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How to write? Experiences, challenges and possibilities of ethnographic writing



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Introduction

Julia Pauli

How to write ethnography?

Moving from fieldwork to writing is one of the key challenges of being an ethnographer. Two recent introductions on ethnographic writing (Gullion 2016; Wulff 2016b) describe the disconcerting moment in which the ethnographer, sitting in front of the computer, wonders how to transform experience into text. Helena Wulff (2016b: 1) captures the moment spatially, as a movement from one place to another:

There you are: facing the computer screen. Your 'field', whatever that was, is some distance away, at least for now. You have worked through the materials you collected there, and think you have them in a promising order. Time for the next step: to write.

Jessica Smartt Gullion (2016: xi) adds a layer of anxiety: 'The only thing left for you to do is write it up. You create a blank document on your computer. The cursor flashes on the screen. *Shit*. You freeze. Your mind as empty as the page'. Although most ethnographers can probably relate to these descriptions, until recently advice on how to get started and what to consider when writing ethnography was not easy to find.

Until the 1980s, few anthropologists commented publicly on how they wrote their ethnographies. Their struggles, and maybe also their pleasures, of writing ethnographies remained largely unknown. John van Maanen captured this attitude towards writing in the preface to his seminal book *Tales of the Field*, first published in 1988 and with a second edition in 2011. Returning from fieldwork, he was instructed to 'write up' what he had 'discovered in the field' (Van Maanen 2011: xvi). At the time, advice on forms and styles of ethnographic writing were almost non-existent. Like many others, he relied on reading to gain inspiration, on trial and error, and on the advice of friends. Van Maanen's work is part of the larger Writing Culture movement (Clifford and Marcus 1986; Zenker and Kumoll 2010) of the 1980s and 1990s. Through a critical analysis of his own and other anthropologists' ethnographic writings, Van Maanen meticulously outlines three major forms (or tales) of cultural representation – realist tales, confessional tales, and impressionist tales. One must applaud Van Maanen and his tales of the field that have made ethnographic writing so much more transparent for later generations of ethnographers.

During the 1980s, postmodern anthropologists increasingly questioned how ‘others’ were being researched and portrayed through ethnography. Critical evaluations, sometimes deconstructions, of ethnographic texts went hand in hand with experiments in different forms of ethnographic writing. New epistemological perspectives on the limits and possibilities of cultural knowledge led to innovative ways of writing (Schnegg 2014). Feminist anthropologists, such as Ruth Behar, used ethnography, and sometimes autoethnography, to voice life-worlds that had previously been muted (Behar 1993, 1996; Behar and Gordon 1995). Paul Stoller (1989, 1997) highlighted the relevance of the senses in experiencing culture and integrated them into his sensuous ethnographic writing. With their new ways of writing, scholars like Behar and Stoller deliberately blurred the boundaries between academic and literary writing. Not all anthropologists agreed with this. Thomas Hylland Eriksen (1994: 194), for example, cautioned against indulgence in the ‘rich and evocative language of creative writing’ as this could lead to the loss of anthropology’s scholarly identity and ‘anthropology would cease being an academic discipline’.

Today one can find a diverse range of ethnographic writing styles. In the last few years, a remarkable number of publications has addressed the question of ethnographic writing (Atkinson 2020; Bock 2019; Ghodsee 2016; Gullion 2016; McGranahan 2020; Narayan 2012; Nielsen and Rapport 2018; Waterston and Vesperi 2009; Wulff 2016a). In 2016 anthropologists Helena Wulff and Deborah Reed-Danahay initiated the Palgrave Studies in Literary Anthropology series that explores ‘ethnography of fiction, ethnographic fiction, narrative ethnography, creative nonfiction, memoir, autoethnography, and the connections between travel literature and ethnographic writing’.¹ The last decades have thus seen a significant shift from the restrained manner in which anthropologists had to deal with issues of writing – such as Alma Gottlieb (2016: 96) who, when applying for tenure, did not mention the ‘too literary’ book *Parallel Worlds* she had co-authored with Philip Graham (Gottlieb and Graham 1994) – to the ease with which ethnographers like Francis Nyamnjoh (2008, 2011) or Ellen Wiles (2017, 2020) are able to switch between and mix fiction and ethnography.

Despite these stimulating new ways of thinking and writing ethnography, I want to suggest that, overall, innovative and often experimental approaches and styles of writing have had an only moderate effect on mainstream ethnographic writing. In fact, the writing style of many contemporary ethnographies does not substantially differ from the ethnographic texts Van Maanen discussed almost forty years ago. Even more surprising, a broader discussion on how to write and how to evaluate the quality of ethnographic texts continues to be largely absent in the discipline. Obviously, not all ethno-

1 See the Palgrave Studies in Literary Anthropology webpage, <https://www.palgrave.com/gp/series/15120> [accessed: 4 December 2020].

graphic writing can count as good writing. During the peer review process, for example, it is rather common that reviewers, following Clifford Geertz's (1973) metaphor of 'thick description', comment on the quality of the ethnographic account as 'thin', 'shallow', 'superficial', or 'lacking depth'. On what basis are these judgements formed? What makes an ethnographic description 'thick and deep'? What quality and/or quantity of information is needed to move from thin to thick? Whilst much has been published on how to master fieldwork, advice on how to write a convincing ethnography and get 'from notes to narratives' (Ghodsee 2016) is still rare (see also Narayan 2012). This indicates that the perception that ethnographic writing is the simple process of 'writing up the field notes', without much consideration and stylistic fuzz, continues to be widespread in the discipline. One of the central aims of this special issue is to stimulate more engagement with the crafting and quality of ethnographic texts.

Another challenge in accessing and discussing the quality of ethnographic writing consists in the complex empirical and ethical grounding of ethnographic texts (McGranahan 2020). Unlike writers of fiction, ethnographers depend on others for everything they write (Wiles 2020). 'Where novelists imagine, ethnographers must observe', Kristen Ghodsee accurately points out (2016: 38; see also Fassin 2014: 53). Without 'the people', whoever they are, no ethnographic text could be written. In an interview in 1999, Nancy Scheper-Hughes describes this complex constellation of authorship:

Ethnographic writing is an art form of its own, and it combines description with an ear for the cadences of language, for the odd way that people put things, and the sense that so many of the people we deal with are organic intellectuals who have not had the opportunity to explain to someone outside the village why life proceeds the way it is. So I often work with people who would have been anthropologists if they had been trained to do it, but they didn't have the opportunity. They intuit almost – not immediately, but can learn very quickly what this is about and then they begin to help me and begin to say, here's something you need to see. Or they'll sit down with me and reflect on it. So I can't say where the writing comes from, but I can say that in a sense all anthropological ethnographies are written by a host of people who have pointed us in the direction. (Kreisler 2000: 6)

Scheper-Hughes has written highly compelling ethnographies (for example, Scheper-Hughes 1992). Her honesty in describing how she gets from fieldwork to deskwork showcases a myriad of ethical and literary challenges ethnographers face when becoming authors. How much credit should be given to the 'organic intellectuals' of fieldwork, as she calls them? Should there not be more shared authorship in ethnographic publications (Dawids et al.

2007; Pauli and Dawids 2017)? What happens if not all of the field's intellectuals agree with how they or their views are represented in ethnographic texts? Not surprisingly then, questions of authorship and representation have become crucial for ethnographic writing since the Writing Culture debate. Acknowledging these challenges whilst at the same time appreciating different (written) ways of dealing with them informs the various contributions to this special issue.

Outline of the special issue: from challenges to possibilities of ethnographic writing

The twelve contributions (plus a commentary and a reply) of the special issue address experiences, challenges, and possibilities of ethnographic writing. My aim has been to bring together a diverse range of perspectives on ethnographic writing. To accomplish this, the length of the contributions varies. Authors had the option either to write full length articles or to contribute with shorter pieces. I arranged the contributions into four sections, the first three looking at the challenges, possibilities, and extensions of ethnographic writing, and the last at learning to write ethnography.

The first section tackles ethical challenges of ethnographic writing. The contributions span a broad range of issues that arise when one turns fieldwork into text. Whilst the first two contributions scrutinise questions of representation, the other two reflect on the (im)possibilities of knowing the truth in ethnographic research and writing. The section opens with an article by Julia Vorhölder in which she discusses one of the most crucial questions of ethnographic writing – what she calls ethnography's Achilles heel – the question of confidentiality. Based on a meticulous review of the literature on anonymisation and her own long-term fieldwork experiences in Uganda and South Africa, she depicts and critically discusses different approaches of using and not using pseudonyms. With great honesty, Vorhölder tells us about her own decisions and the moral dilemmas and struggles that have resulted from them. Her paper is followed by a commentary by Sjaak van der Geest to which she then offers a reply. Van der Geest has been amongst the first anthropologists to reflect on and experiment with anonymisation (Bleek 1976; van der Geest 2003). Initially, I had asked him to peer review the Vorhölder paper. Yet after reading it, van der Geest suggested to move away from the anonymity of a peer review and instead to debate questions of anonymity and confidentiality publicly. I appreciate his willingness to contribute to the issue in this manner, and Vorhölder's readiness to engage with his critique, as I am convinced that the exchange offers insightful reflections on confidentiality and deepens the debate. Thinking about my own imperfect decisions whether and when to use pseudonyms, I am certain that Vorhölder's and van

der Geest's contributions will stimulate a much-needed engagement with the intended and unintended consequences of anonymisation.

The article by Eva Riedke tackles another substantial ethical dilemma when writing ethnography. Self-reflectively scrutinising that leaving the field physically does not mean leaving the field socially, Riedke describes how she and her research were entangled in a cycle of hitman killings that took place on the outskirts of Durban, South Africa. Riedke takes as point of departure the demand to make her ethnographic material available for a trial against the hitmen and discusses in detail the ethical conundrums this led to whilst writing. She beautifully captures this process with the image of an 'inner dialogue' between field and desk.

The second two papers of this section expand the discussion of ethical dilemmas in writing ethnography by focusing on the (im)possibilities of truth and (factual) knowledge. Lena Kroeker's contribution takes us to South Africa and the death of a newborn. In her attempt to reconstruct what had happened to the baby, Kroeker interviewed the young mother, her mother-in-law, a midwife, and a paediatrician. Confronted with contradictory narratives and the impossibility of reaching a conclusive explanation for the events, Kroeker describes how the different renderings of what happened fostered her understanding of each narrator's present situation and how they related to each other. She encourages us to view contradictions in narratives less as *challenges* and more as *chances* for ethnographic writing.

Rounding off this section is Dumitrița Luncă who asks what it means for ethnographic research and writing when interlocutors themselves embellish, hide, or lie. Reflecting on her experiences with pitching a story to a narrative journalism magazine whilst doing fieldwork with Romanian migrants in Rome, she investigates whether ethnographers should try to verify the veracity of what they are being told. Luncă's contribution provides thought-provoking insights into how to understand truths, half-truths, and un-truths in ethnographic writing.

The second section takes us from the challenges to the possibilities of ethnographic writing. The first contribution is the broadest in scope of the four papers. Before becoming an anthropologist, Mira Menzfeld worked as a journalist. This professional background raised her awareness for reader-friendly writing styles. Like Ghodsee (2016), she provides transparent and practical advice on how to craft and revise ethnographic texts, including remarks on formulation, argumentation, *pars pro toto* scenes, and the use of symbols and metaphors. In her inspiring contribution, she emphasises that a recipient-friendly ethnographic style is vital for the public engagement of anthropologists.

The next three papers concentrate on individual stylistic devices and narratives of ethnographic writing. Svenja Schöneich focuses on the use of ethnographic vignettes. Vignettes are narrative descriptions of particular

scenes that took place during the fieldwork. They are much more than mere stylistic adornments. Schöneich demonstrates with an ethnographic vignette from her own fieldwork in Mexico that they can also be tools to reach analytical conclusions. Like Menzfeld, Schöneich provides hands-on advice on how to write a convincing vignette.

Rosalie Stolz draws on a brief ethnographic incident in north-western Laos –rumours circulating about an antidote to the impending coronavirus disease – to demonstrate how fleeting encounters in the field can bring unforeseen topics and phenomena to the fieldworker’s attention. In her nuanced contribution, Stolz encourages us to include such contingent encounters and their unfolding in our ethnographic writing. In resonance with Kroeker’s contribution, Stolz shows how allowing for the contingent and unfinished prevents epistemic closure.

In the final paper of this section, Mijal Gandelsman-Trier thinks through issues of authorship and authority in ethnographic writing. She describes in detail how one of her interlocutors, whom she describes as a resolute elderly woman, gave her authoritative information on the history of Ciudad Vieja, the historic centre of Montevideo, Uruguay. Subtly and gently Gandelsman-Trier unravels how she dealt with the impetus of the interviewee’s narrative when she began to write about it. In line with Schöneich’s suggestion, Gandelsman-Trier went beyond using her interlocutor’s account as simple source of information but embedded her statements in a vignette.

The two papers in the third section examine how one could extend and widen ethnographic writing with other forms of representation. Cati Coe compares and contrasts writing to film-making. Based on her own in-depth experiences with ethnographic writing and film-making in Ghana and the United States of America, Coe observes that films have the ability of reaching larger audiences, including the fieldworker’s interlocutors. But films struggle with portraying broader context or showcasing stories that do not have an interesting visual component. Her thorough comparison of the two modes of storytelling illuminates the strengths and drawbacks of ethnographic writing. Based on her experience with writing three monographs and making two short documentaries on the same themes, Coe concludes that film does not replace writing: writing and film-making work in tandem.

The second contribution in this section, by Gbeognin Mickael Hounbedji, considers the use of photographs for ethnographic research and writing. In his highly original contribution, Hounbedji takes a twenty-four-year-old family photograph as starting point for reflecting on the multidimensionality of representations. The photograph was taken in Benin and pictures himself with several childhood friends and their kin. With great ethnographic depth, Hounbedji unravels how a multiplicity of topics – orphans, kinship fostering, schooling, and craft apprenticeship – frame the meaning of the photo-

graph. Like Coe's contribution, Hounghbedji's article encourages us to enrich our ethnographic writing with other forms of representation.

This special issue concludes with a final section on teaching and learning ethnographic writing. I am convinced that an ongoing dialogue about challenges and possibilities of ethnographic writing must include pedagogical questions and student voices. In my own contribution, I reflect on my experiences with teaching ethnographic writing to graduate anthropology students over the last decade. My teaching is underlined by the endeavour of encouraging students to read (more) ethnographies, reflect on writing styles, and work on their own writing in groups and by themselves.

My contribution on teaching ethnographic writing is complemented by the reflection of one of my students of what it meant to be a recipient of this teaching and to learn how to write in a guided manner. Charlot Schneider observes that although there is an emphasis on academic writing at universities, little attention tends to be given to ethnographic writing. Drawing on her own experiences as student in a series of ethnographic writing seminars, she considers what kind of environment is needed to develop writing confidence and skills. She pinpoints three conditions in particular: freedom, experimentation, and collaboration. In conclusion, Schneider advocates for more space to experiment with different writing styles, figurative techniques, and narrations during anthropological training.

The contributions in this special issue bring together a broad range of experiences, challenges, and possibilities of ethnographic writing. All of them show that ethnographic writing, the core of our discipline, must be critically scrutinised but also benevolently appreciated.

I conclude with a reflection on the cover of this special issue. Michael Pröpper, anthropologist and painter, generously allowed me to use his painting *Reis auf Schrift* (rice on writing) as cover for this special issue. The painting beautifully captures what ethnography as practice and text is about. Peasants are working in a paddy field. Their work is demanding – the knees are flexed, the backs bent over. In the background, the paddy field transforms into a written text. The text has been copied from a Tibetan restaurant menu. The letters remind us that the rice harvested by the peasants will eventually be eaten. Our fieldwork, in a way, is also a collective harvesting. It is our responsibility to make the fruits of fieldwork, our ethnographic writing, and the experience of consuming them as enriching as possible.

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Anthropology Anonymous? Pseudonyms and Confidentiality as Challenges for Ethnography in the Twenty-First Century

Julia Vorhölder

Introduction

Disguising the identities of research participants has long been a key, taken-for-granted ethical principle of anthropological research. Due to the long-term and often highly intimate nature of fieldwork, anthropologists become witness to all sorts of ‘happenings’ in their interlocutors’ lives: not only ordinary daily routines and interactions but also conflicts, personal struggles and failures, secret events, and uncensored behaviours that are not meant for public discussion. Even in predominantly interview-based work, in which respondents are more aware of the research setting and better able to control the information they pass on, anthropologists often gather highly personal – and sensitive – data. Thus, the major strength of ethnographic research – getting to know a particular social setting so well that people forget they are being observed and analysed – is also its Achilles heel when it comes to ethical questions, especially the delicate issue of confidentiality.

Because of this ethical quagmire, students of anthropology learn right from the very beginning that it is imperative for fieldworkers to treat the data they gather with utmost care and responsibility, and to guarantee that no harm is caused to those who become subjects of their research. As we all know, and as decades-long debates about research ethics show, this is much easier said than done. Nowadays, anonymisation of research data is one of the most basic and uncontested principles of fieldwork that students hear and read about in methodology courses, books, and ethic codes. At first blush, assigning pseudonyms to people, places, and events would seem like a simple and effective strategy to veil sensitive information and protect research participants from harmful exposure. In practice, however, ensuring anonymity can be a highly complex and morally ambiguous issue – as I discuss in this article.

In my own anthropological training – first as a *Magister*¹ student at Hamburg University from 2003 to 2008 and then as PhD student at Göttingen University from 2009 to 2014 – the challenges and pitfalls of anonymisation were never discussed in any of the methodology courses I attended. I knew, not least from reading monographs and articles written by anthropol-

1 The *Magister* is the former German equivalent of the master’s degree, based on a five-year study programme.

ogists, that using pseudonyms to disguise interlocutors' identities was standard practice, and it never occurred to me that it could in any way be problematic. However, it is an issue I have continued to struggle with in my research.

This article draws attention to the ethical and practical challenges of assuring confidentiality and anonymity in anthropological research: What exactly does it mean to anonymise one's data? To what extent is the use of pseudonyms really an efficient and legitimate way of protecting research participants and their interests? What are the benefits, and what the costs, of disguising interlocutors' identities? And are there situations where it is possible, or even advisable, not to disguise them? I argue that anonymisation, although nowadays taken for granted, poses an underestimated challenge for ethnographic writing, especially in the context of global mobility, the internet, and social media. The use of pseudonyms is not simply a technical or style issue but fundamentally affects the outcome of the writing and the ways this is shared, read, and received.

The first section provides a brief historical overview of the development of anthropological debates on anonymisation and pseudonyms. I reflect on some contemporary challenges and present two case studies in which anthropologists have tried out different solutions to deal with them. In the second part I draw on moral dilemmas, struggles, and failures that I experienced in relation to these issues in my own research. Here I discuss the complexity of finding the right balance between respecting research participants' interests and well-being (whatever these may be), on the one hand, and living up to both the high ethical standards of the discipline and the desire to provide a meaningful analysis of 'real' issues, people, and places, on the other.

Ignorance, scepticism, dogma: on the history of anonymisation and pseudonyms in anthropology

In the early phases of anthropological fieldwork, anonymisation of research data was not a major concern. Anthropologists generally studied small-scale communities in distant places and many of the people they encountered were illiterate. The hierarchies and unequal negotiating powers between researcher and 'informants' were rarely reflected upon or simply accepted as given. Research findings were credited solely to the anthropologist and few of those observed or interviewed ever read, let alone challenged, the final reports and publications. Whilst there are examples of monographs which were taken back to the field and read by some of the school-educated few, access was limited and there was no ethical imperative to share the research results with the people studied.² Anthropologists hardly worried or considered – naively as it often turned out – that the publication of their research might harm the

2 In fact, whilst this is considered good practice today, there is still no such imperative.

people they studied, and they rarely assigned pseudonyms to disguise the identities of research participants and places.

Not surprisingly, one of the earliest scandals that arose around the issue of confidentiality and participant identity in research based on participant observation occurred not in anthropology but in urban sociology, where most fieldwork was conducted 'at home'. William Foote Whyte's *Street Corner Society*, first published in 1943, became well-known for the ethical and methodological challenges posed by his ethnography of an 'Italian slum' (so the book's subtitle). Whyte spent four years living in an Italian community in the North End neighbourhood of Boston, United States of America, to study the social relations of street gangs. Although Whyte used pseudonyms for the place (which he called Cornerville) and the protagonists of his study, some of whom had become his close friends, the latter felt betrayed when they read his published work. They recognised themselves and other community members in the text and felt embarrassed by Whyte's revelations of intimate details of their lives. In a second edition of the book, published in 1955, Whyte addressed these issues in a new methodological appendix. He thus became one of the first researchers to draw attention to the ethical challenges of participant observation, and his book has continued to inspire debate on research ethics (for example, Adler et al. 1992).

In anthropology, the first official, professional ethics code to contain explicit guidelines on the right of 'informants' to remain anonymous was developed by the American Anthropological Association (AAA) in 1971 (AAA 1971), following cases of severe misconduct and violations of confidentiality by anthropologists during the Vietnam War (Pels et al. 2018: 392). More broadly, however, anonymisation of research data only became more widespread in the 1980s – because of two quite different developments. On the one hand was the Writing Culture debate that significantly influenced anthropological research and writing practices: ethnographies became more reflexive, and more attention was paid to the ethical and political implications of doing fieldwork and its impact on those being studied (Zenker 2014). On the other hand, especially in Anglophone anthropology, reflecting on ethical concerns started to become a standard requirement in the 1980s when new ethical review procedures were being imposed by universities and funding agencies as part of a wider shift to a neoliberal 'audit culture'. To this day, such review procedures contain rigid prescriptions of what constitutes ethical research – anonymisation being one of them. Yet they have often been criticised by anthropologists for being designed more to protect the reputation of universities and funders than the interests of research participants (Pels et al. 2018, 392). It took much longer for the use of pseudonyms – and ethical reviews more generally – to become standard practice outside of Anglophone anthropology. As late as 1991, for instance, German anthropologists Martin Rössler and Birgit Röttger-Rössler felt the need to defend, and

extensively justify, their use of pseudonyms in a study of a rural community in South Sulawesi, Indonesia, in light of reviewers critiquing their approach as unscientific (Rössler and Röttger-Rössler 1991).

Nowadays, just about everywhere the practice of anonymising one's research data has achieved the status of near dogma – to the extent that little reflection is given to whether, and how, it makes sense. Now it is authors who do *not* use pseudonyms who must justify their decision and fear being accused of violating research ethics. Obviously, confidentiality and the protection of interlocutors' privacy should be major concerns in any research, and particularly research based on long-term fieldwork. However, the matter is much more complex than often assumed. Rather than thinking of pseudonyms as a magic invisibility cloak that can simply be 'thrown over' names and places at the final stages of writing, if, how, and to what extent confidentiality and anonymity can be ensured needs to be considered from the outset of fieldwork and discussed with research participants throughout the process.

Four questions should guide every ethnographer, ideally before starting the research. The list is by no means exhaustive, and different research contexts may call for different solutions. The first is: can anonymity be ensured in times of global mobility, social media, and the internet, and, if yes, how? Or, as Rebecca Nelson (2015) succinctly puts it in a blogpost on *Savage Minds* (now called *Anthrodendum*): 'how can we hide participants' identities when they're on Pinterest?' This is probably one of the most widely encountered challenges of contemporary ethnography. Maybe, at one point in time, simply using pseudonyms for people and places was enough to ensure that outsiders did not discover the research setting unless they were prepared to undertake major detective work. Most research sites studied by anthropologists were remote and relatively inaccessible, and people around the world were less connected. These days, however, research sites and researchers' homes are much closer: people travel everywhere; everyone and everything is on the internet; and more and more anthropological research is done at home. Although academic work is still predominantly published in commercial journals for a scholarly audience, ever more researchers make an effort to share their analyses with their research participants and the broader public, for instance through open access publications or blog posts.³ In many ways, increased interconnectedness and amplified information flows are a resource for anthropologists: researchers use the online communications and websites of the groups and institutions they study as material and stay in touch with interlocutors via social media. Whilst these developments are largely positive, they do make it much harder to anonymise data. It is no longer nec-

3 For an early discussion of the challenges that arise 'when they read what we write', see Brettell (1993) and Hopkins (1993), of which the latter focuses on anonymisation.

essary to be a detective to find out where and with whom research was conducted; a quick Google-search is often enough to find people and places, even if names have been disguised by pseudonyms (Nelson 2015). True anonymity requires high levels of abstraction, which may not be conducive if researchers want to contribute to discussions on real places and issues.

The second question researchers should consider is: do the research participants themselves want to remain anonymous, and what do I do when they do not? The widespread presence of, and online self-presentation by, research participants on social media sites like Facebook or Pinterest raises the question to what extent people actually want to remain anonymous. Could imposing particular assumptions about privacy and hiding people's identities actually be a form of 'ethical paternalism' and ethnocentricity – as Sjaak van der Geest (2003: 17) has suggested? Even more relevant is the increasingly accepted notion that ethnographies are the outcome of a collaboration between the anthropologist and his/her interlocutors, and that the latter should be acknowledged – and thus named – as co-producers of anthropological knowledge. Some anthropologists have discussed how research participants expressed disappointment after seeing published monographs and finding their names and home places disguised by pseudonyms. *The Handbook on Ethical Issues in Anthropology* (Cassell and Jacobs 1987), published online on the AAA website, examines this in two case studies on anonymity (Jacobs 1987). In both cases, research participants criticised the use of pseudonyms and explicitly asked for their real names to be used in future publications; in both cases, however, the anthropologists decided not to follow this request, for fear of violating current standards of research ethics.

