border (Nelspruit, Pretoria and Johannesburg). When in the field, I found that I should attempt for a more analogic tactic: walking around the city checking addresses and asking for specific clinics and presenting my project to enquire about their availability to participate. Unfortunately, this strategy did not take me far.

Overall, there are numerous negotiations that take place before and during fieldwork, from formal ones like ethical protocols and clearance, to informal ones. It has been discussed how entering the field in a formal manner and contacting research interlocutors is

different from actually accessing them (Reeves 2010) and how fieldwork is a reflexive process filled with micro-informal negotiations.

There is an apparent disorder during fieldwork which I find somehow characteristic of ethnography, where there has to be time to prepare the research plus time to 'get to know' and be familiar with the informants, the studied terrains and their changes (Davies 1999). During the first fieldwork period of preparation and adaptation, I was able to meet people informally and I often enjoyed the time I spent in the city. These networks would be very important for me while I navigated the rather chaotic start to my fieldwork.

Through these informal connections that I made in Maputo, and through contacts I made with my own gynaecologist in Portugal, who happened to know gynaecologists working in Maputo, I was able to effectively reach one doctor. After so many attempts, it was hard to believe that I had finally succeeded. And that was it: the window for an exploratory interview with a local gynaecologist was opened. I seized the opportunity and met him at the maternity operating room of one of the public hospitals that he worked at. In this meeting, he made some comments about my research project and told me where I could carry it out as soon as I had obtained the ethical clearance.

It so happened that after the ethical clearance had been issued I still needed administrative authorisation from the CNBS/MISAU. I eventually obtained it after repeated daily visits to the Ministry asking for the pending document. In this way, I could go to the clinical sites and start my clinical fieldwork – this commenced during my second stay in Maputo in September 2013.

After this long process, I could see how some doors could be difficult to open. In a place where many academics and scientists try to do research, sometimes without returning the product of this research to the participants and institutions involved, people are likely to be suspicious about researchers until seeing proof to the contrary and receiving assurance that you have no hidden agendas (Dilger et al. 2015; Van der Geest and Finkler 2004).

Indeed, my first experience of navigating the field revealed how projected research pathways are constantly being transformed, and how the fieldwork starts to flow once the local gatekeepers are involved in and acknowledge the reliability of the study. Openness to collaboration with research interlocutors and building a trust relationship are crucial for this.

Other bureaucratic procedures such as obtaining ethical clearance are complex, yet necessary. I agree

with Dilger (2017), who assertively suggests that the heavy bureaucratic processes of procedural ethics are important, while also suggesting that making the bureaucracy more agile and better adapted to each site and/or area of knowledge could bring improvements, not only in terms of granting clearance but also in terms of research supervision (see Schwalbach 2014 for the specific case of Mozambique and how the ethical clearance system was updated to be swift).

Throughout this process, I cannot underestimate the role of informal social networks in navigating the field and establishing trust up to the point that I got into the hospital. Furthermore, the informal conversations about the topic of family, infertility and healthcare, but also power, colonialism, politics and history were invaluable in the navigation of the institutional biomedical research field in Maputo as well as of my own research.

My Place(s) in the Field: Positionality in Clinical Sites and amongst Research Participants

In this section, I will explore the role of change and how I negotiated and defined my position in the field. During fieldwork, my initial focus on ARTs and reproductive travel to South Africa transformed into a focus on broader issues regarding people from different backgrounds and their divergent social and therapeutic navigations for infertility care. In fact, it was only when arriving in the field that I could truly grasp the two main limitations of my initial study project: first, the idea that I would make a swift start to the ethnographic research, which would include hospital ethnography; and second, the necessity of encompassing a broader spectrum of social aspects surrounding infertility, its treatment and the unequal access to ARTs in order to make the research more fruitful.

During fieldwork, I interacted with different kinds of informants in various settings and through diverse kinds of connections. Although I frequently felt like the annoying anthropologist chasing elusive informants, when I did meet with success the troubles of my persistence seemed worthwhile. In the end, I was able to speak to several women, who kindly shared their stories, and sometimes their family spaces, with me, giving me personal accounts of and experiences with the sensitive subject of involuntary childlessness and all the social and emotional baggage that comes with it. Infertility and reproduction are sensitive subjects to explore, and therefore some of the women

that I tried to contact were often less than willing to speak openly about the subject right away. However, it was common that after starting the interview with general questions and conversation, they gradually did open up to me and we spoke about many things related to their infertility situation, including family, marriage, sexuality, religion, reproductive events and therapeutic processes. I believe that the fact that I was an outsider made this process easier, since I would not compromise the maintenance of secrecy about treatments. This is also the case when dealing with other sensitive subjects in medical anthropology in cities where there is greater anonymity (see Hörbst 2012). In this case, not being connected to the informant's more intimate networks and surroundings was actually advantageous both for me and for the informants.