The third question anthropologists should examine before starting their research is: how can 'internal confidentiality' be ensured, especially when it is considered good practice to share research findings with participants? Tolich (2004) makes the useful distinction between 'external confidentiality', ensuring that outsiders cannot identify the research community and location, and 'internal confidentiality', taking care that research participants cannot identify each other. Whilst even in times of global mobility and the internet one can find ways to anonymise data so that those unfamiliar with the research setting will not be able to recognise it, it is almost impossible to prevent insiders from identifying themselves or others. And even though the dilemmas posed by this are well-known, as Whyte's *Street Corner Society* shows, ethical codes and review committees tend to focus nearly exclusively on external confidentiality (Tolich 2004: 101–102). Every anthropologist who plans to share their publications with research participants must grapple with internal confidentiality. For some, the concerns about revealing what one has written to insiders are so great that they never return to the field. Although these worries about offending, or even harming, interlocutors may be exaggerated, there is no easy solution to the dilemmas posed by internal con-

fidentiality. In the end, a compromise must always be found between radical censorship and critical analysis.

Lastly, anthropologists should consider the question of how to deal with the new challenges posed by data management and open science requirements. The open science quest to increase openness, integrity, and reproducibility of scholarly research is the latest buzzword in interdisciplinary debates on research ethics. Across the globe, universities are imposing new standards regarding transparency and accountability in data management on researchers of all disciplines. Although the basic idea that science should be collaborative, transparent, and accessible to the larger public is commendable, anthropologists have expressed various concerns about what they feel is a new – and problematic – form of ethics governance (for a good overview see Pels et al. 2018; de Koning et al. 2019). In particular the suggested requirement to make data – including raw data, such as field notes and interview transcripts – available to other researchers, universities, and funding agencies threatens anthropological understandings of confidentiality and anonymity. In the following (albeit lengthy) quote, Peter Pels and his colleagues (2018: 394) proficiently explain the epistemological, ethical, and political factors that distinguish anthropological data from that of other disciplines and make it less suitable for open science data governance:

Anthropologists [. . .] encounter and record research participants in situations and media where personal identification of and the borrowing of cultural knowledge from other people is not just inevitable: it forms the very foundation of scientific knowledge in ethnography. Moreover, we cannot transfer such knowledge to third parties without editing out the connections between names, faces, secrets and interests – which often renders it useless. Our raw research materials are saturated by personal information and (potential) cultural property precisely because they consist of those kinds of knowledge that are not, and sometimes cannot be, commodified – and yet fully determine social life. Extensive processing of raw materials (beyond mere anonymisation) becomes inevitable if others are to reuse them. This explains why ethnographic researchers question the possible commodification of knowledge by pre-signed informed consent forms: they suspect that such quasi-contractual rituals may sign away respondents' rightful claims to knowledge shared with researchers. (Pels et al. 2018: 394)

It is still unclear how open science and related data management requirements will affect ethnographic research in the long run. Almost certainly, however, they will add a whole new dimension to the – already highly complex – challenges of confidentiality and anonymity.

Creative approaches to anonymisation: Van der Geest and Rottenburg

The literature contains numerous examples of anthropologists who have struggled with the issue of anonymisation and confidentiality. Different authors have come up with various, often quite creative answers and strategies to deal with these questions and dilemmas. In this section I present two of the somewhat more unusual approaches, by anthropologists Sjaak van der Geest and Richard Rottenburg, before turning in the next section to discuss some of my own struggles with, and approaches to, anonymisation.

Van der Geest (2003) dedicated a whole article to the dilemmas he faced when struggling with confidentiality and pseudonyms in his early fieldwork. In the 1970s he carried out research in the rural town of Kwahu-Tafo in southern Ghana, focusing on social ambiguities in extended families, sexual relationships, and birth control. Both his studies touched on delicate and secretive issues. As he became more embedded in the community, his interlocutors confided in him about conflicts, witchcraft accusations, abortion, and other secretive or shameful practices. Van der Geest promised to treat these issues confidentially, being well aware that making them public could have problematic, even dangerous, consequences for individuals and the community at large. It was only when he started writing up his results that he discovered how difficult it was to keep this promise (Van der Geest 2003: 15).

At the time Ghana's academic community was small and Van der Geest realised that pseudonyms for people and places would not suffice to ensure anonymity: his name was too closely associated with the town and the people he had stayed with. Thus, rather than only using pseudonyms for the research community, Van der Geest also decided to hide his own identity – under the pseudonym 'Wolf Bleek'. Given that the challenges Van der Geest faced are by no means exceptional, this solution of disguising the researcher's identity is a surprisingly rare practice in anthropology. And, as Van der Geest soon discovered, it came with several of its own challenges: despite his precautions he did not feel comfortable sharing his publications with his interlocutors for his use of pseudonyms had not solved the problem of internal confidentiality. And when he submitted an article on his research for publication, the text was rejected because the editors objected to his pseudonym which they saw as colliding with the requirement that science should be transparent.

Van der Geest's strategy was successful, however: even twenty years after his fieldwork, no one had made the connection between him and his field site. Yet he became increasingly uncomfortable with hiding his research from those he had worked with:

My decision to 'go into hiding' had several consequences which I found both unethical and simply annoying. I had kept the outcome of my research study from my informants, for their own

good'. On the one hand, I had respected their wish (and the first article of the anthropological ethical code) to keep delicate information confidential; on the other hand, I had deprived them of the possibility of reading what I had written about them (an exchange which, surprisingly, is not stipulated by the anthropological code). They would never be able to 'talk back'. Though trying to make their voices heard by writing about them, I had effectively silenced them. (Van der Geest 2003: 16–17)

Finally, twenty-three years after his initial research, Van der Geest decided to return to southern Ghana and bring along a few copies of his PhD thesis. He reasoned that after such a long time the information contained in the book, including his analysis of delicate matters, would no longer be harmful to social relations in the community. Indeed, many of his elderly interlocutors had died and the younger ones were preoccupied with their day-to-day lives and not concerned with 'gossip' from the past. And yet, people were interested in his work – and some of his former interlocutors expressed disappointment that their names, and the name of their town, did not appear in the book.

In his 2003 article, Van der Geest self-critically reflects on his decision to conceal his research from those he had studied and to anonymise his data to the extent that the 'real' people who had participated and supported him in his research were hardly recognisable:

My struggle with confidentiality and the use of pseudonyms has taught me at least one thing: ethical rules and feelings about right and wrong are as much subject to cultural variation as the topics and themes we study in other communities and societies. Anthropologists have done their utmost to combat ethnocentrism in intercultural communication, but they have been ethnocentric in applying their own ethical standards in their fieldwork. (Van der Geest 2003: 17–18)

After experimenting with 'total' anonymisation in his earlier work, Van der Geest chose the converse strategy in follow-up research on aging and old-age care in the same Ghanaian community. He published under his own name and openly identified the town and the people he worked with. By explicitly naming his elderly interlocutors, he wanted to show respect and recognition or, as he put it: 'I want them to be proud of the fact that their life histories – good or bad – and their reflections about being old have been published and are being read by people in different parts of the world' (Van der Geest 2003: 17).

Where Van der Geest chose the thought-provoking strategy of disguising his own identity and keeping his publications from his interlocutors, an-

thropologist Richard Rottenburg developed a similarly radical though ethically less problematic strategy in his widely celebrated book *Weit hergeholte Fakten: Eine Parabel der Entwicklungshilfe* (2002) (published in English in 2009 as *Far-Fetched Facts: A Parable of Development Aid*).⁴ Wanting to conduct an ethnographic study of the processes underlying development aid in Africa and the interactions between various stake holders – development banks, international experts, local managers – he was faced with the particular challenges of studying up. Few experts in the highly politicised world of international development would have felt comfortable knowing that a critical anthropologist was observing their work (Rottenburg 2009: xxxiv). So, rather than setting out to study and write about a particular project or organisation, Rottenburg engaged in a retrospective study of his own experiences when working on a number of development projects in the 1990s – a total of nineteen months of multi-sited fieldwork in nine development organisations located in five African countries and one European development bank (Rottenburg 2009: xviii). In a further step, rather than talking about real places, people, and projects, he composed his account as a fictionalised ethnography in the style of a literary narrative with four voices.

Rottenburg's account portrays the challenges of implementing a large-scale waterworks improvement project in 'Ruritania', a fictive country in sub-Saharan Africa. The project is funded by the 'Normesian Development Bank' and carried out by a private consulting firm – both based in 'Normland' – under the supervision of African project-executing agencies. The story is narrated by anthropologist Edward B. Drotlevski, who appears as the author of the three main parts of the book. The account features Normesian consultant Julius C. Shilling, who represents the 'voice of development', and Samuel A. Martonosi, another anthropologist, who embodies the position of the sceptics. Rottenburg himself only appears in the prologue and the fourth part of the book, in which he brings together the analytical threads of the story. His main interest lies in the elementary questions that play a role in all development projects, in particularly the processes of translation and inscription that take place in the interstices between different cultural contexts, knowledge traditions, and social settings (Rottenburg 2009: xvi). Therefore, fictionalising the people and places in this way does no harm to the analysis:

All of the characters in the present text have been given fictional names and are literally figures in a play. They do not depict any real, existing people but are constructed from the cumulative characteristics originally belonging to the various people I met during my tenure in the field of development cooperation. They wear the masks and play the roles prescribed by the script, and yet at the same time they perform with the manoeuvring room that I found typical of the development arena. At issue are not

4 For reasons I outline below, I refer to the English translation of the book.

their individual capabilities, honesty, or good intentions; rather, it is presumed that all figures possess the normal competency required for the roles they play. If their interactions do not bring about the desired results, this cannot be traced back to the failing of one or another of the actors. (Rottenburg 2009: xx)

In the English translation of the book, Rottenburg provides a much more detailed reflection on his reasons for fictionalising his account (Rottenburg 2009: xviii–xx) than in the German original. Most importantly he states that his main aim was to draw attention to the structural problems underlying *all* forms of development cooperation rather than questions of individual responsibility for failures in specific projects. This strategy proved quite successful: Rottenburg found that even people working in the field of development were open and interested in reading his book, and – rather than taking offence or considering the problems exposed as only concerning certain organisations and projects – complimented him for his accurate analysis of general principles, contingencies, and fundamental dilemmas that they all faced in their work. For Rottenburg, his choice of fictionalising the account rather than using pseudonyms (which would have been easy to decode by insiders) was also a ‘question of decency’: ‘[I]t seemed to me intrusive and offensive to publish a text in which human beings were so ruthlessly exposed, even if they had previously given their approval for the study’ (Rottenburg 2009: xix). Finally, Rottenburg was also interested in experimenting with alternative forms of ethnographic writing.

Rottenburg’s approach is one of the most thorough and creative forms of anonymisation that I have come across in the literature. However, whilst his strategy might be transferable to studies whose main aim is to provide theoretical reflections on general principles and structural dynamics in a given field, fictionalisation seems less suitable for analyses of very concrete people and places. As both his example and that of Van der Geest demonstrate, strategies for anonymisation are highly dependent on the particular context of the research – the topic, the kind of interlocutors, and the type of analysis. Every strategy comes with certain costs, and how one evaluates these costs may change over time – as the Van der Geest case clearly shows. Furthermore, a strategy that works well in one context may be problematic in another. In the end, there is no easy solution to the challenges of confidentiality posed by ethnography. However, being aware of and reflecting on these challenges from the outset is an important prerequisite for dealing with them – as I have learnt from my own struggles and mistakes.

Examples from my own fieldwork

In this final section of the article, I draw on examples from my MA thesis, my PhD, and my more recent postdoctoral research to discuss some of my own

struggles and ways of dealing with anonymisation in my ethnographic writing. Unfortunately, I must admit, in neither one of those projects did I spend much thought on how *exactly* I would anonymise the data before starting my fieldwork; I just assumed, like so many others, that I would simply use pseudonyms to conceal the identities of my interlocutors. Confidentiality was obviously an important concern for me – but it always remained somewhat abstract, until I came back from the field and sat down to write. Only then did I become aware of the full complexity of the issue. Over the years, I have tried out various strategies depending on topic and type of publication, and often allowed my gut feeling to guide me. It is for these experiences that my own writing provides helpful insights into the challenges of anonymisation. I now examine each of my three research and writing projects in turn.

In my master's thesis I did not use pseudonyms. The research was situated in the field of organisational ethnography, and I spent three months studying a faith-based organisation which was involved in various types of social work in inner-city Pretoria, South Africa. Fortunately for me, the organisation was committed to critical self-evaluation and, unlike many anthropologists conducting this type of ethnography, I did not have problems with access. In fact, when I approached the organisation and explained my research concept – I was interested in studying participatory development approaches – the HR manager openly invited me to join their volunteer programme which would enable me to carry out participant observation of their day-to-day work. Throughout the three months of intensive and sometimes challenging fieldwork, I became witness to very intimate, sometimes sad and discouraging, moments and situations: personal struggles and tragedies of staff members and beneficiaries, conflicts, ethical transgressions, work-related failures, mistakes, and inconsistencies. Obviously I also gained many positive insights and, overall, I was extremely impressed with the approach of the organisation and the dedication of everybody involved in the work.

When returning from the field and starting the writing process, I was confronted with the problem of internal confidentiality. The organisation had explicitly expressed interest in a critical analysis of its work, and I did not want to sugar-coat the shortcomings and problems I had observed. But I also did not want to expose the failures and vulnerabilities of individual staff members or beneficiaries. Simply using pseudonyms did not seem to provide a satisfactory solution: the organisation was small, the relationships between members were close, and everyone knew everyone. In the end I decided that many situations I witnessed or was told about in confidential conversations were simply too delicate to include, even if they would have provided great material for analysis, and I did not discuss them in the final text. I kept my analysis at a fairly high level of abstraction and used only concrete examples that allowed me to focus on general problems or principles underlying the work of the organisation. I referred to interviews by numbers, without men-

tioning names or providing personal details of the speakers – although even with these measures insiders would still have easily identified each other. To my great relief the organisation received my thesis positively and granted me permission to publish it as monograph – without requiring me to anonymise its name (Vorhölter 2009).

In my PhD, I was very sloppy with anonymising my data. This was not primarily because I was careless or ignorant; in fact, I spent a lot of time contemplating the issue. But, somehow, I ended up not using pseudonyms. The thesis was based on eleven months of fieldwork in Gulu, the biggest town in northern Uganda. It analysed the situation of youth and intergenerational relations in the aftermath of the twenty-year war between the Lord's Resistance Army and the Ugandan government. The main aim of the research was to understand how the young people who had grown up during this time of profound social turmoil imagined their future society, how they pictured their role in this society, and how they coped with the expectations directed at them by their elders, humanitarian actors, politicians, and society at large (Vorhölter 2014).

I cannot remember exactly when I decided to refer to my interlocutors by their real first names; maybe it happened gradually. I certainly had the intention of using pseudonyms when I started the writing process after returning from the field. I tried to come up with 'good' pseudonyms for the individuals I was dealing with and a strategy for keeping track of them. But I found that this inhibited my ability to write: not only did the pseudonyms make me feel strangely detached from the people I had lived with for almost a year, but they also made me feel disconnected from my writing. So I switched to the real names, thinking I would re-introduce pseudonyms at the final stages of the editing process. But I never did; something just did not feel right about it. After all, the first names of most of my interlocutors were so common in northern Uganda that no one would be able to identify them except for insiders familiar with my immediate research context – and these would have been able to do so even if I had used pseudonyms. Did it really matter then whether I called someone Acio rather than Akello, Daniel rather than David? Furthermore, with very few exceptions, the scenes and events I discussed were quite common or had taken place in public. Unlike for my master's thesis, I did not think that writing about my research participants could cause them or their community any obvious harm. But in the end, the main reason for sticking with people's first names was, simply, that I felt a need to properly acknowledge them. Most of my interlocutors were young people from fairly marginalised backgrounds whose voices were rarely heard or appreciated in public – although they were shrewd observers of what was happening in their society. Even though I did not think any of them would ever read my thesis, I simply wanted to show respect and accredit to them the perspectives they had shared with me. I was reasonably certain that they would have wanted to

be recognised by their real names – my big mistake was that I had failed to explicitly ask them about it during my fieldwork.

Although I still believe that my reasons for using real names were valid, I never felt completely comfortable with the approach and, in retrospect, wish I had not done so. A year after I published my PhD,⁵ I returned to northern Uganda and provided copies of the book to the library of the local university and to my colleagues at the institute with which I had been affiliated during my research. The book was also available open access online. One event in particular forced me to confront the ramifications of my approach. I was attending a graduate class on research methods when the professor, a close friend and mentor during my research, passed my book around and commented that there were many things they could learn from it when writing their own theses, except for one: my inadequate adherence to the ethical principles of the discipline. To my great shame and embarrassment, she not only publicly scolded me for not using pseudonyms but then read out a passage in which I mentioned the name of one of the students present in the room. It was a horribly awkward moment, and although no one else said anything and we went on to talk about other things, I still feel uncomfortable when I think about it today.

A few days after this event, I visited another close friend, who had also been an important interlocutor, at her family home. I had come to know her parents and siblings quite well during my fieldwork and had conducted a long interview with her father, a retired schoolteacher, towards the end of my stay. He was a keen social critic, and I knew he would have loved to read about my research findings. I really wanted to give him a copy of my book. However, I could not stop thinking about my failure to use pseudonyms. I referred to him and our interview in different parts of the book – all in very positive and respectful ways. But in one chapter I used a conflict between him and his daughter as a case study to discuss inter-generational conflict. I had reconstructed the conflict from two separate conversations in which each of them told me their understanding of the situation. Though the conflict was not particularly unusual, I was worried that he might be upset or angry – not so much at me for exposing it but at his daughter for the views she expressed. It could well be that it would not have been a big deal, and I am not sure using pseudonyms would have made any difference as he might still have recognised his family; but in the end I never gave him the copy I had reserved for him.

Given the moral dilemmas I faced with the usage of pseudonyms in my PhD, one would think that I would have had a clearer sense of how to go about anonymisation in subsequent work. But I still struggle with the issue. In

5 In Germany, it is compulsory to publish the PhD thesis within a year of completion. Because of this, PhD-based monographs are usually published with only minor revisions from the original text and often without thorough review by the publishers.

many ways, the research I have been conducting for my postdoctoral project is quite different from my PhD. It focuses on changing discourses on mental health and emerging forms of psychotherapy (for example, Vorhölter 2019). The work is again based in Uganda, but the main research site has shifted to the capital, Kampala, although I have continued to do some fieldwork in Gulu. I still draw on participant observation and the informal ‘hanging out’ with interlocutors – which were the main methods I used during my PhD fieldwork; but my most important data stems from expert interviews carried out with leading figures in the mental health sector: psychologists, psychiatrists, therapists, counsellors, and researchers. Though growing quite rapidly, this sector is still small. At its core, it consists of a small group of Ugandan professionals and several expatriates who have been working together to expand mental health care services throughout the country. They have established university programmes for psychology and psychiatry, set up private practices, founded the National Counselling Association, and have been involved in various local, regional, national, and international initiatives to increase awareness and improve access to public mental health care. Most of them know each other in some form or other, which was a great advantage for me during the fieldwork. By using snowball sampling, I quickly developed a good overview of the ‘psy community’ and found the most relevant interview partners. However, this created particular challenges for anonymisation – which, once again, I only really started to think about after returning from the field and when starting to write.

Most of the people I had interviewed were reasonably well-known public figures, especially in their professional field. Most of them came from middle-class or upper-middle-class backgrounds⁶ and had university degrees; some were involved in academic research and had published books and articles themselves. Was it useful, necessary, or even disrespectful to use pseudonyms for them? Furthermore, how would I anonymise the institutions they were working for given that the sector was so small? There is only one designated mental referral hospital in Uganda, for instance. I saw my research as a contribution to an ongoing, interdisciplinary, and arguably important debate on mental health in Uganda, so anonymising or even fictionalising the research situation did not seem reasonable strategies. And even though I was working on a highly sensitive topic – mental health – most of my data focused on broader discourses, practices, and developments, and not on individual patients or case histories. So not using pseudonyms did not seem to pose a risk to my interlocutors. Regardless of this, I decided in the end to use

6 Social class is an important factor that may affect how anonymisation is dealt with – albeit in complex ways. Whilst anonymisation is considered particularly crucial for vulnerable populations, including people from lower-class backgrounds with little or no formal education, it might be exactly those vulnerable and marginalised groups that anthropologists want to give a voice to – like I did in my PhD.

pseudonyms for interlocutors and for some of the institutions. After my previous experiences, I did not want to be found lacking again for not respecting one of the most fundamental anthropological principals. At the same time, I did not go out of my way to disguise the identities of my expert interviewees. In fact, in each publication I included a footnote alerting readers to the fact that those familiar with the field would probably recognise some of the people and places I discussed.

Once again, this proved to be insufficient. After publishing an article in *Current Anthropology* (Vorhölter 2019), I was contacted by the director of research at a prominent NGO. In his email, he noted that he had read my article with ‘some concern’ because I quoted extensively from an interview with one of his employees – a leading figure in mental health interventions in northern Uganda. Although I had used a pseudonym for the interviewee, I had used the real name of the organisation and the director of research had easily identified his employee. The director demanded that I should have requested written consent from the organisation to use the interview. I was shocked. Had I failed in my ethical responsibilities once again? I considered the email for a few days. Then I wrote a long response explaining the difficulties of anonymising well-known public figures. I outlined my research focus, methods, and ethical principles, and clarified that I had obtained verbal, though not written, consent from the employee in question after fully informing him about the focus and purpose of my research. I stated that I had not considered it necessary to notify the organisation because, apart from this one spontaneous interview, I had not done any research on the organisation. In fact, I had only mentioned it once in passing and not even critically. I actually have great respect for the work of the organisation and of the interviewee in particular. The more I thought about his complaint, the less I was convinced that it was justified, and told him so. I waited anxiously for his reply. Fortunately, this time his tone was much friendlier. Thanking me for my clarifications, he explained that he had to be concerned about the reputation of the organisation as my interviewee, even though expressing his personal opinions, was a representative of the NGO; his statements (for instance about other actors in the field) could therefore reflect back on the organisation. Finally, he expressed that he found my analysis of the situation quite accurate but would appreciate that I inform him when mentioning the NGO in future publications.

In my latest publication I finally found an approach to confidentiality that I feel less ambivalent about. This article (Vorhölter 2020) emerged out of an interview I conducted with a Ugandan psychiatric nurse who played an important part in the history of Ugandan psychiatry. I had been referred to her by the director of the mental hospital where she had worked for most of her life. Quite unlike other data I had collected, this interview turned out to be a biographical account of her fascinating career. I left the field soon after

but kept thinking about the nurse and her life story. It provided a rare and important perspective on the development of psychiatry in Uganda and, perhaps, Africa more broadly, and I felt it could be interesting and relevant to a broader audience. So I started writing it up – initially just for a conference paper, then for an article. Again I was struck by a series of questions: Should I anonymise her name? Would this make sense if I wanted to tell her personal life story? It was not really for me to decide. But how could I contact her and send her the draft? She was in her seventies and did not use email. Would she even remember me? What if she hated what I wrote? I contemplated these questions for a long time. First I submitted the article to a journal using a pseudonym, thinking that a rejection would solve the issue for me. In the end, I contacted the director of the mental hospital who had initially referred me to her, explaining my difficulties. Not only did he answer my email (I knew how busy he is), he also agreed to get in touch with the nurse and print out my draft for her to read. To cut the rest of the story short: she added a few comments and gave me permission to publish the text using her real name. Today, as I am writing this, I sent another email to the director, this time including a copy of the published article for the nurse. I still feel strangely anxious about it – but at least ethically I think I have done the right thing.

Conclusion

In anthropology today, just like in other social sciences, anonymisation of research data is considered standard practice. However, the issue is a lot more complex than it seems. In many ways, the particular challenges posed by confidentiality bring to the fore some of the most fundamental ethical and moral dilemmas inherent in the anthropological research approach. And there are no straightforward solutions to deal with these. If and how research data can and should be anonymised is highly context-dependent and cannot be governed by an a priori one-size-fits-all recommendation. Precisely for this reason it is important for researchers to consider how they intend to deal with anonymisation before setting out on their fieldwork – every time anew.

At least since the 1970s anthropologists have openly debated and struggled with the issue of confidentiality. Fifty years later, increased global mobility, new information and communication technologies, and recent calls for open science pose ever more challenges to the disguising of identities of research participants. Drawing on examples from the literature and my own fieldwork, I have discussed different ways anthropologists have approached the challenges of anonymisation in this article. Thereby my aim was to highlight some of the problems, ambivalences, and contradictions that come with different solutions.

Reflecting on my own approaches to anonymisation, I certainly feel that some of the strategies I chose in the past were problematic. And I would change

them if I could. However, my insights into the problems raised by my previous solutions have not meant the end of my doubts and struggles. Whilst I hope that I can learn from past mistakes, every research is different and lessons from one 'field' are not easily transferred to the next.

Nevertheless, and as a way of concluding, if based on my own experiences I had to give any advice on matters of anonymisation, it would be the following: where possible, one should consult with research participants at different stages during the research about what *they* think the right strategy for anonymisation is – although this does not mean their preferences can always be accommodated.⁷ Some projects may be more suitable for doing this than others. Thus, discussing the complexities of anonymisation before an interview, when interlocutors are often cautious and feel unsure of what they are letting themselves in for, may be less effective than raising the issue afterwards, when things have loosened up a bit. It is also likely that there will always be people one cannot ask, especially in research based on participant observation. Furthermore, interlocutors will almost certainly have different opinions on who, how, and what should be anonymised. Not everyone will be fully aware of all the implications of exposing their names or locations. In the end, it comes down to the sensitivity and potential harmfulness of the information collected. Maybe sometimes the only way to protect interlocutors is simply not to write anything particular about them at all.

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⁷ Some authors also let their interlocutors choose pseudonyms (see, for example, Behar 1993: xiv). For her book *Ambiguous Pleasures* (which, as the title suggests, discusses quite delicate and intimate issues), Rachel Spronk (2012) asked her research participants to read and approve the ethnographic portraits she had written of them, and to choose pseudonyms for themselves (personal communication).