However, doing fieldwork in an urban environment also posed some challenges. If on the one hand it may have been a productive context where several of my respondents could talk to me openly shortly after we met, on the other hand the fluidity of city life made scheduling interviews rather difficult. I was able to meet only four informants more than once: one doctor and three women reached through snowball sampling. Furthermore, having periods of prolonged interaction with the women or couples was seldom easy due to privacy, secrecy and availability issues. Encounters outside of the clinic had to be scheduled, and people had to be available and willing to attend when the day arrived – which of course did not always happen. During the clinical fieldwork, although the respondents were easier to contact, interviews took place inside the clinical settings, which affected their sense of comfort.

It was hard to negotiate and adapt my position in the field inside and outside of the clinical settings. I opted to always emphasise my position as a researcher in the social sciences and not medicine, in which I have no background (Wind 2008). Indeed, as appears in some debates about positionality in medical anthropology (Dilger et al. 2015), I had to negotiate different positions in the field while prioritising the participants' well-being, particularly when talking to informants with different moral, social and cosmological world views. This kind of reflexivity, where you are aware and mindful of what you are doing, is not always easy to grasp, but it is an essential aspect of fieldwork in medical anthropology involving people who are, in most situations, suffering. It is also essential to consider your interlocutors' stakes (Kleinman 2006) and let these lead the research to some extent, instead of imposing your views and

preconceptions upon them, particularly considering you are in a position that, whether you like it or not, is sometimes perceived as a position of power.

An example of this is how, although I always explained my purpose and where I came from, detaching myself from any connection with the clinical sites or any capacity to improve or affect their treatments, the women I encountered in the clinical sites, as well as the other women sampled outside of the clinic, frequently sought my therapeutic advice. These situations challenged me as a researcher and frequently posed ethical dilemmas regarding the limits of my role within the ethnographic fieldwork versus the expectations that some patients had when we met. These included patients thinking that I would be able to do something to improve their treatment but also 'using' me to get ahead in lines to deal with paperwork. Although it is not possible to foresee when this will happen, when I found myself in these situations I tried my best to explain that my position was not one of a doctor, or medical staff. I also considered this reflexively for the remainder of the research.

I had to learn how to navigate these tacit negotiations, and I came to understand the limitations of research about people and social issues that is made through social interaction. While outside the clinic, where I interviewed mostly women who had already undergone ART, these limitations were less bounded; inside the clinic and hospital, I needed to engage in constant self-surveillance and careful reasoning about my position and the content of my interviews, as my informants were going through stressful therapeutic processes.

During the infertility consultation days, aside from interviewing patients and doctors at the clinical sites, I also conducted participant observation, sometimes during consultations and at other times in the waiting rooms and hallways. This provided me with a broader insight into the clinical sites, the dynamics of their organisation, and the relationships between staff and patients (Gerrits 2012; Kleinman 1978; Van der Geest and Finkler 2004).

In the end, I believe that the most significant limitation of my research is that I did it independently, without a research assistant or interpreter being involved in the study. This nevertheless allowed for an increased sense of ease and intimacy during some interviews. In the end, ethnography functions as a conversation between researcher and observation terrains, but mostly between people, where trust has to be gained and established (Davies 1999), where you need to negotiate 'your place in the field' and manage it in an ethically sound manner (Dilger et al.

2015; Guillemin and Gillam 2004), and where reciprocity with, and inclusion of, research participants and institutions is a need (Dilger 2017).

After Fieldwork: What Do I Do with All This?

This section is dedicated to analysis and writing, a journey as radical as the one of a fieldwork mission, but one that takes you from an apparently chaotic assemblage of data to a final, polished, piece of text.

I returned from fieldwork to Lisbon in December 2013. I must admit that I did not immediately look at the data I had collected, and, once I did, it all seemed a confusing mess. But just as I had learnt to navigate Maputo, I also hoped that I would learn to navigate my collected data, which I once frustratedly characterised as ‘having no pattern’. Little did I realise then that I was carrying the answer to my complaint in these very words: no fixed pattern. It was by looking between the lines of what at first glance seemed to be an irregular assemblage of disrupted reproduction tales that I recognised for the first time the inter-related dynamics of agency, continuity, motion and change as the main shared pattern. This ‘epiphany’ only came to me after what I can describe as a process of disentangling wires: pulling at different ones, loosening knots and changing tactics until I finally ‘saw it’ when I least expected to. In addition, I think this idea only consolidated in my mind some months after returning from the field. After that moment, everything entered into a sort of flow that disembodyed in my thesis.

After struggling a bit with my data and what to do with it, I defined my intention to provide a grounded account on the subject of national and international infertility healing trends amongst Mozambican couples. In this way, my analysis and writing had a continuous concern with the interrelationships between four factors: context, conditions, meaning and agency. In other words, it focussed on how women or couples, according to their context (Schütz 1972; Velho 2003), navigated different channels in their quests for conception (Inhorn and Patrizio 2015; Vigh 2009).

Written down as such, this could seem rather linear. But the process of ‘getting there’ and finding the thesis you want to defend is challenging. During many moments of analysis and writing, I often found myself staring at the wall and finding it much more interesting than what was on my screen. Just as you have to manoeuvre through fieldwork, you also have

to do so through the analysis and writing, and it all comes down to those pages that are the result of your immersion in a particular reality as a whole. Fieldwork and theoretical work are very different from one another, but they also complement each other.

Plans, Changes and Improvisation: Some Notes

Looking back, I am glad that I valued the whole fieldwork process at the time. I believed that long fieldwork missions were common, but since I completed my PhD, fieldwork periods have become shorter. Writing articles and grants have become my closer everyday life companions. As you progress in academic life, empirical ‘action’ becomes elusive, and seems to work more as a motivation for playing the uncertain research financing game.

The processes of *jogo de cintura* and ‘deep hanging out’ always seem easier said than done, yet doing them is a crucial part of a scholar’s trajectory. The most important thing I came to realise is the significance of relating to your terrains in an ethically sound manner, that is, with humility, flexibility and empathy towards your informants (Davies 1999; Dilger et al. 2015). Particularly, it is important to do this without bottling up controversial aspects of history and of anthropology as a discipline (Trouillot 2003) and striving to position yourself as a researcher in the most empathic, collaborative and reciprocal manner possible (Kleinman 2006; Straube 2020). The latter is not always easy, and is currently part of wider debates within anthropology (Jobson 2020).

I believe that a lot of fieldwork is about the (sometimes boring) mundane parts of everyday life and what they tell you (Eijk 2018; Latour 1992), but also, as ‘deep hanging out’ suggests, about human relationships, trust and respect. However, as in life, there are research participants with whom you communicate well, but others with whom you do not. This is part of ethnography and the dialogues are a great exercise.

Bureaucracy, finding research sites and case studies, and meeting informants willing to share their socio-culturally entrenched, intimate and deeply emotional experiences posed challenges and demanded a fair amount of improvisation. Positioning yourself in the field and in relation to your informants, in situations where there is a thin boundary between research relations and trusting human relations, is a crucial negotiation (McLean and Leibling 2007; Straube 2020; Wind 2008). Improvisation is a great capacity to have

and a useful method for reimagining projects when unpredictable events occur – as long as it is weighted and taken as seriously as having a solid ethical stance towards informants and the fieldwork sites (Guillemin and Gillam 2004). Change is a part of research and analysis, but openness to it, with a clear idea of where you want to take your data, is also important. From the project phase to the publication of the results, there is a winding but also exciting and captivating pathway of enquiry, learning and reflection. The process of writing is as much a navigation as the process of ethnography; it is certainly more solitary and analytical, but it is nevertheless a focal part of academic work. In all these processes, during writing as in fieldwork the researcher is a constant presence, and one that should be reflexively considered (Adams et al. 2015; Dilger et al. 2015; Long et al. 2008).