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Anthropology Anonymous? Some Comments to Julia Vorhölter

Sjaak van der Geest

Julia Vorhölter's article 'Anthropology Anonymous? Pseudonyms and Confidentiality as Challenges for Ethnography in the Twenty-First Century' is a wonderful contribution to this special issue and to the growing number of discussions on the ethics of and in anthropological fieldwork. I welcome and admire her openness, which has produced an exciting self-reflection on fieldwork and a balanced overview of the dilemmas accompanying our attempts to be honest to our research participants, to our academic colleagues, and to ourselves. Her focus on how to deal with confidentiality and the anthropological tradition of anonymising research participants and fieldwork locations in order to protect the participants' identity shows that each 'solution' has its problems and none is perfect.

Julia's article raises a number of questions, some of which are mentioned only in passing and may deserve more attention. When the editor of this special issue invited me to review the manuscript, since the author discusses my own struggle with confidentiality and anonymity, I hesitated. Having often been subjected to the tyranny of peer reviewers myself and having been invited (read: forced) to also mention such and such an author, article, or issue (satisfying the reviewers' personal tastes or even including their work), I wondered if I should list all the questions that emerged in me whilst reading the text. It is usually annoying to have to include so many other aspects that a reviewer might want (I am still speaking of my own experience). Obeying the reviewer is likely to disturb the flow of the author's argument and may turn the text into a hotchpotch of many additions and small excursions.

I was therefore thinking of an alternative: instead of the conventional double blind peer review, I could just as well write a few pages of comments to which the author (Julia) could then respond, if she wanted, somewhat similar to the renowned *Current Anthropology* format. I thought that ethical issues based on a wide variety of fieldwork experiences would lend themselves well to this type of dialogue/discussion; this approach allows them to come more to life. Both the editor and the author agreed, and thus I wrote this brief text with my thoughts about Julia's thoughts, and about my experiences versus hers. Although numbered headings are 'not done' in anthropology, I have used them here for practical reasons: the comments are often too closely connected to deserve separate headings.

1

Using or not using pseudonyms is the question that runs through Julia's article. Starting from the ethnographic use of applying anonymisation when in doubt of possible harm to participants, she sums up several problems connected with this custom. One is that anonymity does not work, at least not anymore. In the pre-internet age it may have worked, but with today's countless tools for searching the internet, it has become a futile strategy. I agree only partly. The internet has also proven very effective in concealing people's identity. It is now possible to say anything about anything or anybody without readers knowing who the speaker is. Similar techniques could be applied when anonymising research participants, locations, and even authors.

2

Does the anonymity of participants and – certainly, as in my case – of the author obstruct the transparency of the research? To limit myself to the latter, correspondence via internet with a Wolf Bleek (my pseudonym of many years ago) would be very simple and comfortable today. There is no need for a personal or institutional postal address and colleagues could discuss and raise questions about my research without knowing my identity. I could even reveal that my name is a pseudonym, without endangering my anonymity. If a fellow anthropologist had good reasons to ask for my identity and/or the exact location of the research, I could tell that specific person on condition of confidentiality. There are several devices I could use to hide my location and other identifying data. Only clever hackers would have the means to trace me, though they are unlikely to be interested in anthropology.

3

At the same time, however, it is indeed possible to discover ethnographic fraud through the use of current digital media, even when an author conceals information about research location and the identity of informants. Quick international communication can reveal that certain data in a particular region or country are untrue and made up by the researcher – as was the case when a Dutch anthropologist who published several (English) articles about Bosnia was highly criticised by Bosnian and Croatian colleagues. Fifty years ago he would have written in Dutch and, in the absence of circulation of his work through the internet, local academics would not have noticed his fraudulent imagination.

4

Julia writes: ‘true anonymity requires very high levels of abstraction, which may not be conducive if researchers want to contribute to discussions on real places and issues’. I hold the opposite view: by anonymising people and places, we are rather able to describe the small (and very private) details and ‘imponderables’ (Malinowski’s term) that make a good ethnography. We can do so *because* the participants are unknown. When the participants are known, the author feels obliged to cut out information that may invade their privacy. Of course, if the anonymising fails, this does not apply. I should, therefore, explain what I mean with solid anonymity. The most effective ‘trick’ to achieve the protection of participants’ identity is the introduction of red herrings: providing false information about irrelevant details of the participants. Dependent on the purpose and context of the research, one could, for example, turn a baker into a butcher, a teacher into a cleric, even a boy into a girl. A seven-year-old can become a twelve-year-old, a Roman Catholic church can turn Methodist, the number of inhabitants or the number of children someone has can be changed, etcetera. By giving exact (but incorrect) information about a person or place, without in any way changing the thrust of the ethnography, the reader may never identify the participants. This would provide external as well as internal confidentiality, a crucial distinction Julia makes. The use of red herrings should be mentioned in the methodological section of the study, where the use of fictitious names is reported. Sarah Lamb (2018: 67) too modified a few identifying details to protect anonymity of single women she interviewed in India. I do not claim that this is a 100% effective way of guaranteeing confidentiality, but it nearly is. Within the community, amongst the participants, people may suspect who is who, but they can never be sure. Doubt will remain and so will anonymity.

5

I fully agree with Julia’s concern about the ethnocentric or paternalistic imposition of a pseudonym on people who never asked for it. Giving them another name can be understood as stealing their identity or reducing them to an object. Rachel Spronk’s (2012) solution – inviting her participants to choose their own pseudonyms – is indeed a respectful and elegant way to avoid this unpleasant experience. Sarah Lamb (2018: 67) selected names that fitted her sense of the participants in her research, based on the fact that Bengali names have a specific meaning. I sometimes did the same by choosing names of characters in popular Ghanaian Highlife songs, though I never asked the person’s permission (Van der Geest 2011: 139). Respect for one’s participants must always be the guide to choosing the best way of dealing with people’s privacy. In one case it could be anonymisation, in another the

opposite. In my own research, as discussed by Julia, I first decided for anonymity, but for my later conversations with older people I applied the ultimate openness about their names, as this would fulfil their and my wish to contribute to their memory, a crucial thing in the context of their veneration of ancestors (Van der Geest 2003).

6

This leads me to the role of ethical commissions which – without consulting the people involved – tell researchers what is ethically right and wrong. Let me quote Julia, quoting me: ‘ethical rules and feelings about right and wrong are as much subject to cultural variation as the topics and themes we study’. Somewhere else I have expanded on this ironic odium of ethical rules for anthropologists (Van der Geest 2011). Personally, I have never been subjected to the judgment of an ethical commission, but several of the students I have supervised have had to. Their experiences were sometimes both frustrating and absurd. One student had to resubmit a bulk of about 30 copies of his research proposal because they had been printed double-sided and had to be one-sided (or the other way around). This may not be a good example of ethnocentric ethics as such but of the bureaucracy of ethics. The experiences of two Bangladeshi researchers are absurd to the point of being comical (Zaman and Nahar 2011). Papreen Nahar, who was to study childlessness amongst women in Bangladesh, was told by an Australian ethical commission that her interlocutors should have access to a counsellor since the topic was highly emotional. When she explained that there was no such person anywhere near the village in which the women were living – the closest would be in the capital city Dhaka, about 200 km away – she was told to at least provide the women with the telephone number of a psychiatrist there. Nahar gave up trying to explain to the commission the conditions in rural Bangladesh and obediently gave some of the women the telephone number. The women were surprised – after all there was no telephone in the village that they could have used – whilst the researcher had protected herself and the commission.

7

The cultural variation of right and wrong ethics with regard to protecting participants’ identity is linked to the cultural variation in the perception and experience of privacy in different societies and layers of society. Anthropologists, who often study intimate, private matters, have given little thought to what privacy means in the cultures in which they are working. Obviously, this omission has caused uncertainty about the right way of dealing with participants’ identity in their publications. Privacy – currently one of the hottest

issues in public and academic discussions in my own society – also needs our attention in anthropological research abroad.

8

Some degree of ‘paternalism’ with regard to protecting participants’ identity is sometimes needed and prudent, as is illustrated in two cases in Jacobs (1987). Whilst participants may request that their real names be mentioned in the publication, the researcher might believe that they are not well aware of the consequences that this could have for themselves and for others. Revealing their names may, for example, lead to the identification of vulnerable others. Relatives, neighbours, or colleagues may be not amused by what is said about them. Ideally this dilemma should be discussed with the people concerned before publication, but this may not be possible for all kinds of reasons. When in doubt about the possible consequences of revealing people’s real names, it is wise to be careful and to keep their names hidden; once their identity has been revealed, this can no longer be undone.

As I mentioned before, I was quite pleased with my decision to present the identity of the older people in Kwahu Tafo to readers all over the world. I posted a gallery of the elders on my website, with their portraits and a brief caption on their life history. One of these captions read:

Nana Kwasi Antwi was a tailor who became famous for his speed. People gave him therefore the nickname ‘Five-Minutes-Batakari’ [meaning that he could sew a Ghanaian smock within minutes]. From the money he earned he was able to build his own house. He had seven children, most of whom settled abroad. When I met him he was almost blind and unhappy. Old age was miserable, he said, because he could not work as before. Moreover, he was suspicious of the people staying with him in the house and complained about their behaviour towards him. I never fully understood his complaints but felt sorry for him that his successful life ended so sadly. He died in September 1994, less than five months after I got to know him. The family used this picture, showing him behind his sewing machine, during his funeral.

About ten years after I posted Antwi’s portrait with the above text, I received an email from the old man’s granddaughter, who was living in Canada, telling me that I should remove both the picture and the text. What I had written was untrue and the family was very upset about my words, she stated. I explained to her that I had had conversations with the old man, one of his sons, and his wife. Neighbours had also told me about the affair. What I had written reflected exactly what I had seen and what I had been told.

The granddaughter's mother then took over the email correspondence and threatened to sue me and make me pay dearly for the lies I had published about her father. She also accused me of having betrayed the trust of innocent people. I told her that I would not remove her father's portrait and my brief summary of his life but that I would give her father a fictitious name, which I did. With this our correspondence stopped. But I kept on thinking about the reaction from Canada and how different people's reactions to (non) anonymity can be. A significant aspect of this was that the *Canadian* relatives had protested whilst the *Ghanaian* relatives never raised any objection but rather had assisted me in the research and shared with me their sorrows about 'Nana Kwasi Antwi'.

9

Julia called my decision to take a pseudonym for myself a creative solution. She may be right, but it certainly is an unusual one. Other examples are rare. When I asked William F. Whyte in 1976 if he had ever considered the option of going into hiding whilst writing his *Street Corner Society* (1955 [1943]), he responded: 'I wanted to get whatever credit was due that work'. Then he added: 'there might also be a more respectable reason: if a book makes any sort of mark at all, it may stir up a discussion in the profession, and it is rather important for the exchange of information to include the author in that discussion'. I discussed this aspect in my first comment above, and admit that in Whyte's period, discussion on the book would have been difficult without his name being leaked. In addition, his research was 'at home', which would probably make a pseudonym impossible to maintain.

In my own case I felt that I had no choice but to assume a pseudonym, after I had promised those who had placed their trust in me that I would take care that nobody would find out what *they* had told me. Theoretically there were two other choices: keeping my ethnographic description at a very high level of abstraction or not writing at all (both possibilities mentioned by Julia). For me, both of these alternatives were one and the same and out of the question. 'Abstract ethnography' is a *contradictio in terminis*, or rather not an ethnography. And, obviously, I was not prepared not to write.

10

Interestingly, when my PhD appeared in the Netherlands, it attracted some publicity – not for its contents but for my pseudonym. I was invited to write about my reasons in the Dutch anthropological journal *Sociodrome* (Bleek 1976). An ambiguous pleasure. A few colleagues reacted to this, to which I again responded. Apparently using a pseudonym was a way to attract attention rather than an effective manner to remain invisible. One journalist

wrote that my pseudonym was a pseudo-pseudonym and another wrote that I should keep silent if I indeed wanted to stay unknown. She was probably right, but it is also true that this discussion took place in Dutch in the Netherlands and never reached Ghana, where it was critical for the pseudonym to remain in place.

11

Finally, the most remarkable thing in Julia's reflection is the fact that – in spite of her anxiety about anonymising and confidentiality – she failed every time to discuss this with her participants whilst she was still with them in the field. I share her conclusion that this should indeed be the first and most important thing to do before deciding. It not only demonstrates the respect that research participants deserve; it also shows our recognition that they, more than we, are the owners of the data that we publish for the rest of the world to read.

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Reply to Sjaak van der Geest

Julia Vorhölter

I would like to thank Sjaak van der Geest for his thoughtful comments and reflections on my article. The points he raises complexify the debate on anonymisation in important and interesting ways, and I would have had trouble, indeed, to adequately address his questions in a conventional peer review procedure.

Some of Sjaak's objections to my arguments once again show that the possibilities for, and outcomes of, anonymisation are highly context dependent. Take his points on the internet, for instance. In some research situations, the internet may indeed facilitate concealing people's identities, like Sjaak suggests. It can enable correspondence with an anonymised author without revealing the latter's identity. And it can certainly help uncover fraud or enable research participants to engage in debates about their lives and 'talk back' to the anthropologist, in ways that would have not been likely previously. However, the fact that it is now possible 'to say anything about anything or anybody without people knowing who the speaker is' can also be highly problematic and raises the question of what distinguishes scientific data from other discourse – particularly in the current era of alleged post-factual or post-truth politics (see Harsin 2018; Stein 2017). In contexts where research-based accounts are considered, by some, as no more authentic or truthful than any other statements, it may well be important to (re)establish academic and personal credibility, for instance by revealing one's institutional affiliation and details about one's professional background and previous research experiences – even if it comes at the cost of anonymisation.

Similarly, in some studies it might be viable and unproblematic to completely anonymise names of places or institutions. In others, this may be less of an option. For instance, when contributing to an ongoing debate on a current issue in which names of places and key actors are already being used by other researchers in the field or the media, using pseudonyms may not make a lot of sense. Complete anonymisation is particularly difficult when doing applied research, where one of the key aims is usually to influence policy debates or interventions (be they medical, technical, political, economic, etc.) which target 'real' people and places. In some forms of ethnographic writing, it may not matter whether I change particular characteristics like age, gender, or occupation of an interlocutor. In others, however, it is exactly these particular identity markers that position the speaker in ways that are relevant for the analysis. I am raising these examples not to object to what Sjaak

is arguing but to reiterate my previous point that anonymisation is rarely straightforward and that there cannot be a one-size-fits-all approach.

Like many anthropologists, I share Sjaak's scepticism towards formalised ethical review procedures, particularly in the form of a neoliberal 'ethics governance' (Pels et al. 2018) that is currently being imposed on researchers around the world. However, I do think that making it compulsory for researchers to reflect on ethical issues before commencing their fieldwork is, in principle, a good thing – and something that, to date, has not been common practice in German anthropology, at least not in any systematic way. I concur with Cordillera Castillo (2018: 406) who argues that

[t]he key is the ethical researcher, not the ethics governance regime. Thus, there should be greater attention to the cultivation of ethical consciousness and behavior among researchers through pedagogy and practice. The aim is to develop researchers' capacity to make ethical decisions and actions and make ethical thinking and acting a fundamental part of all stages of our research and engagements.

Maybe, if I had been encouraged at the beginning of my projects to elaborate how I intended to manage my research data, I would have thought more proactively about anonymisation and could have avoided some of the problems I faced in the writing process and afterwards.

Lastly, as Sjaak notes, it is indeed remarkable and surprising that – despite my anxiety surrounding issues of anonymising and confidentiality – I failed, every time, to discuss the matter with my interlocutors. It is surprising, but I do not think uncommon – which is why I decided to write about it so openly. Throughout my academic career, I have met anthropologists, not only students but also more senior scholars, who have struggled with anonymisation. In fact, only last week I was contacted by a researcher who asked my advice on whether she should use a pseudonym for an interlocutor (someone we had both interviewed) with whom she had lost contact and whom she had forgotten to consult whilst still in the field. My hope is that, by talking more openly about our own failures as ethnographers, we can help others avoid making the same mistakes.

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Inner Dialogues: Negotiations Unfolding between the Field and one's Desk

Eva Riedke

Most anthropologists can readily recall ethical conundrums during fieldwork that result in moments of 'intellectual discomfort' (Fassin 2008: 333). Emanating from the reflexive turn, significant scholarly attention has been paid to unpacking the heuristic value of such moments in the field (Alvesson and Sköldbäck 2000; Clifford and Marcus 1986; Gros, 2010; Holland 1999; van Maanen (2011) [1988]). It is through these experiences, so the argument, that anthropologists are encouraged to question what they work on, how they will write, and for whom (Abu-Lughod 1991, 157). However, a certain paradox persists: whilst significant attention has been paid to how discomforts during fieldwork are potentially productive, less attention has been paid to the processes of writing and the significance of that which appears emotionally, ethically, and analytically troubling for it. As I want to argue here, a certain image has been upheld whereby, once an anthropologist returns from the field, they will engage, over months if not years, with notes and transcripts, photos and newspaper clippings, will grapple with a series of complex questions around ethics and integrity, only to finally arrive at a decision over a suitable form of representation. What is only rarely addressed is that for only few researchers is this a linear process.¹ As the following account will render more concrete, ethical decisions – particularly regarding issues of responsibility and representation – are grounded in the very practice of writing. Decisions about how to write are frequently re-thought and often even questioned in their entirety. Here I want to engage specifically with the proposition that complex ethical concerns readily materialise during the writing process, whereby the dialogues that took place with actual people during the field research recede from view and a form of 'inner dialogue' takes over.

1 Renato Rosaldo is a prominent exception, pointing readers of 'Grief and a Headhunter's Rage' to the non-linearity of the writing process that lay behind his texts. Years after his wife's death, Renato Rosaldo was still grieving, still filled with rage, describing how he longed for the 'Ilongot solution'. As he recounts:

not until some fifteen months after Michelle's death was I again able to begin writing anthropology. Writing the initial version of 'Grief and a Headhunter's Rage' was in fact cathartic, though perhaps not in the way one would imagine. Rather than following after the completed composition, the catharsis occurred beforehand. When the initial version of this introduction was most acutely on my mind, during the month before actually beginning to write, I felt diffusely depressed and ill with a fever. (Rosaldo 2004 [1989]: 171)

I thank the anonymous reviewer for pointing this out.

By reflecting critically on some of the uncomfortable moments at one's desk where the text becomes a site of representational struggles, my aim is to prize open a bit more that black box within which ethnographic writing has traditionally operated.

The paper begins with an account of a conflict that unfolded during my fieldwork and how I became witness to a series of contract killings. Once 'back home' and beginning to write, I grappled with a number of difficult decisions of how to compose my ethnography, through and through defined by the further trajectory of conflict and violence that unfolded in the township. My ethical deliberations in the process of writing shifted on numerous occasions, in particular the manner in which I experienced the responsibility of 'witnessing through writing' (McGranahan 2020, 13; see also Behar 1996). Shifting back and forth between the field and one's desk, a series of ruptures and breaks plays out that, as I want to argue here, seldom allows for the easy definition of a fixed narrative arc that can be typed up in a straightforward manner. Building on previous arguments made in relation to discomforts in the field (England 1994; Fabian 1990; Fassin 2013; Lather 2001; Nagar and Ali 2003; Pillow 2003; Visweswaran 1994), I suggest, first, that that which is 'messy, contingent and full of tensions' (Childers 2011, 347), including a sense of being 'in trouble', might in itself be useful and provide new empirical insights – rendering intelligible seemingly incomprehensible discourses and interpretations (Fassin 2008) as well as exposing the grids of unequal power relations characterising the process of ethnographic inquiry (Jacobs-Huey 2002). Second, I suggest that the acknowledgement that one may never in fact reach 'a comfortable, transcendent end-point' in the process of ethical decision making, and that practices of reflexivity might not always bring about the form of 'clarity' (Pillow 2003: 193, 192) that is readily assumed, may in itself serve as a marker of ethically valid practice (see also Lester and Anders 2018; Visweswaran 1994). A critical engagement with one's inner dialogues at the desk – wherein one experiences one's own ethnographic self as through and through 'multiple, unknowable and shifting' (Pillow 2003: 180) and as 'contingent, plural and shifting' (Rooke 2010: 38) – may constitute a move away from the 'comfortable uses of reflexivity' and a step towards drawing out what Pillow (2003: 175) termed 'uncomfortable reflexive practices'. As Lather (2001: 201) underscored: where authors grapple with the failures of representation and their texts become the sites thereof, these 'are not so much about solving the crisis of representation as about troubling the very claims to represent'.

Fieldwork somewhere outside of Durban

I conducted fieldwork for my PhD in a township on the outskirts of Durban, South Africa, between October 2011 and March 2012 and again be-

tween October 2012 and April 2013. For reasons that become clear below, I anonymised where I had conducted the fieldwork. Empirically, I focused on processes of ‘issueification’ by different ‘publics’, exploring how people come to collectively perceive the consequences of indirect actions that affect them, develop and define a form of ‘affectedness’ and, in turn, call publics into existence (Dewey 2012 [1927]; Marres 2012). Instances in which processes of issueification were rendered acutely tangible in recent years were, for example, the struggles around the transformation of the symbolic landscape in post-apartheid cities – pertaining to the renaming of street names, the removal of statues, and the erection of new ones. Tracing the trajectories of a series of very different issues that have figured prominently in the post-apartheid context, where they are negotiated and how, my research rendered tangible *sites* and *forms* of political participation that may not readily be considered as belonging to the settings of ‘the political’ (Dewey 2012 [1927]; Marres 2012; Boltanski and Thévenot 2006).

During my second phase of fieldwork, a conflict erupted in the township around a local housing project. New, fully state-funded houses were to be built in one section of the township for a select number of families who lived in dilapidated houses. These houses dated back to the creation of the township and had drastically deteriorated since. The local councillor set up a list of all households that were to receive a new house. As work on the construction site began, charges were made that the councillor had accepted bribes to replace the names on the list. These new names were names of people who had previously not qualified and, in part, were not even residents of the township. In response, a number of angered residents, including such on the original housing list, joined Abahlali base Mjondolo – a prominent shack dwellers’ movement campaigning against evictions and for public housing – and began to protest against the corrupt allocation of the houses. In February 2013, a man living in one of the newly built houses was murdered. He was one of the beneficiaries alleged to have been allocated a house illegally. His was the first of six murders, all rumoured to have been carried out by paid hitmen.

From the first murder, I was caught up in what – drawing on Pieke (1995) – one can call ‘accidental frontline anthropology’. Prior to the emergence of the conflict, I had upheld close field relations with the councillor, with those residents who had joined Abahlali and who were now protesting against corruption, and with a series of other key actors who, with time, became embroiled in the conflict, including local heads of the dominant political party, officers at the local police station, and representatives of local NGOs. The conflict worked to progressively endanger my existing social relationships: seeing that I had extensive relationships with actors who were now on different ‘sides’ of the conflict, residents began to question who I really was and what I was doing there. Conflicting rumours emerged concerning my past work in the township that began to put my own safety at risk. I had

to adapt my fieldwork practices in a context that was marked by heightened scepticism, vexing uncertainty, and existential disorientation on the part of many of my interlocutors. I developed a series of pragmatic, improvised field strategies, in particular to re-establish trust (see Riedke 2015). I took fewer notes during meetings, and if I did so at all, deliberately made them unreadable in a concern that they might be read by non-intended readers. Where I had often recorded conversations previously, I now only did so in very rare instances. I sought always to make transparent to my interlocutors *who* I had spoken to in the previous days and made sure to tell close contacts where I went, in the hope to invoke a sense of responsibility for my safety.

The first murder marked the beginning of emotionally, ethically, and analytically troubling times. I continued to uphold field relationships and invested time in weekly meetings with those who were in this period considered to be the perpetrators of violence. I felt compelled by what was a strange hybrid of pragmatism – in particular to maintain access to the field – and an urge to uphold a sense of transparency. Whilst I continued to meet with individuals on both sides of the conflict, and there was a readiness to engage with me, I was also frequently made to recognise that neutrality ceased to be a legitimate option. At a local party meeting that I attended, the councillor, in isiZulu, told the audience that there was an *impimpi* (a spy/colluder) present. It was clear to me and a close friend of mine that he was referring to me. The Abahlali protesters, in turn, were concerned with how I could help them and when I would fully ‘join their struggle’. I was on occasion encouraged to accept ‘proofs’ that they had collected on the alleged corruption and fraud offences – including an original housing list to which they had gained access through an employee at the councillor’s office, conversations they had recorded on their cell phones, emails that had been sent, and photographs that circulated. In all encounters I resolutely maintained that I could not accept this material, for not only would it jeopardise my own safety and that of my assistant but also, potentially, theirs.

Back home at the desk

I returned from South Africa in April 2013, after which many months of analysing, discussing, workshopping, and presenting of my material followed. I spent much of this time grappling with how I should write the ethnography I was preparing for. I was confronted with the emotional and psychological fall-out from having known some of the individuals who had been killed and encountered a deep unease with the transition from what Alison Rooke (2010: 26) described as the move ‘from affective participant observation to a distanced writing up’. The distinction between ‘field’ and ‘home’, the trope of entry and exit – despite mostly being used playfully, parodically, and self-consciously – continues to function as an archetype of professional practice

for anthropologists today (Gupta and Ferguson 1997). It continues to be part of the 'fiction and normativity of traditional ethnography' (Rooke 2010, 30). Once 'back home', as Akhil Gupta and James Ferguson (1997: 12) put it in the late 1990s, writing is perceived as being done 'in the academy, in libraries or studies, surrounded by other texts, in the midst of theoretical conversation with others of one's kind', where one's interlocutors no longer talk or peer over one's shoulder.