It is the mundane, human part of the field – the changes and improvisations, the *jogo de cintura* and the ‘deep hanging out’, which so seldomly emerge in plain English in academic outputs or outside university corridors – that I have tried to write about. I believe that a lot goes on between living in the field and producing scientific output. I also believe that how the ethnographer lives this experience is crucial for the process of analysis and writing. One cannot erase this experience, nor can they erase the context where knowledge is produced (Latour 1993), especially when working with qualitative methods, with people, and in projects concerning health matters that deeply affect intimate social aspects of people’s lives. Although there is no universal recipe and anthropology is (gladly) heterogeneous as a discipline, I believe thinking through your research with and for your interlocutors, as much as with and for yourself as a scholar, within the framework of high ethical standards, is warranted. The adequate way to do this will probably appear while in the field, where, as we have seen, change and negotiations often lead you to more trustful and collaborative relations that, all things considered, are the foundations of ethnography.

INÊS FARIA is a Researcher at the Research Centre in Economic and organizational Sociology, Research Unit: Social Sciences and Management, Lisbon School of Economics and Management, University of Lisbon (CSG-SOCIUS/ISEG, University of Lisbon). Her research is in the areas of medical anthropology and economic anthropology, broadly focussing on the relations between technology and society regarding the areas of money, economy and finance, but also of healthcare in European and sub-Saharan African contexts. E-mail: inesdffaria@gmail.com

Acknowledgements

This work was partly supported by the Portuguese Foundation for Science and Technology (FCT, I.P.) – the Portuguese national funding agency for science, research, and technology, under the Project UIDB/04521/2020. I thank the *Anthropology in Action* editors, and the anonymous reviewers of previous versions of the manuscript for their invaluable comments and suggestions. I am also very thankful to the organisers of the Medical Anthropology Young Scholars’ special issue ‘Being There: Early Career Medical Anthropologists’ Perspectives on Contemporary Challenges in the Field’, Francesca Cancelliere and Ursula Probst, both for this warranted initiative and for all of their support with the manuscript.

References

- Adams, T., S. Jones and C. Ellis (2015), ‘Autoethnography: Understanding Qualitative Research’, (Oxford: Oxford University Press).
- Barley, N. (2012), *The Innocent Anthropologist: Notes from a Mud Hut* (Long Grove, IL: Waveland Press).
- Davies, C. (1999), *Reflexive Ethnography: A Guide to Research the Selves and Others* (London: Routledge).
- Dilger, H. (2017), ‘Ethics, Epistemology and Ethnography: The Need for an Anthropological Debate on Ethical Review Processes in Germany’, *Sociologus* 67, no. 2: 191–208, <https://www.jstor.org/stable/45174690>.
- Dilger, H., S. Huschke and D. Mattes (2015), ‘Ethics, Epistemology, and Engagement: Encountering Values in Medical Anthropology’, *Medical Anthropology: Cross-Cultural Studies in Health and Illness* 34, no. 1: 1–10, doi:10.1080/01459740.2014.960565.
- Eijk, M. (2018), ‘The Anthropology of “Boring” Things’, *Medical Anthropology Quarterly*, <http://medanthroquarterly.org/2018/07/16/the-anthropology-of-boring-things/> (accessed 31 January 2021).
- Faria, I. (2018), ‘Therapeutic Navigations and Social Networking: Mozambican Women’s Quests for Fertility’, *Medical Anthropology: Cross-Cultural Studies in Health and Illness* 37, no. 4: 343–357, doi:10.1080/01459740.2017.1417278.
- Geertz, C. (1998), ‘Deep Hanging Out’, *New York Review of Books*, 22 October, <https://www.nybooks.com/articles/1998/10/22/deep-hanging-out/>.
- Gerrits, T. (2012), ‘Biomedical Infertility Care in Low Resource Countries: Barriers and Access’, *FVV in ObGyn Monograph* 2012: 1–6, <https://fvvo.eu/assets/262/01-Gerrits.pdf>.