I share Johannes Fabian's impression that discussions of ethnographic writing still show a curious lack of engagement with how the disjunction between fieldwork and writing comes to have an undeniable impact on the practical aspects of writing, thus 'on the nature of what we are doing when we write' – not just on modes of representation but on the *praxis* of writing (Fabian 1990: 762, 756). Largely because this image remains uninterrogated and these two forms of activity continue to be seen not only as somewhat distinct but also as sequential to each other, particular experiences and forms of knowledge – namely, those collected in 'the field' – are privileged whilst others are somewhat 'blocked off' (Gupta and Ferguson 1997: 15). The latter include those experiences that unfold in the very process of writing, at one's desk. Paradoxically, it is often at the desk that we become acutely self-aware of the tensions and contradictions in our individual research fields, in which we engage with our own shifting subjectivities and in which we are able to study our so-called 'postcolonial self itself as a site where multiple centres of power inscribe' (Trinh, cited in Pillow 2003: 189).

Furthermore, it follows that where reflexive practices engage with ethically troubling moments whilst writing, they are posited and accepted as a method to 'work through'. The dominant, validated reflexive practices that at present have currency amongst qualitative researchers are framed as a methodological tool to take the author beyond the uncomfortable realities of doing engaged research, as providing a release from one's discomfort with representation (Pillow 2003: 187). Further, also an 'acceptance that coming to know oneself will aid in knowing, understanding, "witnessing", the other'. Rarely, however, does it appear legitimate to engage in a far less 'comforting' form of reflexivity, namely 'a reflexivity that seeks to know while at the same time situates this knowing as tenuous' (Pillow 2003: 176, 188).

When I began to write, I was drawn to the position of the witness. Distancing myself from the position of neutrality that I had initially sought to uphold during fieldwork, I now felt compelled to place the conflict centre stage and to help give voice to the lives of those who I saw as its victims, the Abahlali protesters. It was in the process of writing that I now sought to take a side. Problematic about 'siding' but also about placing the conflict centre stage was, in turn, the fact that I had done research on both sides of the conflict and also with those now considered to be the perpetrators of violence. I struggled with the question whether it was ethically justifiable

and epistemologically sound to now produce an ethnographic account that explored the acts of violence they were accused of having committed. Whilst it is no doubt not uncommon for ethnographic research to be done with the perpetrators of violence, in many cases these anthropologists have however set out to deliberately study violence and engage with those who stand behind it. In my case, events that were not initially considered germane to my research became part of it and violent conflict became the object of ‘involuntary research’ (Lee 1995). With a strange sense that I was engaging in a form of betrayal, I decided to use pseudonyms for the people I wrote about, though not for the place, hoping to produce a critical analysis and representation that would nevertheless be accessible and meaningful.

About eighteen months after returning from the field, Thandisile, who had been the chairperson of the Abahlali branch in the township and someone I had spent considerable time with, was assassinated in her home. Five months later, in February 2015, the local ward councillor and a leading ANC figure in the township were arrested for her murder. Rumours circulated that the two men had also been behind some of the previous murders. A few weeks later I received the first WhatsApp messages from my interlocutors asking me to ‘join *their* struggle’.² They also asked whether I had any material – in particular recorded conversations with Thandisile, the councillor, police officers, heads of the political party – that could be used in the trial or be ‘made public’. I felt a strong ethical responsibility to do so, for only a few months ago I had engaged in extensive conversations with them and yearned with them for arrests to happen. But my response was reasoned, explaining that primarily due to safety concerns I would not be able or, indeed, willing to provide access to my diaries, field notes, or recordings. I felt caught in a paradox: I knew too much about what had occurred and, at the same time, too little about where my knowledge would put me and others (see also Lather 2001: 204). Aware of my informants’ struggle for self-representation and self-determination, the challenge for me was, as Kamala Visweswaran (1994, 80) formulated it, ‘how does one act knowing what one does’?

There was no way of knowing at that point what value the stack of material, scattered in different mediums and formats across my desk, could have for the trial. Nevertheless, I feared what a potential subpoena could bring about.

2 Alison Rooke’s (2010: 30) suggestion that ‘the field’ must be seen as having fluctuating boundaries, as expanding and contracting at different times, appears pertinent to describe instances of ‘crossing the boundaries’ and experiences of ‘being back in the field’ – for example through WhatsApp conversations or phone calls with interlocutors – and valuable to transcend the disjunction between fieldwork and writing, field and home, and their assumed temporalities. Kirsten Hastrup (1992: 127) made a similar observation: ‘the field world has neither a firm past nor a distinct future because its reality is intersubjectively constructed and depends on the ethnographer’s presence in the field’.

Unedited, my interviews, notes, and recordings, if simply handed over, would have placed many of my interlocutors in extremely vulnerable situations. Of this I could be certain. But there were also less clear-cut concerns. At the risk of overestimating my role, I imagined audience(s) wider than just the specialists of my discipline that were keen to read what I would write. These possible audiences had a real implication for how I wrote (Descola 1996; Fassin 2013). Faced by these appeals to 'join *their* struggle', I felt forced not just to do the opposite but also to rewrite much of what I had already written. It was at this point that I anonymised the place of my fieldwork to such an extent that it merely became 'a township somewhere close to Durban' (see Vorhölter in this issue for a valuable, related discussion). Further, I laid a theoretical work on top of the stories told, which brought about another significant level of abstraction.

By rewriting the text and anonymising people and place, I sought to preserve a fine-grained, critically realist account of the practices by which actors and groups sought to identify a certain reality and render it unacceptable (Boltanski 2011). Hereby, the reality of domination would still be rendered visible, even if the individual actors remained unnamed. Anonymised ethnographies, indeed, even fictionalised ethnographies, in essence stake their story on a more fundamental problem, on the more general workings at play. Providing accounts of the everyday lives of activists, the grammar of the arguments invoked by local politicians, or the involvement of a community police force in disputes worked to render tangible some of the raw details of politically motivated killings that have continued to plague the province since before the end of apartheid.³

In conversations of mutual reflection with my interlocutors, my legitimisations for how I felt I was able or compelled to write – thus in a highly anonymised form – were partly met with consent and empathy but also with disappointed expectation. Indeed, some expressed concern and disappointment. 'What are you critiquing?', one informant challenged me. 'My critique towards you is that this is not critique! People should know what I fought for!' In rewriting texts and chapters that I had already written, I sought to explore a way of writing that George Marcus (1994) termed a 'messy text'. Experimenting with different textual strategies and pursuing a more unconventional narration – including stand-alone vignettes or lengthy dialogues – I sought to give the reader glimpses of my own autoethnographic work; my often privileged position, by gender, race, class, and nationality; and my feel-

3 A prominent example of a fictionalised ethnography is Richard Rottenburg's (2009) *Far Fetched Facts: A Parable of Development Aid* in which he makes use of generic (ideal typical) consultants, financiers, implementing agents, and the like to tease out the central, underlying workings at play in the so-called 'game of development'.

ings about moving in and out of the text as an observer and an observed.⁴ These, in part, allowed me to reveal the power inequalities that continue to underscore ethnographic fieldwork but also to point to contradictions, ambiguities, and incoherencies informing discourses and practices.

I spent a lot of time at my desk ‘not writing’, and part of my trouble was the difficulty of figuring out whether this was because I felt a deep unwillingness to write or whether it was an inability to do so (Fabian 1990: 769). The unease and apprehension I felt during this period of writing had a diffuse quality. They were at once intimately personal and psychological, rooted in my emotional investment in personal relationships with my interlocutors, and entangled with real or conceivable institutional and political risks. The Abahlali protesters had asked me to ‘join *their* struggle’. They were convinced that my interviews and recorded conversations would be of great assistance in the trial and expected that my academic text(s) would disseminate ‘the views of the marginalised’ and speak to their struggle for self-representation. As I contemplated the commitment and responsibility towards my various subjects of research, I was unable to disentangle these from the legal concerns, physical stress, and academic pressure to write, publish, and perform.

The question posed by my interlocutors was, in essence, the following: ‘for whom do you write?’ Whereby, as Fassin (2013, 640) highlighted, the preposition ‘for’ in such instances does not refer so much to ‘the public dissemination of the work as to its moral obligation: towards whom should we feel obliged?’ He suggested further: ‘carrying on an ethnography is cumulating debts. Making it into an intellectual production is repaying them – at least in part’. The creditors are many, and one is indebted to multiple groups ‘in different ways and with an unequal weight’ (Fassin 2013: 640). When I put my work into words, I sought to remain loyal to more than one side; and yet an uncomfortable sense of different, irreconcilable loyalties persisted. In the final draft of my PhD, I made this transparent, aiming for what Patti Lather (2001: 215) has termed a simultaneously ‘both get[ting] in and out of the way’.

Ethical moments whilst writing

A key aspect that begins to emerge as one discusses the distinction between comfortable/uncomfortable reflexive practices and the ‘black boxing’ of what

4 Wanda Pillow (2003: 193) emphasises how ‘a desire for “honesty”’ often ‘dissolves into an up-front listing of the researcher’s situated identities – a naming and marking of the researcher self’. In my example this would be: Caucasian, of German descent and working-class background, heterosexual, and feminist. The ability to ‘disclose’ one’s own subjectivity in this manner rests on the assumption that one can be honest about oneself, particularly in relation to an Other. Feminist researchers have critiqued practices of (self)reflexivity that seek to demonstrate how the researcher truly knows themselves. Thereby feminists put into question where the researcher/author begins and ends in relation to the research and research subject.

we are doing when we write is a distinction between ethics as constituting a single moment at the outset of the research and ethics as made up of a series of ethical moments which arise throughout research and reach on into the writing up of the ethnography itself (Simpson 2011). Furthermore, it is seeing ethics not as a series of methodological decisions but as a material experience (Childers 2011). The emotional and intersubjective concerns I grappled with during the writing period point, to borrow from Marilys Guillemain and Lynn Gillam (2004), to the significance of ‘ethics in practice’ (vis-à-vis ‘procedural ethics’): ethics is understood as grounded in the day-to-day practice of research and, in contrast to procedural ethics, also in events that may not be anticipated when applying for approval. Ethics in practice has an ‘everyday’ sort of quality to it. A useful term may also be ‘micro ethics’, underlining not that only little is at stake in (quite to the contrary) but rather pointing to the manner in which ethics remain grounded in day-to-day practice and are experienced at times as troubling, discomfiting moments.

Although significant efforts have been made to effectively unravel more of the ethical tensions that are part of the everyday practice of doing research – particularly by critical and post-critical scholars – there is as yet little conceptual work that engages with the ‘ethically important moments’ (Guillemain and Gillam 2004: 262; Simpson 2011) whilst writing. My proposition is that ethically important moments, or indeed ethical dilemmas, do not end with a return from the field but carry forth and remain acute in the everyday doings at the desk. It is in the very process of writing that commitments to transparency, trustworthiness, and advocacy are complicated. Engagements continue to unfold with interlocutors (in my case through phone calls and WhatsApp conversations). At the same time, and less frequently acknowledged, is a tactical avoidance, or postponement, of ‘full and open dialogue’ to other occasions (Tedlock 1993: 370; see also Vorhölter in this issue).⁵ The result is not seldom a form of inner dialogue, revolving around what research subjects and certain publics might have said, that in essence simulates a discussion and debate with these parties.

It might appear discomfiting to acknowledge that such a form of dialogue unfolds when writing, sometimes initiated through actual communication with a ‘real Other’ (as in my case) but often without. ‘Dialogue, perceived vaguely as an alternative to isolating or domineering monologue’, emphasised Johannes Fabian (1990: 763), ‘has been *en vogue* more than once during this century’, so much so that forms of dialogue practiced in ethnographic research have received relatively little attention. Kevin Dwyer (1979) and Dennis Tedlock (1979, 1993) were two of the most prominent proponents of dialogue in anthropology. By opposing dialogical to monological, or analogi-

5 The possibility for such an avoidance or postponement is put under question by the new communication technologies and forms of interconnectedness (Pelckmans 2009).

cal, anthropology, they argued that dialogue is not only central in the early phase of knowledge production but also generates a specific discourse in its later phases, up until completion of the written text (Fabian 1990: 764). In turn, inner dialogues, as I suggest, can – in the sequence of ethnographic knowledge production – be located as unfolding prominently in that space between the field and one’s desk. Whilst, as Fabian (1990: 764) argued, critics of Dwyer and Tedlock inevitably ‘put their teeth into the most palpable part of what is really a complex epistemological argument’ and dismissed it too quickly as ‘well-intentioned but utopian’, it appears fruitful to pursue an exploration of the dialogical nature of ethnographic research, more specifically, as I posit here, to consider the significance of inner dialogues unfolding around ethical dilemmas and concerns, in part grounded in actual conversation and in part produced as we simulate conversations in our thoughts.

Concluding remarks

My decision of how to write did not develop linearly but was subject to shifts and, at different points, became radically decentred. Over time, I pursued different forms of anonymisation, all the while concerned that I needed to ensure the anonymity of my interlocutors whilst they still deserved acknowledgement. Anonymisation is far from a straightforward ethical practice. As Niamh Moore (2012: 332) has noted, ‘for much of history anonymity did not protect the vulnerable’ but rather created vulnerability ‘by rendering people nameless’. It follows that the ‘assumption of the universal/ist ethical good of anonymity’ (Moore 2012: 331) is not readily tenable. In my case, through the decisions I felt compelled to make, anonymisation was accompanied by a process of abstraction, with my ethnographic material losing much of its situatedness. Though I experimented with different writing styles, the anonymisation nevertheless worked to separate data from place and (often) people.

Ethnographic writing has traditionally operated in something of a black box. Whilst a lot of attention has been paid to unpacking the black box in relation to ethnographic methods and fieldwork, particularly regarding ethics and integrity, less has been paid to ethnographic writing, even though the two are intricately related. Decisions over ethnographic representations continue to be treated as decisions that unfold linearly, as if a reflexive stance will result in a form of ‘comfortable, transcendent end-point’, to re-use Pillow’s phrase. As feminist postcritical theorists have argued, it can be productive to set up ‘disappointment as a rubric’ (Lather 2012: 47) and, relevant to my concern here, to acknowledge the doubt and failure that shape how we try to tell other people’s stories. As Sara Childers (2011: 247, 353) put it forcefully: ‘foregrounding, rather than attempting to reconcile, that which is messy, contingent, and full of tensions’, the dynamic interruptions and ‘trouble’ cre-

ated might 'help to generate a different practice and product of research'. It is in foregrounding the ruptures and breaks evidenced in our research that not merely negative cases are identified but that new empirical material emerges. The result envisioned is a somewhat less tidy form of writing, but one that provides more transparency about the difficulties of turning messy, uncomfortable experiences into clean and comfortable scholarship (Tamas 2009: 18). Whilst ethnographers frequently think from positions that underline the heuristic value of discomforts, they do not always allow these discomforts to dictate their practice to the same degree.

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Factual Conflicts and the Hegemony of Interpretation: Four Narratives and the Anthropologist's Version

Lena Kroeker

Introduction

Often anthropological projects conclude their reports to the funding agencies by saying that the available time had been too short and the resources too little for a topic so complex and that more research was required. The publications that are prepared on the basis of the data collected, however, present only conclusive interpretations: inconclusive, puzzling findings remain unmentioned. Publications hardly ever tell about the matters that are left open and just did not make sense to the ethnographer. The story of MaMeli, one of my key informants, is one such inconclusive case. When writing my PhD thesis, I found myself with two options: either to silence her case completely or to use it as frame for my methodology chapter, where I could address the inconsistencies it threw up (Kroeker 2015: 49–51). I considered it more honest to account for the gaps and contradictions in my data and to identify how, in some parts, my analysis may be speculative rather than interpretative, and thus opted for the latter. I did so fully aware that ethnographic texts, in general, only suggest one amongst many possible interpretations. In this paper, I reflect on my experience with dealing with MaMeli's case: how I moved from my field notes, which documented my interactions with MaMeli, her mother-in-law, a paediatrician, and a midwife, to an interpretation where I extracted details and placed the four narrations in a cultural context. I examine how my version was at best an approximate reconstruction of the situation. I draw on Reyna (2019) who argues that in anthropology there are but 'approximate truths', each of which has to be based on an explanation. He underscores that in the end it is the anthropologist who judges certain explanations to be more reliable than alternative ones.

Between 2007 and 2009, I conducted ten months of ethnographic research at Lesotho's best HIV treatment site, the Mafeteng Government Hospital, located in Mafeteng, a town about eighty kilometres south of Lesotho's capital Maseru. I had set out to study how young women integrate the complex HIV programme to prevent passing on the virus to their babies. Lesotho was one of the countries suffering heavily under HIV/AIDS. At the time its HIV rate stood at 25% of those in the reproductive age range and the rate amongst pregnant women was even higher, at around 28% (ICAP 2009). HIV can be transmitted from mother to child in utero, during delivery, or through breastfeeding. Compliance with medical protocols allows significant reduc-

tion in HIV transmission from mother to child and promises a healthy life. Still, I had prepared myself for having to deal with cases of maternal and infant death.

During the first phase of research, I accompanied medical staff at the hospital, a facility that provides an excellent infrastructure for women during pregnancy and labour. I was able to observe and participate in the practices of antenatal care and HIV counselling. In the second phase, I identified thirty pregnant HIV-positive women who allowed me to accompany them to their antenatal appointments, visit them at home, meet their relatives, and engage with them in interviews, informal conversations, and through participant observation. One of the women I met was MaMeli, 19 years old and pregnant for the first time. MaMeli had completed Form B (Grade 11) and, for the last term of her pregnancy, was staying with her mother in a better-off neighbourhood of urban Mafeteng. During visits, I could feel the tensions between her mother, her mother-in-law, and MaMeli. Her mother would have preferred MaMeli to continue her schooling instead of getting pregnant from a much older man, a mine worker and the son of a neighbour. Because her mother did not agree to the liaison, MaMeli seemed to struggle more with her social situation rather than her medical condition – at least that was my perception. Medically, MaMeli's pregnancy was without complications and she was an understanding and informed client in antenatal counselling sessions. However, I began to perceive problems once the baby was delivered and passed away a day later. In an attempt to reconstruct the hours between the baby's birth and its death, I interviewed the young mother, her mother-in-law, the midwife, and the paediatrician who attended to the baby in its severe condition, but their stories differed so significantly that it was just impossible to discern with any conclusiveness what had happened to the baby and the reason for its death. Emotionally, I felt with the family, mourning the loss of a child; academically, I was puzzled by their narratives. MaMeli's case was one of those in my ethnographic sample that raised a whole set of methodological and analytical challenges as well as possibilities. The deliberations around the baby's death revealed etiquettes, tensions, and social roles that allowed important insights in the social drama.

The paper opens with a presentation of the four varying narratives of the birth and death of MaMeli's baby. The rest of the paper then examines how I wrote my ethnographic account of the event for an academic and ethnographically-trained audience. I do so in four steps: I discuss why informants tell contradicting versions of the same story, delve into the theory of storytelling, briefly examine the issue of lying, and examine the hegemony of interpretation. I argue that ethnographic writing is not necessarily about finding the truth but about finding plausible explanations for not finding the truth.

Four stories with factual conflicts

It was after Christmas when my research assistant MaKhotso and I checked the delivery record at Mafeteng Government Hospital's maternity ward. Usually we passed by the maternity ward and the post-natal room every morning to see whether any of the thirty women participating in my study had arrived to deliver their babies. Because of the Christmas break, we had not been in for a couple of days. Noting from the delivery record that MaMeli had delivered a healthy boy and already left the hospital a few days ago, we decided to pay her a visit at home.

In Lesotho, first-time mothers and their babies normally stay at the mother's maternal home, not with their in-laws at the marital homestead.¹ We were surprised, however, to find MaMeli at her in-law's place. When we entered, she was sitting on the floor dressed with a white headscarf, a garment marking that she was in mourning. We greeted MaMeli but were unable to talk to her in private because her mother-in-law came into the room with us. The mother-in-law told us that the baby had passed away the day after the delivery. In the presence of MaMeli, the elderly woman harshly described what had happened. I summarise her account from my field notes that I made on the same day²:

MaMeli's baby passed away the day after she gave birth. After the delivery, the baby dropped out of the midwife's hands whilst a nurse was sewing the episiotomy. This fractured the head of the child, the mother-in-law claimed. The staff did not treat MaMeli, the baby, and the mother-in-law nicely at all. The nurse spoke in a rather rude way to her. She and MaMeli even overheard the nurses complaining that the in-laws were visiting too often and interrupted them in their duties.³ When the midwife discharged MaMeli and the baby from the hospital, MaMeli was told to come back for a check-up in two days.

- 1 The movement between households is linked to the rites of passage a woman undergoes with marriage. First-time mothers in particular go to their maternal homestead during the last trimester of pregnancy until three to four months after confinement. In this liminal phase, they are fully catered for by their own kin (Ashton 1952: 30; Kroeker 2015: 106–107). The movement between households sometimes interrupts the HIV prevention programmes the mothers are following.
- 2 See also Kroeker (2015: 49–51). I also carefully noted the mother-in-law's non-verbal expressions and any other observations I made during the event, as suggested by Girtler (2001: 141–143). Immediately after the meeting, I sat down with MaKhotso to recapitulate and complete the notes.
- 3 Relatives are not tolerated in the delivery room and the nurses and midwives strictly send them to wait outside the ward. Relatives are, however, allowed in the postnatal room. I doubt that the mother-in-law had been present during the delivery as she makes us believe.

But, the mother-in-law asked, turning to MaKhotso and me, was it usual for a baby to be brought in for a check-up after two days already? When I answered, 'No, it is usually after one week',⁴ she exclaimed that this proved that the nurse knew something was wrong with the baby but kept them in the dark. But MaMeli took the baby to the hospital again the very next day. The baby was bleeding from the nose. And the mother-in-law claimed that this must have been caused by its deformed head. The doctor examined the baby, wrote something in a book, tore out the page, and sent it to maternity ward, the mother-in-law recalled. He also immediately sent a call to the midwife who had delivered MaMeli's baby and who was again on duty. When the doctor asked the midwife about what had happened to the child, she did not say anything and just kept quiet.

The mother-in-law strongly expressed her dissatisfaction with the nurse and claimed the staff behaved in a careless and rude manner in front of mother, child, and mother-in-law. Based on that experience, she argued, MaMeli should rather deliver her next baby at home.⁵ Shocked to hear about the baby's death and the bad service MaMeli had received at the delivery ward, MaKhotso and I promised to investigate the case. Thus, on the following day, when the paediatrician who had attended to MaMeli's baby was on duty, I approached him to ask whether and what he remembered about MaMeli's case. He recalled having attended to her and her baby when MaMeli brought baby to the hospital one day after being discharged. The paediatrician gave me this account:

MaMeli arrived after 7 p.m. when he was on call for the night shift. The mother-in-law dropped in later and immediately started to talk angrily. She blamed the midwife for having dropped the child, arguing that this had resulted in the baby's malformed head. She claimed the bleeding was a consequence of a head fracture for which the midwife was responsible.

When he checked the baby, he noticed that the baby was bleeding from the nose, but it was not severe. He asked when the baby's condition started, but the mother-in-law was unable to answer. He figured that the mother-in-law was not with the child during the night and asked her, 'Who is the mother of this baby?' upon which the mother-in-law pointed to MaMeli. The doctor

4 I was here referring to the check-up one week after birth as stipulated by the 2009 medical protocol for the prevention of mother-to-child transmission of HIV.

5 From a medical perspective, home deliveries are not advisable for HIV-positive women.

interrogated MaMeli and she explained that the baby had been crying all day starting from 8 a.m. He told them that they had come too late. The baby had a neonatal sepsis, which he could have treated if they had come earlier. He added that traditional people often apply Vaseline or cow dung to the umbilical cord, which causes infections of the blood cycle.⁶

I then looked for the midwife who had delivered the baby, and she told me:

The doctor [paediatrician] only called her when the baby had already passed away. She remembers the baby's fontanelle had not been closed, but a malformed head is not unusual with newborns. Because the Apgar score⁷ had been low, they had kept MaMeli and the baby in hospital for two days before sending them home. When the paediatrician called her [when MaMeli brought the baby back to hospital], she was surprised to find out that this baby, whom she remembered as having been big and healthy upon discharge, had passed away. The midwife suspected that the baby could have died of an undetected heart or lung problem.

With that information, I asked MaKhotso to accompany me back to MaMeli. This time, we met her alone. MaMeli, in the absence of her mother-in-law, gave us a slightly different version of events:

MaMeli already realised in the postnatal room that her baby was unwell. When I asked her what symptoms led her to think this, she mentioned the deformed head, which her mother-in-law had interpreted as caused by a fracture. MaMeli stated, however, that the nurse had told her that 'the baby won't have a problem with the head'. She also noted that the baby was crying a lot whilst all other babies in the postnatal room were quiet. The other mothers in the postnatal room told her that she must have lived in a noisy place during her pregnancy to have a baby that was crying so much. MaMeli recalled thinking that the baby may calm down at home, but the crying got even worse.

When they were discharged, MaMeli spent the first night at her mother's rural home some forty kilometres away from hospital. The baby cried all night long and had hot flushes, like a high fever. It was also having trouble breathing. She called her sister-in-law to accompany her to the hospital in the evening. The

6 I have been unable to find any further indication of such traditional practices in the literature or on the ground. The paediatrician was not of local origin and did not have children himself.

7 The Apgar score is a method to quickly appraise the health of a newborn baby.

mother-in-law joined them at the hospital. By this point the baby had a severe nosebleed and, when they undressed the baby upon arrival at the maternity ward, they realised that the insides of its clothes were all bloody. Mother and child were then referred to casualty, where they were attended to by the paediatrician.