- Guillemin, M., and L. Gillam (2004), 'Ethics, Reflexivity, and "Ethically Important Moments" in Research', *Qualitative Inquiry* 10, no. 2: 261–280, doi:10.1177/1077800403262360.
- Hörbst, V. (2012), "'You Need Someone in a Grand Boubou': Barriers and Means to Access ARTs in West Africa", *FVV in ObGyn Monograph* 2012: 46–52, <https://www.fvvo.be/monographs/biomedical-infertility-care-in-poor-resource-countries-barriers-access-and-ethics/>.
- Inhorn, M., and P. Patrizio (2015), 'Infertility Around the Globe: New Thinking on Gender, Reproductive Technologies and Global Movements in the 21st Century', *Human Reproduction Update* 21, no. 4: 411–426, doi:10.1093/humupd/dmv016.
- Jobson, R. C. (2020), 'The Case for Letting Anthropology Burn: Sociocultural Anthropology in 2019', *American Anthropologist*, doi:10.1111/aman.13398 (accessed 31 January 2021).
- Kleinman, A. (1978), 'Concepts and a Model for the Comparison of Medical Systems as Cultural Systems', *Social Science and Medicine* 12: 85–93, doi:10.1016/0160-7987(78)90014-5.
- Kleinman, A. (2006), *What Really Matters: Living a Moral Life amidst Uncertainty and Danger* (Oxford: Oxford University Press).
- Latour, B. (1992), 'Where Are the Missing Masses? The Sociology of a Few Mundane Artifacts', in *Shaping Technology/Building Society: Studies in Sociotechnical Change*, (ed.) W. E. Bijker and J. Law (Cambridge, MA: MIT Press), 225–258.
- Latour, B. (1993), *We Have Never Been Modern* (Cambridge MA: Harvard University Press).
- Lewis, G. (2012), 'Before and after Fieldwork: Ingredients for an Ethnography of Illness', *Anthropology and Medicine* 19, no. 1: 27–36, doi:10.1080/13648470.2012.660468.
- Lock, M., and V. Nguyen (2019), *An Anthropology of Biomedicine* (Hoboken, NJ: Wiley-Blackwell).
- Long, D., C. Hunter and S. Geest (2008), 'When the Field Is a Ward or a Clinic: Hospital Ethnography', *Anthropology and Medicine* 15, no. 2: 71–78, doi:10.1080/13648470802121844.
- Marcus, G. (1995), 'Ethnography in/of the World System: The Emergence of Multi-Sited Ethnography', in *Annual Review of Anthropology* 24: 95–117, doi:10.1146/annurev.an.24.100195.000523.
- Mariano, E. (2014), 'Understanding Experiences of Reproductive Inability in Various Medical Systems in Southern Mozambique' (PhD diss., Katholieke Universiteit Leuven).
- McLean, A., and A. Leibing (2007), *The Shadow Side of Fieldwork: Exploring the Blurred Borders between Ethnography and Life* (London: Blackwell).
- Readings, B. (1996), *The University in Ruins* (Cambridge, MA: Harvard University Press).
- Reeves, A. (2010), 'A Difficult Negotiation: Fieldwork Relations with Gatekeepers', *Qualitative Research* 10, no. 3: 315–331, doi:10.1177/1468794109360150.
- Schwalbach, J. (2014), 'Comité Nacional de Bioética para a Saúde — experiência Moçambicana', *CNEV: Bioética nos Países de Língua Oficial Portuguesa, Justiça e Solidariedade*, 43–50. (Coleção Biotética 18: Conselho Nacional para as Ciências da Vida), https://www.cneqv.pt/pt/publicacoes/download_documento=5590&token=45c47e5d0425a484f099384162e9dc03 (accessed 31 January 2021).
- Schütz, A. (1972), *The Phenomenology of the Social World* (Chicago: Northwestern University Press).
- Straube, C. (2020), 'Speak, Friend, and Enter? Fieldwork Access and Anthropological Knowledge Production on the Copperbelt', *Journal of Southern African Studies* 46, no. 3: 399–415, doi:10.1080/03057070.2020.1770512.
- Trouillot, M. (2003), *Global Transformations: Anthropology and the Modern World* (New York: Palgrave Macmillan).
- Van der Geest, S., and K. Finkler (2004), 'Hospital Ethnography: Introduction', *Social Science and Medicine* 59, no. 10: 1995–2001, doi:10.1016/j.socscimed.2004.03.004.
- Velho, G. (2003), *Projecto e Metamorfose: antropologia das sociedades complexas* (São Paulo: Jorge Zahar).
- Vigh, H. (2009), 'Motion Squared: A Second Look at the Concept of Social Navigation', *Anthropological Theory* 9, no. 4: 419–438, doi:10.1177/1463499609356044.
- Viseswaran, K. (1994), *'Refusing the Subject': Fictions of Feminist Ethnography* (Minneapolis: University of Minnesota Press).
- Wind, G. (2008), 'Negotiated Interactive Observation: Doing Fieldwork in Hospital Settings', *Anthropology and Medicine* 15, no. 2: 79–89, doi:10.1080/13648470802127098.