The four perspectives show some overlaps but also contain some diverging interpretations that left me puzzled. Whilst the baseline of the story became clear, the versions did not seem to fit together and there was little chance for me to get the full picture. Who was present when? What time was the baby delivered, when were mother and child discharged, and when did they return to the hospital? When did baby die and was its death caused by a fractured head, a neonatal sepsis, or some cardiovascular problem? Why did MaMeli let her mother-in-law tell the story to the paediatrician and to us, when the mother-in-law was not even present during delivery or at discharge, or when MaMeli went back to hospital with the baby? Who rewrote, intentionally or unintentionally, the story and why?

I tried to reconstruct the scene and looked for any information that would allow me to consolidate the versions. I learnt through participant observation that the paediatrician habitually blamed ‘those traditional people’ and was easily annoyed by relatives of patients who ‘bothered’ medical staff. I also found out that MaMeli had been all by herself the night after the delivery. Indeed, I established that MaMeli had no experienced women around her who could have assisted her in the first hours with her baby and who could have noted much more quickly that something was severely wrong: her mother was unable to be there because she was working night shift at a textile factory; and her mother-in-law had only the previous day returned from a six-month training period as a traditional healer. The hospital delivery record was incomplete yet attested that no abnormalities were detected during or after childbirth. The delivery had been without complications and the baby’s Apgar-score high (contrary to what the midwife had indicated). Even MaMeli’s medical booklet and the baby’s health record gave no indication that mother or child were advised to stay in hospital for medical treatment or observation. And, in contrast to the mother-in-law’s claim, the booklet and health record both indicated that mother and child should come back after seven days for the baby’s first check-up. There was no record at all that a visit of the clinic was recommended two days after discharge. From the hospital practices I learnt that relatives are in fact not allowed in the delivery room, and yet the mother-in-law claimed to know that the midwife had dropped the baby in this room shortly after birth whilst a nurse was sewing the episiotomy. And despite knowing the situation best, as the only person who had been present at all times, MaMeli did not make any effort to correct inconsistencies in the storytelling; nor did the midwife add any clarifying

information. In fact, I had the suspicion that the midwife was confusing the case with another one altogether.

Generally I took the approach during fieldwork to believe the key informant's version, for the sake of our further collaboration. In MaMeli's case, however, I gave up on finding reliable information and concluded that the details I gathered were selective and reductive. This threw a shadow over the reliability of the information I had gathered earlier about MaMeli. Speaking to her relatives revealed that she had indeed lied to me about her marital status. MaMeli had told me she was married whilst the wedding had only taken place upon the mother-in-law's return to town. I concluded that she was likely an unreliable informant. At this point I had to decide that MaMeli's case contained little information about the HIV prevention programme I was examining and so completed my research without seeing MaMeli again. Yet, when I now engaged with my other informants, I began to wonder about the truthfulness of the information they gave me. This reflection on methodological limitations led to me dedicating a complete chapter of my dissertation to the topic. I took MaMeli to showcase that there is always uncertainty in research and there can be situations where it is impossible to believe a story one did not witness oneself. How can one tell facts from fiction?⁸ Is it my job as anthropologist to do so? Would I turn fiction into claims of fact for the insight of my readership, to make a story compelling and real (Fine 1993: 277)? Would it not be more plausible for my ethnographic writing to state that informants at times err, lie, or manipulate? Do I have the authority to tell what version is wrong? And what is my role in writing that story down?

Writing an ethnographic version of MaMeli's story

The bottom line is that I was not able to reconstruct exactly how and why MaMeli's baby had died, but the case was nevertheless an eye opener for me as ethnographer. I now turn to discuss how inconclusive stories such as this one have informed my ethnographic writing and how and why it is possible to produce ethnographic knowledge from unreliable data. My knowledge production follows the postmodernist notion that there is no factual and correct truth but a multiplicity of equally valuable interpretations. Such interpretations relate closely to the narrator's knowledge embedded in a respective 'frame of reference'. In an analysis of global and local knowledge systems, Loimeier and his colleagues (2005: 12) defined a frame of reference as

8 I follow Hastrup (2004) in understanding 'facts' as information that is organised as undeniable, general knowledge. Whilst facts are generally agreed upon, there may be different interpretations on whether a certain fact answers a particular question. Only in the service of a claim or a question do facts become significant.

the standards of evaluation and orientation which can be applied by actors in a specific situation and which make fundamental statements about the actual and ideal nature of the world. Reality is interpreted with the help of and within central frames of reference.

Barth (2002: 3) identified three aspects of knowledge. Like Loimeier and his colleagues, he rejects that knowledge is an object or that there is such a thing as objective knowledge; rather people host a ‘corpus of substantive assertions’, based on experience, embodied knowledge, and inferences. Second, knowledge is instantiated as words, symbols, or performative actions. Third, knowledge is dissemination in social interaction, which means that knowledge production takes place during an exchange. In MaMeli’s case, knowledge production happens at the various meetings, with MaMeli, her mother-in-law, the midwife, and the paediatrician staging their performances. Yet knowledge production takes place not only in the field but also in the engagement of me, as ethnographer, with you, my readers. Hastrup (2004: 465) rightly notes that ‘the anthropologist in the field engages the world as a “double agent”, being both a trained researcher and a character in the local drama’. Knowledge is produced in relation to an audience, which in my case is an imagined, invisible community of readers. The anthropologist has the authority to select and arrange the information and gather it as a text, for the end of providing a meaningful and convincing statement. Given this selection and interpretation of data, it is not the empirical material that counts but rather the art of making the interpretation convince a (present or imagined) audience. It needs to be noted that scientific knowledge often claims to be the dominant frame of reference, and yet, as Neubert and Macamo (2005: 246) state, ‘scientific knowledge only represents one of several possible frames of reference and is therefore the result of interactions, and is historically specific, culture-bound, and not value-neutral’ (my translation). Following this premise of the sociology of knowledge, I now offer my own writing of MaMeli’s case from an anthropological frame of reference. Whilst one could argue that, by presenting my version, I intentionally or unintentionally claim that my version trumps all others, I rather consider my version as entering into a dispute over the ‘hegemony of interpretation’ (Neubert and Macamo 2005: 254) with the four narratives presented above. I present my version in three steps, first by looking at why stories matter, then by examining biographic illusions and, lastly, by analysing the link between power and authoritative knowledge.

The narrators err but their stories matter

In my version of the story, the four narrators embellish some parts of the story and conceal others. They interpret what they know about what hap-

pened in the few hours between the birth and death of the baby in line with their frame of reference and their audience. Their versions are marked by significant differences, suggesting intentional or unintentional acts of erring, manipulation, or lying. Often ethnographers will sift out indications of such glaring inconsistencies, something I first intended to do. But van der Geest offers an explanation of lying that allows a deeper understanding: he argues that lying is a way of keeping face. He takes an obvious lie as indicator that he has touched a sensitive issue, with the false information pointing to the relevance of the hidden information (van der Geest 2018; see also Salamone 1977). Van der Geest argues that hiding information in a public conversation shows demeanour and respect, and helps people defend each other's respectability. Based on Erving Goffman's concept of 'face', van der Geest shows that lying is sometimes the most respectful and tactful way of evading having to give difficult answers. Passin (1942: 235) also sees possibilities for using field notes that contain obvious lies: 'it is possible to use lies very profitably as field-data, in some cases even more significantly than truthful statements'. Salamone draws out that 'lying is a form of communication, not its negation' and that anthropological investigations 'can lead to the discovery of cultural values, dynamic aspects of social organisation and the informal structure of networks' (Salamone 1977: 120, 117). And despite the methodological problems that lies pose in the field, McGranahan (2017: 247) sees the following potential:

Witnessing [lies] is to see and experience from the inside of a community, to gain an experiential sense of its logics and rhythms, and to be able to mark and explain how truths and fears and lies combine to eliminate certain histories in favour of felt or desired beliefs.

The authors argue that whilst checking information might prove that an informant has lied, it does not answer the question as to why the person has lied and in what forms of social relation the person is embedded. Revisiting my field notes I found that keeping 'face' was crucial for young urban women in Lesotho, particularly in their interactions with men, elders, and in other hierarchical social relations like with medical staff. Many young women intended to show their compliance with behavioural protocols and tried to avoid open disputes; they would rather lie, manipulate the expected outcome of a process, or simply abscond.⁹ If an informant was trapped between candour and seamliness, seamliness was often favoured, argued Goffmann (1963: 75), and my observations support his statement.

To gain some understanding as to whether MaMeli, her mother-in-law, the paediatrician, and the midwife lied intentionally (also see Luncă, this

9 I elaborate on these modes of dealing with conflict in asymmetrical social relations in Kroeker (2014).

issue), we need to place MaMeli's case into the social context of premarital relationships in Lesotho. Earlier studies show that Basotho people generally discourage premarital sexual relationships and rather encourage early marriage (Maqutu 2005: 36), although secret premarital relationships must be considered the norm rather than the exception. This is even more so the case in times of HIV/AIDS. However, if a relationship became known, parents (and specifically parents of girls) were blamed for the lax moral upbringing of their offspring or their lack of supervision. Parents, therefore, tried to avoid this stigma from their neighbours. In cases where a relationship was kept out of the public eye and did not have any consequences such as a pregnancy, many parents would turn a blind eye (see also Bochow 2007). But if a pregnancy became visible, the parents of the girl would certainly demand to know who had impregnated their daughter. To avoid a loss of face if the daughter were labelled loose or spoilt, parents often felt compelled to marry off their daughters to the father of the child (Kroeker 2015: 92; Maqutu 2005: 152, 155): a child born out of wedlock would serve as a constant public reminder that 'something is wrong with the mother of this child' (Ashton 1952: 33), and the bad name given to the child would serve as a continuous lesson to the mother (Kroeker 2015: 87).

MaMeli had told me, in light of her parents' reluctance to accept her relationship with their neighbour's son, she had planned her pregnancy in order to speed up the process of marriage negotiations. Thus, expecting the wedding to ensue in due course and in light of the moral stigma accorded to unmarried mothers, MaMeli already began to call the father of her unborn child her husband. She considered herself already 'as if' married and thus avoided scrutiny from medical counsellors and relatives about her premarital sex life. To me, it looked like a lie meant to conceal that the baby was born out of wedlock. But when I compared my field notes on MaMeli with those on other young women, I realised that the avoidance of disputes and the attempt to solve conflict non-verbally was a strategy they all tended to use. For instance, out of respect MaMeli avoided a discussion of her HIV status and used a strategy of indirect disclosure instead. It would have been a dishonour to explain to her mother or her mother-in-law her amorous relation, premarital sex, and how this led to her infection with HIV, about which she had learnt in antenatal care. Instead, as MaMeli told me, she left her medical booklet lying around, expecting that her mother would look at it and thus find out about her medical condition 'by surprise'. Like this she disclosed her HIV status without having to talk about it.

Taking such social constellations into consideration, I argue that MaMeli gave false information in order to keep face. But not only did MaMeli manipulate her story. It seems that all narrators tried in one way or another to keep face and to make their story more meaningful. I explore one form in which this was done in greater detail in the next section.

The biographic illusion

Besides the desire of keeping ‘face’, the narrators were driven to tell their specific stories due to a ‘biographic illusion’. This term refers to a sociological debate that discusses whether a ‘good story’ is necessarily a ‘true story’ (Apitzsch and Jansen 2003: 195–110). When analysing situations and information, we need to remain aware that experiences are not told the same way they took place: narrations rather depend on the narrator’s current state of mind, the situation in which they are told, and the way in which memories develop over time. As memory fades, the narrator fills in gaps, and as recipients express comprehension (or incomprehension) of a story, the narrator adjusts. Interactions, therefore, already imply an analysis of the past in the way the stories are narrated. In general, a narration draws on two aspects: the situation as experienced (*erzählte Zeit*) and the present situation (*Erzählzeit*), the perspective through which the past is interpreted (Lucius-Hoene and Deppermann 2002: 24–29). As Apitzsch and Jansen (2003) show, autobiographic narrations may not necessarily match an objective description. A narrator may, with or without the intention of faking a story, consider other parts and explanations more important than the listener. Occurrences that would be of importance for explaining the self of that past time might not be considered relevant when narrating what had happened in the present. Information might have faded or might not be worth telling due to different assumptions of the topic under study, by the informant and by me, the recipient. Ochs and Capps (2001: 45) warn that researchers need to bear in mind that ‘narratives of personal experience do not present objective, comprehensive accounts of events but rather perspectives on events. [. . . They must be considered] as selections rather than as reflections of reality’. Details will be generalised, selected, or completely neglected if they did not seem valuable for the core of the story. Hence, a narrative may become an illusion that is enshrined in the telling of the occurrence rather than the memory of the occurrence itself. Besides the inability to memorise and narrate occurrences accurately, a narration conveys a message to the audience and aims to ‘construct an over-arching storyline’ that embellishes the narrator’s presentation of the self (Ochs and Capps 2001: 4).¹⁰

When we apply this to MaMeli’s case, we see that MaMeli and the other speakers reinterpreted the events and, in the narratives presented to MaKhotso and me, each focused on blaming another, absent person. Whilst the mother-in-law placed the emphasis on the midwife’s bad medical and social skills, the doctor blamed the mother’s ignorance and ‘those traditional peo-

10 Ochs and Capps (2001: 4) note: ‘All narratives exhibit tension between the desire to construct an over-arching storyline that ties events together in a seamless explanatory framework and the desire to capture the complexities of the events experienced, including haphazard details, uncertainties, and conflicting sensibilities among protagonists’.

ple'. The midwife found the reasons for the baby's sudden death in the medical realm, but one unrelated to her expertise and practice. MaMeli indirectly blamed the other mothers in the postnatal room for accusing her of having stayed in noisy places during her pregnancy and for advising her to go home and calm down the baby. The narratives already entailed foreshadowing and back shadowing which hinted at what the narrators aimed to convey (Ochs and Capps 2001: 5). With this technique the narrators already forecast their point of blaming someone else and justified their own actions.

In summary, the performative act of telling a story entails information about the social constellation of narrator and listeners that is crucial for an anthropological analysis. However, the actual happenings move to the background, as they can hardly be reconstructed retrospectively.

Power and authoritative knowledge

One line from an ethnography of childbirth struck me and made me once more revisit MaMeli's case in the process of writing my thesis. The line read: 'the power of authoritative knowledge is not that it is correct, but that it counts' (Jordan 1997: 56). This raises two questions: how did the narrators in MaMeli's case make their stories count? What authority could they draw on to make their versions gain value?

MaMeli, the only person who had attended to her baby in its first and last hours, kept silent (and 'saved face', for that matter) whilst her mother-in-law talked to the doctor and, again, when telling the story to MaKhotso and me. Obviously MaMeli had her reasons: she had the knowledge to answer the questions, but she lacked authority to make her version be heard. Revisiting my field notes and interview transcripts opened my eyes and broadened my perspective on the rest of my data.

Within the biomedical frame of reference, patients speak frankly and openly about their complaints. From the perspective of health policy makers, patients are democratic and self-determined individuals who are able to make informed decisions over their bodies and do not need to consult family members for this. This position explains why the paediatrician wanted to know who the mother of the baby was and gave MaMeli the opportunity to tell her version of the happenings. From the perspective of the patients, the situation looks quite different. The medicalisation of health gives privileges over the patient's body to experts and their machines and thus disenfranchises the patient. Biomedical staff thus gain strength and decisive power over clients with the help of scientific 'evidence-based' operations. Women and their reproductive health are a particularly heavily medicalised field (Jordan 1997; Davis-Floyd and Sargent 1997). Medical technology 'exalts practitioner over patient in a status hierarchy that attributes authoritative knowledge only to those who know how to manipulate the technology and decode the informa-

tion it provides' (Davis-Floyd and Sargent 1997: 8). The relation between staff and patient in biomedical facilities is based on professional distance and authority (Jordan 1997: 70). This dual perspective on hierarchies explains why MaMeli was unable to speak openly to the doctor; neither could the midwife speak openly in the presence of her patient when the doctor called her in. Otherwise the midwife would have lost face.

In addition, childbirth is a contested field. Power relations within the medical realm are no less restrictive than systems of kinship and familial interdependence. Anthropologist and midwife Brigitte Jordan argues in her book *Birth in Four Cultures* (1993) that familial systems claim hierarchies of age, respect, and decision-making power over young and inexperienced mothers. Mothers-in-law in many cases take care of a newborn and thus make decisions in the mother's stead as part of their care obligations for mother and child. Elder women have a profound effect on decision-making during pregnancy, delivery, new motherhood, and childcare, which means that new mothers need to accept subordination to and advice from their female elders (such as mothers or mothers-in-law) for the sake of the well-being of the family. Jordan argues that certain aspects in the discourse on the 'right' behaviour in pregnancy, delivery, and childcare gain value, which qualifies a statement as decisive and authoritative, even if the young mother may have alternative opinions or feelings. The expecting or new mother needs to balance conflicting opinions against each other in relation to the speaker:

The central observation is that for any particular domain several knowledge systems exist, some of which, by consensus, come to carry more weight than others, either because they explain the state of the world better for the purposes at hand (efficacy) or because they are associated with a stronger power base (structural superiority), and usually both. (Jordan 1997: 56)

Sich (1983: 21–40) argues that even in situations where a young mother might have contrasting information at hand, she is likely to comply with the recommended behaviour of authorities, despite better knowledge. She argues that in this way the young person lets the elder keep honour and save face. Scott (1990: 82–83) explains that such gerontocratic orders are very stable since young people expect in time to achieve a similar powerful position for themselves. Expecting in future to become a respected parent allows young women to accept a subordinated role and compliance with authoritative orders in the present. Gerontocratic dominance and subordination seem, thus, a matter of being and becoming. Thus, in generational debates youngsters comply with directives for the sake of showing respect and honour to their elders. Such respect for elders is an integral part of many African societies and younger people avoid openly questioning the advice of elders as this would be seen as disrespectful and offensive. This explains why MaMeli would not

speak up in front of her mother-in-law and the doctor and why she allowed her mother-in-law to tell her misstatements.

Despite this authoritative setting, I found the young mothers to be actors in their own right. Eager to avoid conflict, they found creative ways to solve problems (see also Kroeker 2014), for instance by involving another person. Such a person would hold legitimacy through access to one or more of the following resources: money, social status, the control over supernatural forces (Eckert 2004: 20), authority of age (as in the case of Lesotho), or the backing from a domineering medical system. Goffman (1963: 28, 31) calls such an ambassador 'the wise' and uses this term to refer to a person who knows about a stigmatised identity, but one in the face of whom the individual affected does not feel shame or the need to hide the stigma. Involving an ambassador is a usual strategy of conflict resolution in Basotho culture, one which is institutionalised and non-violent. It is likely that MaMeli put her mother-in-law in this position to explain to the doctor what had happened to her baby. This would explain why the mother-in-law, who had not witnessed the events, dared to enter into a discussion with the doctor and the midwife on behalf of MaMeli. Her authoritative position made it legitimate for her to act as ambassador.

This brings me to my own role in the set-up of my study: as study participants gained trust and confidence in MaKhotso and me, they began to ask us to step in as ambassadors to assist them. They expected us to be knowledgeable as well as convincing. We had observed their family situations and knew a great deal about the relationships and conflicts present. Often we already knew some of the household members through our home visits and interviews. The informants had taken our conversations as a chance to air unresolved life events, particularly generational and marital conflicts (see also Ochs and Capps 2001: 7). Indeed, some even approached me directly to take on the role of ambassador, as I had researched their stories and they felt I could therefore speak to third parties on their behalf. At first I did not consider this role as an opportunity for participant observation; actually, I did not like to become an ambassador and avoided this role as far as possible. Taking on such a role in a situation of conflict seemed to me to be a form of undue interference in the lives of my research participants and thus a hindrance to my study. It was only when I analysed my field notes that I started to appreciate the value the position holds. As a social function, the role of ambassador between generations incorporated me into the local structures of conflict management; it also allocated to me a set of rights and duties. It was when I took on the role despite my earlier hesitation that I began to recognise the social positions and hierarchies that were underlying the social relationships. It is thus in hindsight that my involvement at the request of informants can be characterised as active participant observation (Girtler 2001: 63). In MaMeli's case, however, I had missed that opportunity.

It was with the literature on face saving, biographic illusion and the power of authoritative knowledge that I finally sat down to write. Some parts of MaMeli's story began to make sense when read through the lens of this literature, though others remained unclear. Going back and forth between the literature and the field notes helped me cross-check and identify certain gaps, though some might also have been the result of imprecise note taking on my part. I was unable to fill the gaps of knowledge fully, though I noted that these gaps become more visible and defined: what conclusions can I draw? What knowledge did I produce? What is it that I do not know?

In the fifth version of the story, my own, MaMeli is troubled by conflicting demands. An analysis of the narrations indicated that her social role was one of a minor towards her mother-in-law and within the medical realm. I assume that MaMeli was neither in a position to confront the paediatrician nor her mother-in-law and that her inability was not only due to the trauma she had just experienced but lay in the social structures of power relations. These power relations are representative for the frames of reference, all of which claimed to explain the happenings around the baby's death. MaMeli actively made room for her preferences whilst acting in a subordinate role by using the tools at her disposal. She behaved according to the norms of respect for elders and refused to challenge the existing social order when she avoided conflicts – by lying about her marriage, leaving her medical booklet for her mother find out about her HIV infection, or planning a pregnancy to hasten marriage negotiations. I also sensed that she felt insecure at being left alone with her baby without the support of experienced women who could have told her that newborn babies usually sleep a lot and that she should be alarmed by her baby's incessant crying.

This is as much as I feel comfortable to say that I know. I am sure, however, that I lack detail on the following: the events in the delivery room; those at MaMeli's home; with whom she interacted; and how the midwife reacted to the accusation of having dropped the baby and fractured its skull. There is no convincing portrayal of those parts of the story. However, by defining my gaps of knowledge in this clear-cut manner, it was easier for me to write about what I had learnt, my version, in the name of science, not in the name of MaMeli.

Conclusion

Do the stories matter? Yes and no. Taken literally, the different narratives of MaMeli's story were contradictory and confusing and I did not know whom to believe. The case challenged me methodologically and analytically. Methodologically, I was unable to verify the information: the happenings around the baby's death were in the past and I had missed the opportunity of establishing their facticity through first-hand observation (and perhaps to change

the course of events) (see Hastrup 2004: 467). Having heard four versions of the same story revealed some congruency but also quite a bit of inconsistency, so that the material seemed to be useless to recapture the course of events. How was I to bring such diverse accounts into resonance with each other? This opened up analytical challenges.

First, the way I tell the story is a fifth version, one that I as anthropologist and author decided to tell you, my imagined readership. I directed your thinking (foreshadowing) by adding my thoughts on approximate reconstructions, frames of reference, lying, biographic illusions, and authoritative knowledge, and posed a line of questions that I am unable to answer. One could counter that I did not try hard enough to check the facts that constitute the narratives. That is correct: I chose rather to work towards providing a plausible explanation for how the individual narrators presented the information and their selves.

Second, the material collected on MaMeli entailed a blessing in disguise. Revisiting the material in the process of writing, I began to recognise its value. Analysing the speech act in the present (*Erzählzeit*) was more informative for my study than what happened in the narrated past (*erzählte Zeit*). The stories had errors and inconsistencies and were manipulative; beyond the surface, they were suggestive in regard to culture, power, and conflict. What was said and unsaid pointed out the power relations between those who were involved in the various situations, including MaKhotso and me. That was the relevant data I had at hand. I may have over-interpreted the social relations (who can tell for sure?), yet I did so with the best intention and in respect of my theoretical and empirical knowledge. In the text, I established that knowledge happens in social interactions. My double-agent role as researcher and actor in the social drama suggests that I migrated between different social frames of reference that allowed me access to knowledge exclusive to me and my academic audience. Such knowledge was not accessible to MaMeli, her mother-in-law, or the medical staff. Given this, I saw something in them and in their interactions that the interlocutors themselves might not have been aware of. This position gave me the authority to present in my version the whole dispute over the hegemony of interpretation, from an apparently more holistic point of view that encompassed all other frames of reference. It is from this position that I as anthropologist judged certain statements as embellished, neglected, selected, authoritative, or silenced and, lastly, saw narrations as performative acts of self-representation.

Third, I agree with Piker (2011: 985) that 'lies reveal deeper truths, examining the process of constructing and maintaining lies helps us to appreciate the interconnectedness and insecurity of our subjects'. It feels unjust to call the four stories lies, and it seems more plausible that informants intentionally or unintentionally interpreted the events in different ways. It is not my task as an anthropologist to correct the stories and to make them fit

nicely, but it is my task to point to why these contradicting stories matter. If different accounts do not fit nicely, I argue it is my task as anthropologist to inform my readership accordingly. I second Passin (1942: 246) in his conclusion that 'it remains for this writer simply a catch-all for such lies as he cannot now explain, pending their future disposition as a consequence of greater knowledge'. Let us be open about the gaps in knowledge and not pretend that the answers we find match the questions we posed.

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Where Does a Lie Start? Untruths, Half-Truths, and Strategic Self-Presentation in Ethnographic Fieldwork

Dumitrița Luncă

Introduction

Whilst conducting fieldwork for my PhD on migration and intimacy amongst Romanian migrants in Rome, Italy, between February 2017 and March 2018, I came across an intriguing story: in a parking lot on the outskirts of the city, a community of Romanians was congregating in a small, tarp-covered wooden church, led by a lone, self-described ‘missionary’ priest.

Improvised places of worship were not uncommon some thirty years ago, at the very beginning of the mass immigration of Romanians into Italy, but since then the Romanian Orthodox Church has established an extensive network of parishes throughout the country, with Rome counting at least ten. I was, therefore, immediately intrigued by this place, which seemed to be an outlier amongst the Orthodox churches in Rome and a far cry from what Romanians might have been used to back home, where the insides of even the more modest churches are covered in murals and heavily adorned with gold leaf. I wanted to investigate more deeply the reasons that made people attend mass in a parking lot when other, more church-like spaces were available.

Realising I would not find a place for this story within my thesis, I proposed the idea of an anthropological contribution to a narrative journalism magazine in Romania. For years I had admired the in-depth articles and strong focus on social issues that this publication was known for, in a vein reminiscent of ethnographic writing. I thought a cross between anthropology and journalism could benefit both sides: I could write something with less academic jargon for a non-academic public whilst maintaining a thick ethnographic description and attention to detail. At the same time, the magazine could benefit from my long-term fieldwork, allowing a deeper understanding of the social and cultural dynamics surrounding the church in the parking lot, a luxury that journalists do not usually have. I had long dreamt that I would one day be able to publish something in this magazine and was elated by the editor’s enthusiastic response to my proposal. But after more than a year of working on the article, just days before its planned publication, a lie told by the main interlocutor – the priest heading the church – came to the surface, calling the veracity of everything else he had said into question and completely derailing the story, along with my credibility.

This experience has enabled me to reflect on the topic of truth in ethnography, which I had previously contemplated solely in relation to the deontological responsibility an author holds towards both interlocutors and a potential readership. For example, the code of ethics of the largest professional organisation in the field, the American Anthropological Association (2012), stipulates that the researcher is obliged to be truthful and transparent in the research process and when presenting gathered data. Whilst advocating for a more contextualised review of ethical standards on a case-by-case basis, the German Anthropological Association similarly raises the question of ‘transparency’ (Hahn et al. 2008).¹ However, for all the discussions of researcher conduct, there are surprisingly few sources discussing the truthfulness, or lack thereof, of the data itself. This article, based on my personal experience in the field, reflects on this through a series of questions: What happens when interlocutors lie to ethnographers? How do lies told by interlocutors change what is observed in the field? And where does a lie start, anyway? To address these questions, I first examine in greater detail the lie my interlocutor told me and its ramifications. I then consider accounts by a few other ethnographers who have explicitly discussed being lied to in the field and finally open up the discussion to a wider debate on truth, lies, and anthropology.

The lie my interlocutor told me²

Shortly after my arrival in Rome, one of my interlocutors asked me to accompany her to the Sunday mass organised at one of the many orthodox churches that Romanians have established across the city. She warned me that this one was somewhat out of the ordinary, but I was yet to understand why. The following day I took the metro and travelled to the outskirts of the city. Outside the station was a large cement lot, filled with cars and busses, and with *ban-carelle* – improvised market stalls selling everything from used clothing to cheap electronics. This bustling area is so popular with Romanian migrants that the South-Asian men selling phone chargers, MP3 players, and speakers usually play popular Romanian party songs to attract business. I did not see the church at first, so I had to ask around until someone pointed out a fenced-up parking lot on a side street. There, to my surprise, stood a small improvised wooden church, partially covered with tarp and topped with a wrought-iron cross.

During the following months, I went back regularly. I was fascinated by the community that had formed around the short and affable priest, who wore modest clothing and scuffed shoes under his ceremonial Orthodox vestments. There were many other places of worship in Rome for Romanians,

1 For a critical review of ethical standards in anthropology, see Dilger et al. (2015).

2 This and the following subtitle reference Nachman’s article *Lies My Informants Told Me (1984)*.

where the decor and atmosphere were more reminiscent of the churches back home. However, many of the people devoted to the priest in the parking lot, most of them working in construction, cleaning, and care, preferred their spiritual guide to be a hard-working simple man like themselves, not someone 'with a master's degree' who chides and looks down on them. Until recently, the priest had himself been working in construction, distributing leaflets in mailboxes, and cleaning offices. When I met him, the congregation had grown enough that he could support himself from the small donations he received from his parishioners.

There was something else that differentiated this priest and his church from all the others in Rome. He was rumoured not to have been ordained. Every now and then an accusation would be posted on Facebook, but his followers were quick to take his side, as was he to defend himself in the comments section. He never shied away from bringing up these accusations with me, along with proof to refute them. The confusion, he explained, came from the fact that he belonged to the Ecumenical Patriarchate of Constantinople and not the Romanian Orthodox Church. Both are amongst the nine existing autocephalous Orthodox churches. The former historically has jurisdiction over some countries in the Mediterranean Basin where Orthodox Christians represent small minorities, such as Italy and Turkey. The latter governs over Romania, where the vast majority of the population declares itself Orthodox, but has in recent years extended its presence in countries with strong Romanian immigrant communities, such as Italy, Spain, and Germany. This resulted in some overlap, with priests from both institutions present in some areas. It was all a big misunderstanding, I presumed, as some Romanian people simply did not understand that both churches were equally legitimate and powerful. The walls of his little church were plastered with certificates of attendance from a Romanian theological university and a letter from an Ecumenical Patriarchate of Constantinople priest in Rome attesting that he was affiliated there. The priest in the parking lot often talked of this priest from the Patriarchate, calling him his boss. On his Facebook page, where he is very active, posting prayers, live sermons, and icons, he even had photographs of a visit that he and some of his parishioners paid to his boss' church in central Rome.

Shortly before the end of my fieldwork in Rome, I was joined by a professional photographer sent by the magazine to take pictures of the priest and the congregation. We spent an intensive week following him around whilst he performed his daily pastoral tasks. Over the next year, I worked on multiple drafts of the article, which was meant as a sympathetic portrayal of this improvised but devoted parish. Shortly before publication and as a legal formality, one of the magazine editors contacted the press office of the Ecumenical Patriarchate of Constantinople for confirmation of the priest's membership. The reply was shocking: the church had, in fact, never heard of him.

This discovery shook me. I had visited the church many times during my thirteen months in the field and had had countless conversations with the priest so that I could not believe he would be so dishonest with me. I felt disappointed and personally offended, even though I had to admit that he had not told me an outright lie but had simply stuck to his story. I could not believe he would so boldly lie to his congregation and to me, knowing how easy it would be to verify the information. I also felt foolish for believing all his explanations and for not having verified the information earlier. At the same time, however, I was convinced that the discovery would not really damage his reputation since his parishioners already liked him specifically because he was not part of a church system they criticised; his outsider status gave him a messianic aura. But then why lie to them? Aside from a few who might have worried that their baptisms and weddings were not 'real', since he was not a 'real' priest, most people incessantly talked about how down to earth he was, how good they felt talking to him, and how they liked attending his mass. Even though it turned out that he had never graduated, he had indeed studied theology and had worked in various capacities in churches, both in Romania and in Italy. He had a pleasant enough voice for singing and reading from the Bible, which he freely referenced in his conversations with his congregants, always bringing up just the right parable for the right problem. Many of the people I had spoken to in his church mentioned negative experiences with other Romanian priests in Rome, who were too dogmatic, and emphasised how different this priest was. On one occasion, when the accusations of his illegitimacy came up in a conversation with one of his parishioners, when I still believed that the rumours were all due to a confusion, she told me: 'What do I care? By whom must he be recognised, by God?'

An atheist myself, I empathised with people's need for rituals, spiritual guidance, and a sense of community, especially considering the difficulties of being a migrant, the social isolation, the distance from loved ones, and the hard work. Believing the priest's explanation of the two overlapping church jurisdictions, the possibility that he had not been ordained at all, in any church, had not crossed my mind.

For the journalists at the magazine, however, this one blatant lie meant there might be others waiting to be discovered and that the article might be putting the publication's reputation at risk. Indeed, in 2018, a journalistic fraud scandal that erupted when award-winning German journalist Claas Relotius was proven to have embellished and even fabricated his field pieces (Fichtner 2018) demonstrated how one person's lies can cast doubt over an entire profession and even discredit an entire political cause (Jones 2019). The editorial board of the magazine in Romania decided to put the story on ice and we all agreed to send an experienced investigative journalist to Rome to confront the priest and verify some of his other claims. In the following months, this journalist and a new editor reworked my text extensively and

added some new information, although no other lies had come to surface. The final article, which mentioned the lie in a matter of fact way, still focused on the priest and his parish, as well as the larger Romanian immigrant community in Rome, but lost the direction and depth that I had intended. I almost lost authorship and was given little space to shape the end result.

Journalistically speaking, the lie that the priest told me and his parishioners made him a less than sympathetic subject and an untrustworthy source. Anthropologically speaking, however, the fact that he was a rogue, non-ordained priest who built his own church in a parking lot and nevertheless had so many people flocking to him and swear by his advice was not only fascinating but also revealed to me something deeper regarding the relationship between the Romanian Orthodox Church and its believers, especially those who are migrants.

To many, the Church is disconnected from their real lives, as it holds up impossibly high canonical standards. For example, migrants working in households and living with their employers are often not allowed to cook their own food and thus cannot follow the extensive fasting schedule stipulated by the Orthodox Church. Required to work long hours and only having time off on fixed days, they cannot attend midnight mass at Easter or other ceremonies that do not coincide with their free time. The dissident priest, on the other hand, not burdened by higher-ups and strict church dogmas, adapted the schedule of his church services to the working hours of his parishioners, gave blessings over the phone, and performed engagement rituals without asking couples whether they had already had sex, even when it was obvious to him that they had. Whilst other priests barred parishioners from communion for seven years if they had attended mass in a Catholic church, the parking-lot priest was understanding and easily offered divine forgiveness.

For me, the priest's lie mattered on an interpersonal level: I struggled to understand how someone could persistently and convincingly lie every day to everyone's faces about something that was so fundamental to their person. But this did not change the fact that hundreds of people were looking to him for spiritual guidance and divine salvation and in this view, the lie made the case of the church in the parking lot even more interesting. My conclusion, I realised, made me a bad journalist, but a good anthropologist. It also prompted me to think more about the role of truth and lies in ethnography. What happens when interlocutors lie? And how far should ethnographers go to uncover the truth?

The lies interlocutors tell other ethnographers

Any discussion about lying in the field should start with the lies that ethnographers themselves tell (Fine 1993; Fine and Shulman 2009). They range from omitting details about one's life or beliefs in order to fit in better or

misrepresenting their research scope, all the way to failing to obtain informed consent or even doing research without the interlocutors' knowledge (Allen 1997). Researchers may also consciously or unconsciously manipulate a desired outcome by how they select their interlocutors, how they process their data, or when they fail to acknowledge 'inconvenient phenomena' (Duneier 2011: 9).

Even when it comes to lying interlocutors, researchers might also have a responsibility to carry. Bleek (1987: 320) proposes that fieldworkers are 'themselves liars when they do not tell the whole truth about the way in which they collected the lies from their interlocutors, thus obscuring the likelihood that it was their interrogating technique which produced the lies in the first place'. Indeed, lies, like truths, are always socially produced between certain actors and in particular social and cultural contexts and situations. Even though ethnographers, like me, might feel betrayed or confused when they spot inconsistencies in their interlocutors' accounts, trying to understand the mechanisms behind the lies can reveal important insights, perhaps even more so than direct information (Passin 1942: 236). After all, as Salamone (1977: 120) says, 'lying is a form of communication, not its negation'. Nachman, who worked on Nissan Atoll, Papua New Guinea, also contends:

Despite the problems that fieldworkers will encounter in such research, lying is so much a part of human social behavior that in order to comprehend with any certainty the life of a community, they must come to terms with this issue, for both ethnographic and methodological reasons. (1984: 540)

There are many reasons why interlocutors might lie, just as there are many degrees of (not) telling the truth. In fact, Berckmoes (2012: 136) proposes, 'lying is desirable in some situations; it is not inherently 'bad''. In conflict zones, such as in Burundi where she did her research, lying, adjusting the facts, or withholding information can oftentimes be a measure of protection. Lying can also be a form of agency, especially if the interlocutors are part of a population that has historically lacked political power (Nachman 1984: 538). Passin identifies various types of lies, based on his fieldwork amongst the Tarahumara Indians of Chihuahua, Mexico, and he calls this latter type of lies 'of cultural vested interest'. Another type of lies he observes is 'prestige lies', where an interlocutor might want to distort information in order to appear to have a higher social position than that currently held. Some lies are told to ethnographers specifically because of their position as outsiders or because of their perceived or real connection with certain authorities, whilst others are deeply rooted in cultural and social practices (1942: 242).

Moreover, lying, or rather truthlessness, can be ingrained in the cultural fabric of societies and is ritually and habitually employed, as Blum observes in her ethnographic work:

In Japan, as in China, some of the roles of language might be considered non-truth, rather than falsehood. For example, politeness requires humbling and elevation of the interlocutor. Whether the speaker genuinely feels abased or not, the language must be spoken thus. Politeness obliges a range of utterances, including not only compliments but even invitations. (2005: 303)³

Ethnographers who realised that the information they received might be flawed have chosen varying strategies for how to proceed. Some try to avoid the lying interlocutor (Allina-Pisano 2009; Berckmoes 2012), whereas others investigate further whilst maintaining vigilance (Fujii 2010; Kroeker 2020; Passin 1942; Saleh 2017). In regard to how to establish truth in ethnographic accounts, extended participant observation (Berckmoes 2012) and the situational analysis and extended case-study methods developed by the Manchester school (Evens and Handelman 2006) seem useful approaches. Some ethnographers attempt to cross-examine their data by asking different people about the same event, in an attempt to find out the truth. As Kroeker (2020) shows, however, even these attempts might prove futile: if the accounts of an event all differ from each other, then how does the ethnographer decide whom to believe and how to understand the reasons each person has for adjusting the facts? Fuji (2010) proposes that we should focus on the metadata – spoken and unspoken thoughts and feelings, such as rumours, inventions, denials, evasions, and even silences – in order to establish the truth and to understand the reasons behind the lies.

Truth, anthropology, and society

This is an essay about lies: white lies and ones black as night, evasions, exaggerations, delusions, half-truths, and credible denials. Consequently, it is about art and literature, and specifically the art and literature of anthropology, as ambiguously manifested in our unique genre, the ethnography. It is a response from one discipline to the pervasive epistemological skepticism of our times. At the beginning of the twenty-first century, it is swimming against the intellectual tide to discuss the truths that ethnographies may contain, so let us instead see what profit there is in examining the kinds of lies in which they traffic. (Metcalf 2002: 1)

These are the opening lines of Metcalf's (2002) *They Lie, We Lie: Getting on with Anthropology*, a volume which is in equal parts an ethnography of a Berawan Longhouse in Borneo, an auto-ethnography of the author's experience in the field, and a reflection of the constant renegotiations and

3 For extensive cross-cultural studies of lying, see Barnes (1994) and Bok (1978).

power struggles between anthropologist and interlocutor in their individual attempts to reveal and obscure the truth. Such methodological and analytical considerations, like the ones I have exemplified above, are part of greater debates regarding the nature of anthropology (is it a science or an art form? [Carrithers et al. 1990; van Maanen 2011: 34]) and the nature of truth in the society at large.

There are many nuances of truth and untruth when it comes to human action and interaction. Lying is ubiquitous and, indeed, quite often seen as necessary. In everyday life, so-called '*Lebenslügen*' (life-lies – in German –) (Simmel 1950: 310) or 'vital lies' (Goleman 1985), as well as personal and family myths (Hochschild and Machung 1989), are unconscious blind spots which help people make sense of their lives and come to terms with painful, unbearable truths. In politics, as Hannah Arendt (1972: 4) writes, secrecy, deception, deliberate falsehood, and the outright lie have always been seen as 'legitimate means to achieve political ends [. . .] since the beginning of recorded history'. Another common occurrence is not occulting the truth but simply withholding it, as is the case with the secrets and lies that doctors employ with their patients 'for their own good' (Fainzang 2006).⁴

Discussing anthropology, Wilson (2004: 14) delivers a scathing critique of what he calls the discipline's 'epistemological hypochondria' and its 'inability to move beyond a weak, relativist theory of knowledge', arguing that 'ethnography is inseparable from the pursuit of truthfulness'. Others contend that ethnography, anthropology's main methodological and theoretical tool, is rather a quest for meaning rather than truth (Wall 2018). Fainzang, for example, writes:

The anthropological approach is not intended to express an opinion on the duty of people or on the merits of their practices. It simply aims to analyse what motivates the choices of individuals vis-à-vis the fact of saying or not saying [the truth], even if we can infer a reflection on the freedom that this gives them, and on the power they draw from it in the relation to the Other. (2006: 28) (my translation)

Few anthropologists have openly discussed lying interlocutors, which is not a sign that it is uncommon but rather that it is uncomfortable and messy. This absence is particularly glaring seeing that one of the most resounding scandals within anthropology, Derek Freeman's (1983) public challenge of Margaret Mead's *Coming of Age in Samoa* (1928), was based on accusations of falsehood. He claimed that her work lacked academic rigor, that her interpretation was naïve, and that her interlocutors had been deceitful. Freeman argued that Mead's conclusions were deeply flawed because her adolescent

4 For more on secrets, see Bok (1983).

interlocutors ‘intentionally misled her out of combined notions of Samoan courtesy and simple girlish mischievousness’ (Young and Juan 1985: 67).

The main reason for the silence on this topic could be the fact that discussing, and therefore calling into question, the very foundations of our field might open up questions of legitimacy. But perhaps there is no better time than now to bring up such a discussion. First of all, we live in what has been called a ‘post-truth’ society (Mair 2017), one in which the line between fact and fiction often becomes blurred (as illustrated by the monikers ‘fake news’, ‘alternative facts’, or ‘truthful hyperbole’ [McGranahan 2017: 244]) despite people’s unprecedented level of access to information. Second, there is an increasing cross-pollination between disciplines using ethnographic methods, including across the academic/public divide, such as between anthropology and journalism. Third, anthropologists are increasingly addressing subjects from the news cycle, as we have observed during the so-called 2015 European migrant crisis and the global Coronavirus pandemic, a fact which has made their work more and more relevant to the general public. All in all, the work of anthropologists in particular and ethnographers in general is more pertinent and accessible than ever to the general public and this creates an increased need for transparency, critical analysis, and, consequently, a more honest discussion of lies from the field.

Between lies and strategic self-presentation

To conclude this short discussion of lying and ethnography, I return to the story which prompted this reflection: the priest in the parking lot, non-ordained yet beloved by his congregation. I have come to see his lie as a combination of a ‘prestige lie’ (Passin 1942: 242) and a ‘noble lie’, which ‘may not be justified by an immediate crisis nor by complete triviality nor by duty to any one person’ but which the liar considers ‘right and unavoidable because of the altruism that motivates them’ (Bok 1978: 75). This is also related to Jones and Pittman’s (1982) ‘strategic self-representation’, an idea based on Erving Goffman’s (1959) presentation of self as a kind of dramatic performance. Whilst it can be argued that we all, to a certain degree, enact our gender, class, or professional status in front of an audience, Goffman (1959: 18) distinguishes a type of ‘cynical performer’ who deludes their audience for self-interest or for what they consider to be the audience’s ‘own good, or for the good of the community’. Much like a performer on stage, so did the priest enact his priesthood, not only by providing (false or misleading) documents purporting to prove his legitimacy but also by using priest-like mannerisms, costumes (his vestments), and scenography (the home-made church itself) in front of his audience (the congregation).

These theatrical elements, along with the fact that officiating mass in many Christian denominations is usually a highly choreographed perfor-

mance following a preordained script, make it even easier for impersonators to take on the role in a convincing manner. This is illustrated by the Polish film *Corpus Christi* (Komasa 2019), about a reformed juvenile delinquent who is mistaken for a priest, a role which he then performs with gusto and charisma. The story is inspired by both a particular case and the pervasiveness of the phenomenon of fake priests in Poland (Ellwood 2020). Even more common around the world, albeit with more serious consequences, seems to be the appearance of numerous fake doctors, often confidently wearing the garments of medical personnel and reciting medical jargon for years before getting caught (Martyr 2018). In recent times, several cases of people who used deceit to build their careers have become public, ranging from those who faked their ethnicity to others who faked diplomas or plagiarised other people's work. In 2018, for example, Wolfgang Seibert, leader of a Jewish community in Germany for fifteen years, was exposed to having faked his Jewishness (Doerry and Gerlach 2018). The same year in the United States, long-time civil rights activist Rachel Dolezal was revealed not to be ethnically African American but to merely 'identify as Black' (Haag 2018). In Germany, perhaps the most high-profile case of this sort took place in 2011, when Karl-Theodor zu Guttenberg, a rising star in the Christian Democratic Union, was stripped of his doctor title and forced to resign as defence minister after being found to have plagiarised his doctoral thesis (Pidd 2011).

In the case of the priest in the parking lot, the consequences were not of a similar magnitude, partly because he wielded little power in the grand scheme of things and his fall, if there was to be one, was not from a much elevated level. After the journalist sent to Rome concluded his investigation into the priest and completed the article, it was finally published (Odobescu and Luncă 2019). The parishioner who invited me to the church the very first time wrote to congratulate me on the publication. The priest, she explained, was 'a little upset', but the truth had to come out. People continued to attend his services in the parking lot. He posted the link to the article on his own Facebook page and gathered many 'likes' and congratulations. It seemed to me that people either did not read the entire article, did not care or think it important, or simply concentrated on its positive effect, the 'publicity' it brought.

This experience made me reassess my relationship with my interlocutors and the role of truth in my own ethnographic writing. Whilst conducting my doctoral fieldwork in Rome on the intimate lives of Romanian migrants, my experience was one of interlocutors gladly sharing their secrets rather than trying to deceive me. Often I had the feeling that, as much as they were helping me in my research project, they were also happy to be heard and to have someone genuinely interested in their lives. I recognise that when I tried to expand towards journalism, I should have been more vigilant. Despite feeling duped in this one instance, I nevertheless continue to believe that inter-

locutors more often than not tell the truth, or at least what they believe to be accurate. After all, perceptions are true in their consequences (Thomas and Thomas 1928). In fact, anthropology as a discipline, along with ethnography as a method, hinges not only on the skills of observation and interviewing but also on trust between researcher and researched. At times this may be seen as its weakness, but it is also one of its greatest strengths.

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Composing Ethnographic Texts: How to Use Stylistic and Argumentative Techniques Properly

Mira Menzfeld

Introduction

Writing to please has something to be said for it.
 – Clifford Geertz, *Works and Lives*

Imagine leaning back in your office chair and unpacking a freshly pressed book. The last beams of the setting sun stroke your wooden desk. You are sitting alone in your small working space, which feels cosy and intimate in the fading light. Most of your colleagues have already left for one of the crowded rush-hour buses back to the outskirts. You, however, decided to stay a little longer. Now, at the end of a busy day, you allow yourself some stolen minutes alone with the latest title from a new anthropology series. Today it finally arrived. You smell the paper and ink whilst running through the redolent pages with your thumb. You open the book on a random page, excited to take a peek into all the yet unknown stories that it holds. Then you read: ‘being thrown into the transitive hermeneutics of loosely connected ontologies which have been simultaneously challenged and realigned by representations of phenomena that are yet to . . .’. Your gaze starts wandering. Your open and laid-back posture tightens, your shoulders stiffen. You glance at the clock (already seven!) and put the book away. Your willingness to be captivated is lost, at least for now.

This is the initial disenchantment we provoke if we forget the reader during the process of writing, something we should avoid.

Most of anthropology’s classics shine timelessly in at least one regard: they are written compellingly. I name just a few whose particularities still set the right tone. Margaret Mead not only provided the most novelistic descriptions of field scenes (see, for example, Mead 1928: 14) but also published publicly acclaimed poems and magazine articles (Shankman 2009). Clifford Geertz used words to convince. The light-footed and polished rhetoric¹ in which he clothed his reservations against comparative approaches (see, for example, Geertz 1973: 26) surely played a significant part in contributing to the weak standing of cross-cultural comparisons in anthropology today. Geertz knew how to write and argue, so much so that his views became paradigmatic.

1 See, for example, Geertz’ (1988: 8) more extensive ‘mule’ analogy or the small but effective analogy to romances.

Good ethnographies even charm people who are not anthropologists. Erudite critics of the newspaper *Die Zeit* (Raddatz 2009) as well as random survey participants interviewed by *Le Monde* (Savigneau 1999) counted *Tristes Tropiques* by Claude Lévi-Strauss (1992) amongst the 100 most important books of all times, alongside renowned works such as Homer's *Iliad*, Kafka's *The Trial*, and Augustine's *Confessions*. Lévi-Strauss's opus magnum manages to navigate the paradox of forming a paragon of intellectual heaviness whilst simultaneously captivating lay readers – notably despite, or perhaps because of, the author's notoriously rigid 'rage for order' (Geertz 1988: 143). He succeeded in this delicate balancing act because he composed the text competently: *Tristes Tropiques* impresses with innovative thoughts for its time, colourful field scenes, catchy phrases (just remember his 'I hate travelling and explorers' [Lévi-Strauss 1992: 17]), and a palpable author's persona, as well as surprising and yet coherent insights for the reader. His capability as writer allowed Lévi-Strauss to reach an audience far beyond anthropology departments. His audience did not just *force* itself to read; it wanted to *continue* to read.

One feature unites all great ethnographers: they are polite to the reader. They put a great deal of thought into presenting their ideas in a way that the audience can follow with ease. This politeness – formulating and structuring a well-thought-out text – is worth exploring here and cultivating for oneself as writer.²

Of course, writing an engaging text has its own merits, even if not necessarily intended as gesture of courtesy towards the reader. First, it is fun. Second, it saves time and effort whilst garnering more positive responses to our ethnographic texts. Third, and quite bluntly, it helps us pay our rent and keep our jobs. But what is perhaps even more important is that the attempt to write compellingly should be motivated by an intrinsic desire to be read and understood (see also Salazar 2020); otherwise it makes little sense to publish at all. At the same time, I am well aware of course, it is impossible – and unnecessary – to satisfy all potential readers; an audience is not a unified mass of people with synchronised tastes and interests. And we also have other duties to perform, besides impressing recipients with our stylistic skills. Generally speaking, though, it suffices to adopt a writing style that does not scare away the majority of readers by the third page. Clear formulations and tight arguments help a lot in this regard, as I discuss below. And yet, even if we remind ourselves that it is sufficient to compose a readable text and that no-

2 When I argue for readable ethnographies, I do not mean that anthropologists should compete with novelists or that they need to write the next most important book of all times. On the contrary, there are good reasons not to confuse the job of a novelist with that of the ethnographer, as I elaborate later. My argument is rather that it can be of benefit for ethnographers to consider, from time to time, whether their texts are readable for their potential readers, and to edit their writing if that is not the case.

body expects us to win the Nobel Prize in literature with our monograph, attempting to become a reader-friendly writer can prompt unease. This unease can manifest itself in many ways, ranging from writer's block over the fear of being critiqued for writing for public audiences to crises of authorship and representation. However, we should not underestimate the help that dialogue over writing techniques and the sharing of experiences can offer in overcoming writing problems (and maybe other issues too).

It is in this context that I suggest we engage in an intradisciplinary dialogue to explore ways in which we can compose honest and readable ethnographic texts. I open the discussion with a description of my own approach in the hope that it may motivate others to share their techniques – that we may inspire each other and stimulate a broader sensitivity to the importance of clear writing.

What makes for a 'well-written' ethnography? A personal approach

Ethnography must be done with grace, with precision, with an eye for the telling detail, an ear for the insight that comes unexpectedly, with a tremendous respect for language, with a compassion for homesickness, and yes, with a love of beauty – especially, of beauty in places where it usually is not looked for.
(Behar, 1999: 477)

In this quote Ruth Behar deftly defines the five components of a well-written ethnography. I examine three of these in greater detail: precision, effective details, and well-chosen language. But, first, I briefly explain why I turn to journalistic text-crafting standards when shaping ethnographies.

I was a journalist before I turned to academia – not only for its ethnographies but also because of them. I like them better than two-pagers, just as I like long-term fieldwork better than deadlines that barely allow you to spend more than a few days on a given project. Anthropologists resemble journalists, the 'junkyard dogs of ethnography' (Harrington 2003: 90), in many ways. Both share the difficult task of intruding into people's lives, uncalled-for, to dig out knowledge and experiences worth writing about. Both struggle with and agonise over 'correct' representation, authorship, and honesty (see, for example, Klusmann 2019; Rapport 1990; Atkinson 2013). But there are also striking differences. Anthropologists can dive deeply into a chosen topic and have space to write, even if the issues they deal with can be so voluminous that they are difficult to condense into a book. Journalists, on the other hand, have available a practical hands-on canon for crafting reader-friendly texts, and possess a heightened sensibility for keeping readers involved and interested.

One of the most influential text formats in journalism, often called its supreme discipline, is the *Reportage*, or feature article.³ Ethnography and the *Reportage* share a common root in travel reports⁴ (see Stagl and Pinney 1996; Rubiés 2002). They are siblings, so to speak. They both offer an abundance of surprising and reflection-prompting scenes, with insights into worldviews that would otherwise remain hidden to the reader. There is thus a certain fitting accuracy in this common descent. But because *Reportage* writing has paid more extensive attention to ways of writing, I draw inspiration from journalistic canons (especially Haller 1997) when trying to compose an engaging ethnography. *Reportage* writing influences my way of composing ethnographies in terms of text organisation, stylistic elements, and narrative techniques.

Of course, an ethnography is not a *Reportage*. Ethnographies usually take the form of books, target academic audiences, and rely on an author's deep familiarity with the perspectives and voices they present; readers expect them to present different results than what is offered in journalistic pieces. And there are other valuable techniques besides journalistic approaches that provide useful guidelines for crafting well-written ethnographies (see, for example, Narayan 2007, who draws on fictional storytelling techniques). However, because the writing craft is so carefully examined in journalism, and because it shares the non-fictional approach with ethnography, I here focus on the *Reportage* and how it inspires me when I am writing an ethnography.

The spine

Whenever I begin writing anything, I am reminded of my former training editor declaring: 'if you *do not know what you want to say*, your text will not be worth the paper it is written on'; or, the more precise and considered the example chosen and the argument made, the more you will catch and convince the reader. And so I begin by defining my main argument, summarising my main findings, and identifying the supporting evidence I will need. Of course, there are a few immensely gifted people who are able to create convincing texts by just starting to write. Most people, however, do better by thinking through what they want to say before beginning the actual writing: for if they do not, they may end up producing impressionistic streams of consciousness (which are horrible to read) instead of considered and compelling texts. I am one of those who need more preparation rather than less. Thus I start with a first, draft list of my main arguments, which at this initial stage means writing down little more than bullet points.

3 I use the German term *Reportage* because it captures more closely what I am referring to here.

4 These roots are sometimes still palpable in *Reportages*, though the latter's inclination to exoticise peoples and places has vanished from today's ethnographies (and fortunately from many contemporary *Reportages* too).

Next, I arrange these bullet points in a coherent sequence; thereby I create the framework for my future text. Sometimes I even look into some basic formal logic to order the arguments. To understand the importance of argumentative structure, we should keep in mind that ethnographies by definition use inductive argumentation; our entire discipline relies on collecting particular impressions to build up a larger picture of what is going on. Even if deductive elements are included – that is, when the author starts from a theoretical point of view and tries to explain particular phenomena through that lens – inductive methods remain the most frequent style of argumentation used in ethnographies. Despite this, we can see why having a well-constructed order of arguments is decisive for a good ethnography: if my argumentation rests on particular ideas that lead to a cumulative conclusion and interpretation, I should order them mindfully so that the reader can follow each step and (ideally) share my final analysis or interpretation. I can, for example, order subsidiary arguments in a linear way: an introduction that sketches where I am leading the reader, observation A, premise B, context knowledge C, and the culmination in conclusion D. Alternatively, I can choose an indirect form of argumentation, saying something like the following: initially I (or colleagues) thought A; this made sense because of B; my research showed C; I can illustrate the rationality of C by D and E; There are many other ways in which to convince readers. However, it is vital to have *at least an idea* of how one wants to structure one's argumentation. From an ethical point of view, it is crucial to use only those arguments that are most compelling and adequate. Ideally, the way the argument is built up will not only seem convincing but will also be logically valid.

Some people intuitively follow a temporal sequence based on the order when they made their findings and discoveries. This is not always a good idea. For an academic audience, this strategy can even evoke mistrust, sometimes being interpreted as an author's basic incapacity to analyse (see, for example, Hammersley and Atkinson 1983: 212–213). A chronological approach can also be confusing to read and may place too much emphasis on subsidiary questions and marginal discernments and too little on the most important insights. If the line of argumentation meanders too much, only few readers might have the patience to read to the last paragraph where the final insights appear. It is thus likely that many will not get the most important point.

As soon as I figure out my arguments and how I want to structure them, I shore them up with substantiating evidence. I thus identify descriptions of events and encounters that led me to my arguments and insights in the first place. Such *pars pro toto* scenes⁵ – individual scenes used to represent the

5 Different disciplines – photography, journalism, rhetorics, and others – use the expression *pars pro toto* (a part for a whole) when a limited part is used to represent the entire object, person, or situation to which it belongs. An example would be a detailed close-up on Frida Kahlo's eyebrows that a film director uses to remind the audience of Frida Kahlo herself. The part (*pars*) can be,

larger picture – illustrate arguments in many kinds of texts. We also apply *pars pro toto* techniques intuitively in everyday conversations (as when we say ‘living under one *roof*’ and everyone understands that it refers to all individuals living together in the same housing structure). Narrating *pars pro toto* scenes is a basic human communication strategy, which is why they are very accessible and convincing for readers: one tends to believe in reasoning structures that one knows well. Moreover, *pars pro toto* elements expressed as quotes and colourful scenic descriptions perform an especially important function for ethnographers. Many ethnographies alternate dense descriptions of selected ‘usual’ situations, of ‘normal’ proceedings, and of everyday occurrences in the field (an approach that evolved together with the genre of ethnography itself [see Malinowski 1922]) with depictions of special events and portrayals of particularly ‘dramatic’ situations. Even the extraordinariness of these dramatic scenes often highlights a particular process, or basic characteristic, that is an integral part of the sociocultural context in which the scene takes place (see, for example, Turner 1974). Thus, descriptions that seem to bring up supposedly dramatic counterpoints, or scenes that are somehow ‘different’ and special, fulfil *pars pro toto* functions too – because they complete the picture of what the ethnographer wants to tell. Narrative *pars pro toto* close-ups contain, inspire, and reveal authors’ key conceptual ideas (see Atkinson 2020). Ultimately, they are what ethnographies rest on, and will decisively influence the image the readers receive of the field site and interlocutors. Consequently, we have to select with care the particular scenes we consider suitable to justify and illustrate our larger argument.⁶

For me, writing several versions of each possibly relevant scene is a good way to find clarification. I will thus paraphrase field event **a* repeatedly, leading to versions **a1*, **a2*, **a3*, and so on. The more often I reword the same

but is not necessarily, representative of the addressed entirety (*toto*, the ablative case of *totum*). Usually, the part is somehow characteristic of the entirety but not sufficient to be the whole’s main component: Frida Kahlo certainly would have existed without or with differently shaped eyebrows, and her eyebrows say nothing about her style and abilities as a painter. Yet, a look at a picture of Kahlo’s eyebrows will evoke associations with Frida Kahlo, her life, and her works for anyone familiar with her. I use the term whenever I want to explain quickly how ethnographic argumentation works. Scenes within an ethnography will never be sufficient to give a complete picture of our fieldwork and interlocutors. However, if we choose the scenes well, we can dare to let them stand for characteristic aspects of the experiences we had, and of the people we met.

- 6 Sometimes it suffices to identify the one most important single scenario that happened in the field. If sufficiently rich and dynamic, this scene alone can serve as a thread that runs through the whole ethnography. Woven into it, the author can organise all other arguments and contextual information (see, for example, Shah [2018] who constructs her ethnography around the singular core plot of a night walk with Indian guerilleros).

situation, the better my understanding of its potential significance becomes. Each reformulation emphasises different aspects and qualities and reflects different meaning levels. In a next step I compile a single condensed narrative from these diverse versions, drawing on the best parts of each and thus producing account *A*. The procedure of forging forward with only the richest and most accurate parts of each version results in one very usable description of the scene. After proceeding in this manner with every field event that seems potentially relevant to me, I achieve a number of rather well-formulated and cohesive *pars pro toto* descriptions (*A, B, C, . . .*), all ready to be woven into the draft.

This is the point at which I pick up my bullet points again to connect the condensed *pars pro toto* scenes with my main arguments. I often find that *not every engaging field scene is actually relevant* in terms of the overall topic, text message, or argumentation. In order to craft a consistent text, it is vital to include only the most meaningful and appropriate scenes. Weak *pars pro toto* scenes, or quotes forcefully made to be suitable, would at best make me appear like a waffler, at worst a swindler or fool. The main task now is to identify the stories that ‘can’t be left out’ (McGranahan 2020a: 54) and to match them with the arguments they confirm.

Sometimes it is obvious which scene led to, or ‘belongs to’, a specific argument. If so, it is not only the author who rejoices in how smoothly a description from the field blends with a conclusion; it will also be easier for the reader to follow and remember the line of argumentation when observation and interpretation seamlessly and ‘intuitively’ fit together. It is, for example, not only hard to separate Scheper-Hughes’s conceptualisation of mother love as a cultural construct from the situations she describes in *Death Without Weeping* (1992); it is also hard to fundamentally doubt her conclusions after reading the scenes that build up her argument. Being captivated and touched by her experiences in the field, the reader intuitively synchronises with her conclusions, which seem to almost tangibly grow out of the research context.⁷

Yet sometimes the most intriguing event that may have been impressive and spectacular in itself says nothing particular of value in terms of the larger argumentative structure. Similarly, sometimes the catchiest quotes become insignificant when related to the larger topic of the text. Additionally, as we all know, even seemingly unambiguous quotes can acquire a different

7 I here chose examples that readers of this special issue are likely to be familiar with. They may be recent ethnographies but may also be classics that possess an individual style which many anthropologists will instantly be able to recall. It is these references to classics that remind us in particular that a ‘twenty-first-century publication date is no guarantee that a text will be livelier or more compelling than something written in the early or mid-twentieth century’ (McGranahan 2020b: 7). Regarding good style and stringent argumentation, even ethnographies written a century ago often appear to have anticipated the stylistic rules that professional writers learn today.

meaning in a different situation.⁸ In fact, there are many ways in which a scene or quote may turn out to be useless. To decide whether it should be part of my ethnography, I apply a simple strategy: if I cannot immediately attach it to one of the bulleted arguments, I cut it out. If a field situation does not intuitively relate to any of the arguments, be it by contradicting them or by illustrating them, it should not be part my text.

Whilst matching the most meaningful scenes and quotes with my arguments, I try to *consider all possible readings* of a scene – not only the seemingly obvious ones. If I do this honestly, I will realise that some conversations and experiences do not actually speak to the topic or argument I had initially associated them with. I will find that some interlocutor responses fit better with questions that I posed at different occasions (or that I never thought of in the field) than to the questions they were given in answer to. If I come across such mismatches between my initial understanding of a scene and other possible layers of meaning, I may find the need to readjust my interpretation, my argument, and even the table of contents that I might have already begun to imagine in line with what I now think this scene was *really* about.

An episode from my PhD dissertation (Menzfeld 2018) may effectively illustrate this issue. Initially I wanted to include a substantive chapter on the final words uttered by my research participants on their death beds. I hoped to connect it to a conversation with a terminally ill interlocutor from Germany named Hermann who had talked extensively about the significance and non-significance of final words. In the end I did keep the scene with Hermann but incorporated it into a discussion about changing perceptions of relevance whilst dying – not one about final words. Indeed, when the section was complete, the ‘last words’ played only a marginal part. By working on the material I began to understand that the topic on the ‘final words’ was much more an obsession of the non-dying (like, for example, relatives, or me) than of my dying interlocutors. And I came to realise that this was something Hermann had already begun to explain to me in our conversations but I had not been attentive enough to hear. When I sensed that there may have been a deeper meaning in Hermann’s words, I double-checked my notes on other conversations on final words and realised that many of them had actually spoken about a dying individual’s perspective on relevance and irrelevance. Yes, the importance or pettiness of words did sometimes play a role, but more often the relevance of final experiences and actions was the crucial underlying topic. Finally, the chapter I wrote dealt with perceptions of relevance con-

8 As a mnemonic aid to remember the importance of context, I like to imagine a person drunkenly slurring ‘I want to marry you’ to a complete stranger and then falling asleep on the last bus of the night, and how this differs from the exact same words uttered in a serious proposal on a secluded beach. The first incident is likely to reveal more about local norms regarding alcohol consumption and the situational tolerance for intoxicated behaviour in public than about marriage and matchmaking.

nected to actions, experiences, and (also) words, woven around the supposed ‘final words’ conversation with Hermann – for this had emerged as the most important layer of meaning (see Menzfeld 2018: 206–210).

Some scenes, however, possess various layers of meaning of equivalent importance. In these cases, we might not have an initial misreading or mis-prioritising of a layer but may have to manage manifold connected meaning dimensions. One possibility is to place the most multilayered scenes into the first chapter, pointing towards the conclusion or towards the way the different layers will be revealed and explained in the subsequent chapters. Alternatively, these scenes can be placed at the end to illustrate the larger conclusions. It would not be clever, however, to place an especially enigmatic scene within chapters that deal with smaller arguments or minor findings. This would make the reader assume that I simply did not understand the multiplicity of meanings inherent in a scene, or was reducing the complexity of the scene in order to veil some of the conclusions that might be drawn. Though these suspicions might be wrong, to write in a way that allows for negative speculations would destroy the author’s in-text credibility as competent and scholarly. In my own work I therefore always consciously read my depictions and analyses ‘against myself’ and modify them if they offer any possibility for misunderstandings.

In the writing process, I also pay attention to the danger of placing scenes near (or connecting them with) arguments that may cast a different or misleading light on them. If I get into a situation where I would have to delete important paragraphs or intra-textual connections because they could be misunderstood in the context they are in, I provide footnotes to explain how such a reading would be inaccurate. But if this requires a long explanation, I prefer to cut the entire scene: in this case the inclusion would definitely cause more misapprehension than enlightenment.

After matching my arguments with key scenes, I weave in references to the relevant literature. Drafting *literature overviews and formulating references* to my colleagues’ theoretical approaches does not have to result in boring or repetitive paragraphs. In fact, re-reading existing texts at this point provides the opportunity to pick out the most outstanding quotes and the clearest definitions, as well as vivid examples that can contrast or match my own field experiences. If I choose carefully, I am profiting as a reference free-rider (in a positive sense) based on my colleagues’ writing talent; this is anything but dull for the reader. It also reads elegantly when quotes are blended well into one’s own (counter)arguments and *pars pro toto* scenes. The same goes with general background information: if woven into a thrilling narrative line of first-hand field scenes, the reader will enjoy even the para-

graphs in which the author situates this field experience in a larger historical or economic context of the area, topic, or persons studied.⁹

There are, however, situations where longer literature reviews may be required. Yet, producing long paragraphs that reproduce what others have said can waste the reader's time (the work discussed may already be quite well known), may portray the author as unoriginal or even slightly incompetent, or may disturb the flow of reading, as it may mean breaking from the writing style used elsewhere. Therefore, when I have to write longer literature reviews, I try to identify cross-cuts and unusual connections between works in order to allow commonly known texts appear in a fresh light. Usually there is much more in a text than the author thematises in the header. It is exciting to read texts under the lens of a different question than the title suggests and discovering hidden aspects than those commonly acknowledged.¹⁰ This strategy enables one to add an unexpected and hopefully interesting half-sentence here and there to an otherwise 'ordinary' literature overview.

As I thus write more and more text, I already engage in rereading my longer paragraphs to check whether they would appear reliable and comprehensible to my future readers. *Reliability* is just as much an issue for the *Reportage* as it is for ethnographies. Both genres describe scenarios and phenomena that are highly situational and dependent upon the persons involved. Moreover, the reported scenes can be far removed geographically, linguistically, and culturally from the readers. As these cannot quickly go to the field site to check up on the facts and interpretations I present to them, proving that my account is reliable and trustworthy is an intrinsic challenge for me as author.

Different disciplines have different ways of building credibility. Larger magazines and news houses have fact-checking departments where specialists double-check what their in-house journalists write. In Germany the debate around the reliability of facts, and the precautionary measures required in high-quality journalism to ensure factuality, has intensified in the wake of the recent Relotius affair (see Klusmann 2019; also see Luncă, this issue). In contrast, honesty and sincerity are an ethnographer's sole means to show that the ethnographic account is valid, specifically when concerned with remote areas or people speaking rare languages. There is no fact-checking unit rechecking ethnographic texts before publication, and it is only the academic audience that appraises an author's credibility.¹¹

Even with the fact-checking in journalism, the journalist and the ethnographer are tasked with demonstrating credible first-hand competence of the

9 See, for example, Shah (2018: 204–212) for a nimble embedding of background information.

10 An example would be asking what Inge's (2016) research on Salafi women says about being a person of colour in Great Britain.

11 See, for example, Reichertz (1992) on issues of ethnographic credibility and persuasiveness; and Clinard (1970) on academic respectability.

phenomena they write about. Ethnographers do not simply create captivating books that are plausible in themselves: they must also grasp wider, contextual realities. This separates ethnographers from fiction writers and novelists whose created, in-text plausibilities do not necessarily have to refer to out-of-text realities. It does not limit our pleasure of the Karl May adventure stories situated in the American Old West if the author never visited America. It does not diminish the way Lily King conjures up Margaret Mead's research setting in her best-seller *Euphoria* to know that she did not possess a particularly deep understanding of Papua New Guinea (King 2015; see also McGranahan 2020b). However, most readers would be rightfully unsettled and suspicious if an ethnographer appeared sloppy in terms of deep knowledge of their field site or over-fictionalised field experiences. What readers expect is that that ethnography convince them that the author has 'been there' (McGranahan 2020b: 8), in the field; and that the author is able to interpret field experiences plausibly and honestly.

Interlocutors trust ethnographers with their stories, and we have an ethical commitment towards them not to unduly harm or distort their realities and perspectives (McGranahan 2020b). Nevertheless, we also hold the parallel responsibility of writing reliably for our readers. It is an ethical imperative to prove that, and why, they can trust us. To build and maintain this trust, ethnographers should take care to make their methods, and their possible biases, very transparent, elaborate on how they worked, and explain why they interpreted their fieldnotes the way they did. When I as an author explain myself in this regard, I inherently argue for being perceived as an honest researcher, which is crucial for my in-text credibility and my overall credibility as researcher. It is for this that the methodology chapter is crucial.

Putting flesh on the bones

Once the skeleton for my text seems robust, I begin putting flesh onto the bones. This phase of working on the text is driven by two overriding concerns: to achieve *clarity* and *intelligibility*. It means turning ethnographic texts that, at draft stage, can still be quite obscure or confusing into lucid and accessible narratives.

We often find published anthropological accounts that are still relativistic in nature (not necessarily something bad), vague (problematic though sometimes inevitable), or just obscure (definitely to be avoided). To a certain degree, indefinite and relativistic language simply mirrors the ambiguous, dynamic, and at times paradoxical phenomena we are trying to grasp. After all, anthropologists work with people, and people are neither binary, nor stable, nor definite. I cannot hope to completely prevent myself from using vague or complicated sentences: sometimes this is the only way to express one's grasp of a situation or an experience that is inherently indefinite. Yet

often I find that splitting longer and vague passages into shorter sentences already does wonders in terms of comprehensibility: if sentences have to remain vague on the level of content, it is at least a relief to the reader to be able to read them easily, without having to work out where they begin or end.¹²

But often academics chose unnecessarily obscure formulations: passive voice where active constructions are possible, unnecessarily abstract nouns, useless imagery, clauses that do not add anything, equivocal formulations, and the like.¹³ Obscuring expressions and phrases should be identified and altered before a text is published. I try to eliminate them whenever I notice them. A passive and blurry writing style may be quite acceptable to academics and even impress a few readers, but it will never convince a broad audience. It will seldom provide the most honest positioning, let alone the most useful insight for the readers.

In other words, it is *always* a good idea to choose active constructions and direct language, culminating in sentences that are not overly long. In most cases, we *can* communicate complicated realities and arguments in an accessible language, if we but try.¹⁴ Even the small effort of replacing the passive voice with active formulations helps a lot in this regard. Using names makes texts convincing and truthful. A welcome side effect is that active formulations and precise nouns force authors to think through what they want to say. One can easily hide intellectual sloppiness and indifference behind confusing sentences. It is much more difficult to do when using clear words that anybody can understand. Writing without overcomplicating what can be expressed simply takes discipline and is actually more difficult than writing obscurely. However, I often realise that it is indeed possible, even without narrowing down my thoughts. Frankly, most of our thoughts are of a less-than-Heideggerian complexity and do not necessarily require complicated language to be expressed. So why not formulate them in an accessible manner?

At times, I am glad to find that I hardly need to demystify a rather complicated draft passage if I provide vivid examples that illustrate the argument (perhaps by using a *pars pro toto* scene). In some cases, captivating depictions of field experiences fit well into the text and succeed in animating dull, complex theoretical passages. Care must be taken, however, not to become too enthusiastic as it might cause academic readers not to take the text seri-

12 See Klinkenborg's (2013) persuasive call for the use of short and clear sentences.

13 Howell (2017: 18) gives an excellent example of unnecessarily complicated formulations that look impressive but do not mean much once the awe of the author's linguistic flourish passes. In the end, obscurantism only disappoints and annoys the reader.

14 See Ghodsee's (2016: 84) argument for clarity and against 'academese'. She provides an outstanding guide to comprehensible ethnographic writing and useful examples for straightforward language and argumentation.

ously. A dense clustering of vivid paragraphs, for example, can make a writer look guilty of over-interpretation, exaggeration, or waffling. I do not think there is anything wrong with lyrical introductions or experimental ‘jazzy’ passages. However, I also want to show my audience that I have mastery of a sober and straightforward writing style. Therefore, I try to balance a factual tone with a lyrical appeal: the *overall* style should be as vivid and comprehensive as possible to captivate the reader whilst being as sober and precise as necessary to be truthful.

When I read, I enjoy styles of formulation that are *rich in contrast*, with smooth transitions, and endeavour to apply this in my own writing. It is possible to employ contrasts on different levels: for example, I try to narrate indifferences and ambiguities whilst remaining clear and accessible at the linguistic level. I interweave my own arguments with anticipated counterarguments that could relativise them, resolving the tension by giving a detailed account of why I prefer my own interpretation. I strive to alternate between a detached, factual tone and vivid depictions, whenever appropriate. Usually, it makes sense to blend the two, so that the text reads as if it were cut from one piece even though it may contain contrasting styles and tones. A brilliant example of a contrast-rich text that never loses its inner stringency whilst oscillating between cool, academic language and vibrant, immediate scenes is *Coming of Age in Samoa* by Margaret Mead (1928).

There are several techniques that authors can draw on in their writing to create contrast-rich texts. I discuss five here: suspense; verbs and nouns; adjectives; rhythmic formulations; and symbols, analogies, and metaphors. The first technique, suspense – thus describing something that initially remains mysterious or unresolved and is deciphered only later – works well for some ethnographies. Following van Maanen (1988) and his three-fold categorisation of ethnographic styles, suspense falls under the storytelling-heavy characteristics of what he calls an impressionist writing style.¹⁵ It is a classic tactic to captivate the audience. If I describe an event or practice that at first confuses or contradicts the reader’s intuition and offer an explanation or interpretation only at a later point, I create a narrative structure that is thrilling and insightful at the same time. By not immediately revealing how particular conclusions or scenes contribute to the larger puzzle can make longer monographs gripping and exciting to read. However, the reader’s intuitive focus on getting a release from tension, or their longing for the author’s explanation, can also distract the reader from questioning if a provided interpretation is likely or correct. Because of this potential for manipulation, suspense tech-

15 Van Maanen (1988) identifies three main narrative conventions followed by ethnographers: *realist* tales try to convince the reader that emic perspectives are being ‘authentically’ presented; *confessional* tales highlight the personal situatedness and feelings of the author; and *impressionist* tales try to bring both subjective and objective perspectives together by employing a novelistic, story-focused style.

niques should be applied mindfully. If an author masters that, as, for example, Alpa Shah (2018) does in her nightmarch account, recipients will feel as if they are being thrown into the middle of a captivating story rather than a dry academic book.

Strong, surprising verbs and nouns, the second technique, animate texts: they cannot be applied often enough. They specify and characterise ideas, theories, things, persons, actions, intentions, and (assumed) feelings better than all-purpose terminology (see Klinkenborg 2013; Haller 1997) – not only when it comes to precise definitions but also when it comes to field scenes. Because of this, I repeatedly rework the verbs I use to describe scenes; the aim is to put as much meaning into my verbs and nouns as possible. For instance, ‘she cherished having a room all to herself within the cottage’ says more than, ‘she liked her own room’.

Adjectives (and adverbs), the third techniques, specify and enrich sentences. But they are also risky to use. When used or even over-used in the wrong places, they can clog a text. Appraising adjectives that assess the quality or a person or event can plant disparaging subtexts in the reader’s mind, whether intentionally or not, and, when clumsily placed, can make the reader feel patronised. The overuse of appraising adjectives in fact-based passages can undermine the aim of writing in a manner that readers take seriously. Eliminating adverbs and adjectives, but using strong verbs, has thus become something of a gold standard of ‘good’ writing, especially in English-speaking contexts.¹⁶

And yet, adjectives (and adverbs) animate field scenes and strengthen statements. Certainly it is due to my enculturation in German-language journalism that I pay such attention to using as few appraising adjectives as possible, but I am also not comfortable with condemning adjectives and adverbs altogether.¹⁷ If I use descriptive – and, much less, appraising – adjectives considerably and precisely *when* I use them, it is likely that the reader will understand a particular field scene better (or even follow the implicit evaluation I am expressing). This is the reason why, when I rework my drafts, I do not simply delete as many adjectives as possible, as many native English speakers might do, and retain more than they might feel comfortable with. But my judgement is guided by the knowledge that descriptive adjectives are

16 Ghodsee (2016) makes a strong argument against the use of adverbs and adjectives whenever a strong verb expresses just the same, or more.

17 German journalism makes a useful differentiation between descriptive and appraising adjectives. They consider as useful and acceptable descriptive adjectives that capture facts (as in ‘she greeted each client with a *firm* handshake’), but try to avoid appraising adjectives (as in ‘the *open-minded* saleswoman’). See Haller (1997) for strategies of how to check a *Reportage* for its stylistic quality, which includes considering the issue of descriptive and appraising adjectives; see Schnibben (2007) for a hands-on checklist that contains questions on the use of adjectives.

most suitable for colourful field scenes or qualifying specifications, and that appraising adjectives, if employed at all, should be used when creating explicitly subjective impressions, or in hypotheses that I intend and allow to be criticised and questioned.

Our fourth technique, rhythmic formulations (often including accumulations, alliterations, and the like), are an effective but also crafty technique to make an argument sound appealing. Structural variety, syncopation, and a palpable pulse energise even seemingly dull paragraphs (Claus 2020: 41). However, rhythmic formulations' good or ill depends on their placement and the integrity with which they are used. Usually, at least the title of an ethnography should possess some rhythm. A subtle pulse instantly glamorises a heading, and more people will want to read the text it introduces. An easy way to check the beat of a text is to read it out loud. If the tongue stumbles, or if there is no rhythmic or memorable alternation between stressed and unstressed syllables, it is unlikely to capture the reader's attention. Good examples for titles with a rhythmic quality are *Coming of Age in Samoa* (Mead 1928), *Dramas, Fields, and Metaphors* (Turner 1974) and *Death Without Weeping* (Scheper-Hughes 1992). The ethical aspect has to be kept in mind in the use of rhythm, however. Well-crafted abstruse statements can seem as compelling as proven facts if their metric patterns stick easily in the reader's head. On the other hand, rhythmic formulations can also make a text appear dishonest or over-simplified because they may bear a resemblance with propaganda or advertisement claims. However, if embedded carefully and prudently, rhythmic formulations can jazz up ethnographic texts and help good ideas stay in people's minds.

Symbols, analogies, and metaphors, the last technique we consider here, evoke feelings of consistency and approachability in a reader. I write 'feelings' as a catchy metaphor or analogy can clearly also be used to pseudo-verify an inconsistent argument. Yet a sincere analogy can deepen a reader's understanding of what the protagonists experience. The seemingly simple knack of repeatedly addressing a broken cup, for example, can help concentrate an interlocutor's sentiments, global historical contexts, the suffering and hopelessness of the many, and even an ethnographer's compassion: We find an example in *Patterns of Culture* (2005 [1934]: 22) where Ruth Benedict describes a scene in which her interlocutor Ramon uses broken dishes as figure of speech. He begins talking about preparing corn soup, but suddenly switches to reflections on life's meanings and lost futures. Benedict, clearly a careful listener, keenly noted Ramon's metaphor. Paying attention to and interpreting interlocutors' metaphoric inventions is a safe way not to misplace metaphors, which happens easily when ethnographers rely on their own associations instead of their research partners' ideas and contexts. In my own work I avoid applying self-invented symbols, analogies, and metaphors. If I consider using them, I reflect carefully on how they may be misleading (in

fact, many metaphors and analogies fall short when one takes a closer look). If I then still feel the need to use them, I make sure to match them with the situational, cultural, and geographical context I am writing about – for unconsciously misplaced metaphors, analogies, and symbols are the easiest ways to make a text sound ridiculous.¹⁸

A confident and skilled writer can even use the paradoxical effects of consciously misplacing metaphors or analogies. In this way sarcasm can be used to draw attention to an argument, for example. The author can also try to familiarise the reader with unfamiliar meaning contexts by using analogies or terms from the reader's everyday life against which to explain what is going on in the field. Geertz (1983: 243; 256) used this strategy when he creates parallels between elements in Balinese cockfights and topoi used in Hollywood movies or with cricket. We also see its use in Malinowski's *Argonauts of the Western Pacific* (1922): the term 'argonauts' evokes associations with seafarers in an audience familiar with Greek mythology, even though argonauts have nothing to do with any Kiriwana meaning webs. Such parallelisms and analogies are, by definition, misplaced and often meaningless within the field context. Yet, this circumstance does not necessarily corrupt their aim, as they are specifically employed to take up the recipients' supposed world knowledge for an immediate intercultural translation of otherwise hardly accessible scenes from the field. Concretely, comparisons with Hollywood movies and cricket mean something to the anticipated reader, not to audience of the Balinese cockfight; but within the text, these analogies are supposed to serve as a shortcut, offering the recipients access to what a cockfight might mean for Geertz' interlocutors. It is advisable to employ such stylistic tightrope walks only if the author knows that the impact and side-effects of such ironic or deliberately unsuitable metaphors can be controlled. Not everyone finds intercultural comparisons such as Geertz' parallelism between Hollywood motion pictures and Balinese cockfights suitable. It is hard to say if they can ever be illuminating enough to become a truly justifiable part of an anthropological text – or if they are more often than not a paternalising instrument that reveals more about the author than the interlocutors' experiences.¹⁹ In my own work I prefer not to use intentionally mismatched symbols and metaphors; the practice has too many side issues to fit into the genre of the ethnography.

18 It would be embarrassing, for example, to describe a child's dress as 'white as snow' when the context described does not know snow, or where white is not associated with purity but death and decline.

19 See, for example, Rosaldo (1989) for a broader discussion of these issues, combined with a call for interlocutors to verify anthropological interpretations.

The final makeover

In the final makeover, I read and rework the full draft. My first read is concerned in particular with the following question: Did I take anything for granted? Usually, I will find passages that lack some *contextual knowledge* that the reader requires to understand. This is where I insert additional explanations. At this point, I also add necessary information on myself as ethnographer, paying attention to describe relevant context knowledge about me as carefully as I would describe contextual knowledge from a field that a reader is not familiar with. I take this approach simply because I cannot easily anticipate what contexts my readers *will* be familiar with.

My second read then shifts to consider this question: Is the text consistent and convincing? Once I am confident that this is the case (or at least more or less), I then insert explanations on questions that I left unanswered (which is usually much easier to do than formulating what one knows and is convinced of). Whilst there are always many things I do not know about my research site, I here focus on those questions that readers are likely to pick up on – for example, that it would have been interesting to visit X, but that I did not get access to it. Admitting and reflecting on the weaknesses of one's own work is part of writing with integrity. It is useful to keep in mind, however, that readers usually want to know what an author *did* find out and not what did *not* work out, so I generally try to keep these considerations as succinct as possible.

I then re-read my text a third time, to check if I managed to distance my private impressions and feelings from my descriptions of the interlocutors' points of view. There is nothing wrong with a few strong subjective opinions that reveal the author as a person,²⁰ and it is indeed necessary to 'write the author "into" the ethnographic text' (Atkinson 2013: 26). However, an author's perspective should not dominate the text unless this is designed to be about the author. My aim is usually to write an ethnography, not an ego-graphy (see also Atkinson 2013 on 'sentimental realism'). I aspire to make it easy for the readers to, for example, distinguish my own opinions from the perspectives of my interlocutors. Endeavouring to create a readable text, I also try to burden my recipients as rarely as possible with epistemological struggles. The problem of turning fieldwork experiences into insights that are at least more or less detachable from me as a person is *my* riddle to solve, and not one to pass on to the reader. Most recipients will justly expect a structured and reliable ethnography that presents findings without hiding insecurities and ambiguities. They will not, however, be interested in a painfully detailed presentation of an anthropologist's dilemma.

A fourth read is then necessary to smooth out the text and blend the shifts between different topics. Here I attempt to *reduce hard breaks* that do not have any particular meaning within the text structure. This means filling

20 Or rather *persona*, as Narayan (2007: 132) puts it precisely.

in the missing links between statements and paragraphs. Of course, leaving out conceptual bridges between paragraphs, arguments, and topics can be used as a stylistic tool (for example, when summing up or providing a list of definitions such hard breaks can make a text appear more factual). More often than not, however, hard breaks may point to an incomplete chain of argument or an incomplete smoothing of the steps between arguments. The good or bad use of hard breaks depends on the frequency and deliberateness with which they appear: they may contrast refreshingly with a writing style that has general flow but may disrupt the reader if used too frequently.

The last read presents a particular difficulty: deleting any words and parts that are not vital. This makes particular sense for articles with limited word counts but, counter-intuitively, applies even more when writing monographs with a potentially infinite number of pages. The longer the text, the less can an author risk boring the reader. In my own work, I sometimes delete up to a third of the entire text at this point. Because cutting always means letting go scenes or paragraphs that I had cherished, I usually place them into a separate document as collection of excerpts that I can draw on for later articles.

Before I submit my final draft to the publisher, I usually request one of two experienced colleagues to give me feedback. After having rewritten the text repeatedly, I can often no longer see the wood for the trees and the opinions of other readers can be very insightful, especially when it comes to the structure of the text and the final choice of scenes. At the same time I find it important to be circumspect with the evaluations I receive: too many opinions can distract me from my own line of argumentation (though I really enjoy critique *after* publishing as it inspires me for future texts). What is very important to me is always to have my texts reviewed by at least one person who has *absolutely nothing* to do with anthropology. Every sentence they do not understand is reason for me to rethink content, argumentation, and formulation. Even if I (rarely!) leave as is a passage that such a lay reader has heavily critiqued (for example, because it addresses a highly discipline-specific discussion which cannot be reasonably paraphrased), the critique usually provides an opportunity to improve the disputed paragraph on other terms, perhaps by formulating shorter sentences, using stronger verbs, and so on.

Concluding remarks

To close, I want to turn to the question of why it is good to write ethnographies. Ethnographies are author-friendly in nature. Anthropology's signature format offers considerable stylistic and thematic liberties. We can customise it according to our research topics and outcomes. An ethnography provides numerous opportunities to convince and captivate the reader. It allows us to

educate, entertain, and make people think. As ethnographers, we have the privilege of possessing relevant and stimulating first-hand insights. And an ethnography's length allows us to engage in a detailed and careful examination of our data, doing justice to the long duration and intensity of our fieldwork, much better than any short article could.

Clearly, we do not always manage to turn our author-friendly signature format into reader-friendly texts. Many authors do, but some do not, and it is the latter that I am concerned with. One reason for producing reader-hostile texts might lie in the author's uncritical and unmerited admiration for scholars who curate undisciplined obfuscation as their distinct writing style (in other writing professions, of course, the same behaviour is rightfully decried as narcissistic and leads to contract termination).²¹ Abstruse texts may also originate from an author's fear of peer critique, as obscure formulations are rarely subjected to sharp criticism. Furthermore, anthropology's traditional timidity to formulate insights clearly and boldly may also play a role (a reservation rooted in the honourable, but also potentially inhibiting, concern with the risk of misrepresenting people and phenomena in writing).²²

I think it more likely that it is ignorance that leads authors to write unclearly. I am quite certain that most reader-hostile texts are not deliberately created as such. It seems more likely that authors do not consider (or have never learnt to pay attention to) a text's clarity and comprehensibility as crucial criteria for evaluating quality of writing. Many of us rarely discuss with colleagues how to craft texts or how to recognise infelicitous writing, thus implying that the issue of how to communicate insights is of little importance. Why should any scholar reflect on their own unreadability unless they hear and experience that comprehensibility matters, especially since they often have to figure out on their own how to achieve such readability?²³

Causes of (frequently unintentional) abstruse writing, as well as the effects of such texts on our discipline, should be debated intensely. Yet, as long as such writing and bad communication are not widely recognised as an obstacle to the wider circulation of cultural anthropology's insights (and I think they are indeed an obstacle), many will continue to ignore readability as a critical quality of their ethnographic texts. This means significantly fewer readers for texts that have the potential of being of interest to a much wider audience.

Our lack of interest in the relevance and techniques of composing accessible ethnographies strikes me as particularly paradoxical in times when neighbouring disciplines seem very keen to appropriate our working style

21 See Ghodsee (2016) for an elaborate critique of unreadable ethnographies.

22 See Ghodsee (2016) on the issue and how to overcome it.

23 In German-speaking academia, for example, it is quite possible to become a senior researcher in anthropology without ever having received specific training in ethnographic writing and/or in advanced writing techniques.

and writing formats (or at least what they imagine these to be).²⁴ It seems that there is too little visibility of and knowledge about ethnographies to prevent academics from other disciplines from misunderstanding our very specific monographic style. Anthropologists do not communicate often and clearly enough about what we want our signature format to be, so that we are beginning to lose sovereignty over defining what an ethnography is.

Even more astonishing, we find it hard to imagine how important it is *in general* to communicate anthropological insights comprehensibly, and to a wide public.²⁵ The result is that we still play just a small part in public discourses that should be co-shaped by anthropologists because they touch on its core research fields (such as global health, supposedly religious conflict, or transmigration). There are a number of new initiatives that aim to change this condition. Some ambitious anthropological online channels deserve respect and acclaim for publishing relevant multimedia content quickly.²⁶ These channels are a pure gain in terms of anthropological reach. Nevertheless, communicating towards an audience that is actively looking for anthropologically inspired writing is not enough; we also need to reach those who do not yet know the anthropological approach.

There are only few anthropologists amongst the experts in the public eye who claim to be explaining cultural specificities,²⁷ which frequently results in an underinformed or even misinformed public opinion.²⁸ Yet as academics we have the duty to actively and comprehensibly inform the public about what we know (see also Ghodsee 2016). If we do not want our voices to echo only in limited social bubbles, we have to step up our efforts to be visible in the media and in public debates.²⁹ I suggest that developing a heightened

24 See Howell (2017) on the core characteristics of ethnography and anthropology, and how necessary it is that we delineate them clearly.

25 See also Schönhuth (2009) on the relevance and accessibility of anthropological insights.

26 See, for example, the *Witnessing Corona* blog, a joint initiative started in 2020 by Medical Anthropology/Medizinethnologie, Curare, the Global South Studies Centre Cologne, and *Boasblogs* to reflect on the coronavirus pandemic from an anthropological perspective (Boasblogs n.d.).

27 There are remarkable exceptions, scholars who take the risk of engaging the media and the public book market (see, for example, Rauner 2009). The German media landscape also shows centralised public relations offices increasingly connecting journalists with anthropologists (see, for example, DGSKA 2020).

28 Media debates about migrants or Islam often misuse and instrumentalise terms such as 'Kulturkreis' or even base arguments on essentialising fantasies of the supposed characteristics of male refugees (see, for example, Güntner 2016). Contributions like these even make their way into renowned newspapers but are rarely challenged by anthropologists.

29 Schönhuth (2009) and Antweiler (1998, 2005) make comprehensive pleas for a 'public anthropology' in the German context, initiating several prominent initiatives.

sense for strategies of communicating clearly, beginning with intensifying intradisciplinary debates on what we want our gold standards of writing to be and how to achieve them, would in themselves already make anthropological knowledge more visible. For a discipline whose signature method is to relate to people outside the ivory tower for *gaining* insights, it should not be an insurmountable obstacle to relate to people outside the ivory tower for *sharing* insights as well.

Public and academic debates would benefit if we developed more enthusiasm for writing and communicating in an accessible manner. 'It is evident that ethnographers write' (Kalthoff 2013: 272), but let us talk more about *how* we write, and let us not forget *for whom* we write. We write not solely for, but also not least for, the recipients, who deserve the respect of being offered a readable text. Our insights are too valuable to be circulated only amongst ourselves.

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'On a Hot Day in the Field . . .' The Art of Writing Ethnographic Vignettes

Svenja Schöneich

Introduction

The engine of the bus suddenly starts roaring and a thick cloud of exhaust fumes rises into the air, increasing the sweltering heat that engulfs me. I hastily grab my backpack and squeeze through the narrow doors. As soon as I have taken a seat, the bus starts to move. I stand up and wave goodbye to the people who accompanied me to the station. I was part of their community and even their family whilst staying with them during my field research, and now I suddenly feel a painful melancholy. I look at my tightly filled backpack containing the tangible yield of my fieldwork. Interviews, maps, copies of historic documents, survey forms, notebooks full of fieldnotes, and many, many photographs, all waiting to be sorted, analysed, and eventually made sense of. I have already ordered large parts of my material, but now the hard part begins: transforming dialogues, ideas, memories, and research protocols into a dissertation. Thinking about the months of writing ahead of me, I already feel like I am beginning to lose touch with my research site. Fortunately, I have my fieldnotes, which will help me remember and get back into particular situations. I remember some of the key moments during the field research when I realised certain connections, or I suddenly found explanations for things I had not understood before. To write up the text which will become my dissertation, I will have to think about possible ways of integrating these situations into the narrative. The people who came with me to the bus stop have disappeared from view and I sit down to enjoy the draft of fresh air from the open window. I think about my experiences in the community and start visualising some of the moments that helped me gain insights as the bus rumbles down the dirt road on the way to the city and away from what has been my home during the fieldwork.

Ethnographic meaning is something that emerges within what Jasmin Mahadevan (2012: 119) identified as an ethnographic triangle. This triangle consists of researcher, field, and audience. To generate meaning, the writer needs to invite the audience to share the experiences made in the field, which enables the reader to properly understand and partly retrace the analytical processes that led to the results. Many authors increase accessibility to their papers or books by integrating small descriptive passages into their analytical text. These 'vignettes' should serve not only as adornment but should support the analysis by making it more accessible to the reader. Vignettes come

into being as evocative little stories, illustrating the theoretical approach in a rhetorically effective way (Erickson 1986: 150). In his textbook on qualitative methods and writing, Frederick Erickson (1986: 140–150), for example, defines vignettes as ‘a vivid portrayal of the conduct of an event of everyday life, in which the sights and sounds of what was being said and done are described in the natural sequence of their occurrence in real time’. Many scholars who engage with ethnographic writing consider this definition comprehensive and accurate and often refer to Erickson’s discussion of vignettes (see, for example, Wilks 2004; Humphreys and Watson 2009; Jacobsen 2014; Jarzabkowski et al. 2014; Kandemir and Budd 2018).

Through vignettes, the audience is invited to witness certain situations in the field. Vignettes illustrate moments of insight that then serve as the basis for the subsequent abstract analysis. These moments might appear unspectacular at first, describing brief encounters or everyday occurrences. Yet, they are generally events that had an important impact on the generation of knowledge. Rosalie Stolz (in this special issue) calls them fleeting moments and suggests that they can be captured particularly well by narrative elements in ethnographic writing. Vignettes are one such narrative element, allowing these fleeting moments to be illustrated as a story. Vignettes can thus contribute to conserving ethnographically rich moments of fieldwork for the reader (Stolz this issue). They also assist with finding a balance between avoiding a crisis of representation in the narrative and producing a long self-reflective essay (see, for example, Erickson 1986: 150; Jacobsen 2014: 41; Jarzabkowski et al. 2014: 280; Miles and Huberman 1994: 83; van Maanen 2011: 132).

Even though vignettes have become an integral part of contemporary ethnographies, the process of developing them is not always explicitly explained or reflected upon. This paper addresses ethnographic vignettes as stylistic devices as well as analytic tools that help the reader comprehend the conclusions presented by the author. First, I reflect on the different uses of vignettes in ethnographic writing. I then elaborate on different forms of vignettes and their position within a text by presenting examples of texts where vignettes have been used. In the third section I propose some practical advice on how vignettes can be developed and written up and finally wrap up the paper with some final reflections.

Using vignettes in ethnographic writing

Vignettes can be used in different ways, depending on purpose and scholarly discipline.¹ In ethnographic writing, however, vignettes display real events

1 Unlike in ethnographic writing, qualitative social science uses vignettes as research method: researchers construct little stories that are integrated into surveys or qualitative interviews and shown to interviewees as fictional scenarios on which to reflect or comment. They can also serve as entry point to

which occurred in the field, usually inserted into passages where the author emphasises a certain analytical insight. Another common moment when vignettes are employed is when a new topic or thematic section is introduced, as in the opening paragraph of this article (see also Jacobsen 2014). Ethnographic vignettes generally describe something that happened in a certain moment. The boundaries of the event are usually not clearly defined. Vignettes can represent, for example, dialogues, a solitary instant of reflection, or a description of an event with many protagonists. They thus function as *narrative windows* through which the reader becomes a witness to the events. To do this, vignettes often employ the 'momentary style' (Erickson 1986: 150) of writing that aims to give the reader a feeling of being present. I, therefore, suggest framing vignettes as *narrative scenes* one would expect to read in a novel or to see in a theatre or movie, albeit in prose.

Etymologically, the word *vignette* denotes an architectural ornament, usually of leaves and tendrils, and this is a second form in which researchers may employ them: as adornments to their texts (Dumont 1992: 1). Erickson (1986: 150) argues that the stylistic form of the vignette goes back to the training in rhetoric in ancient Greece, where orators were encouraged to include descriptive passages in their speeches to persuade audiences that the orator was speaking the truth about real events. This purpose is similar to that of the vignette in contemporary ethnographic texts. Erickson (1986: 150) ascribes a threefold function to this use of the vignette: 'rhetorical, analytic, and evidentiary'. First, by pulling readers into the scene, the vignette captures their attention and presents an interesting and at times refreshing anecdote within the academic text to function as practical illustration. Thus, the vignette serves a rhetorical purpose. Second, the vignette exemplifies the underlying, more abstract analytical concepts of the text and thus serves an analytical purpose. Third, the vignette bears witness to the ethnographic setting, as well as the analytical assumptions the author presents as the truth. In this function, it fulfils an evidentiary purpose (see also Jarzabkowski et al. 2014: 276).

The vignette also supports a certain didactic approach of letting the reader come to the same analytical conclusion as the researcher did. Vignettes can thus enable the reader's co-reflection on the data, even up to the point of acting as 'co-analyst of the study' (Jacobsen 2014: 41). However, even though the idea of the reader becoming the researcher seems tempting at first, the analytical effort is still the task of the author. Vignettes represent little stories that can be explored by the reader, using the analytical tools provided by the author. In this manner the reader does not merely need to follow the author's conclusions but can retrace the steps on their own. To allow the reader to do

complex research questions or a new topic section (for example Barter and Renold 2000; Hughes and Huby 2004; Jenkins et al. 2010; Kandemir and Budd 2018).

so, vignettes should not be presented without contextualisation. They should thus complement, not replace, the analytical narrative (Erickson 1986: 150).

Vignettes in ethnographic practice: their role, position, and length

How authors approach the endeavour of writing vignettes often differs according to their individual style and the objective of the narrative. One of the first and most prominent examples of the usage of narrated scenarios with an analytical purpose is Max Gluckman's essay 'Analysis of a Social Situation in Modern Zululand', which was published in two parts in 1940. In this essay, which became known as the 'Bridge paper', Gluckman describes and analyses various events – which he calls *social situations* – that took place on a single day in 1938 in what was then northern Zululand. What has become a classic in the anthropological repertoire at the time represented an important and novel methodological contribution to anthropology (Cocks 2001: 738). How Gluckman defines a social situation is very similar to Erickson's definition of the vignette. He states: 'They are the events he [the anthropologist] observes and from them and their inter-relationships in a particular society he abstracts the social structure, relationships, institutions, etc., of that society' (Gluckman 1940b: 150). Yet Gluckman's example does not strictly qualify as a typical ethnographic vignette in the contemporary sense due to the role the described situations play in his analysis. He virtually dissects each part making up the events, constructing his findings based on what happened in each described scene. Where vignettes usually illustrate findings that are the result of a more comprehensive analysis, including additional data, Gluckman placed his scenarios at the centre of his text (see Humphreys and Watson 2009: 46).²

Another difference between Gluckman's social situations and vignettes is the question of length. Gluckman's detailed description of a social situation takes over a large part of the text and thus acts as its main component rather than as the useful, yet minor illustration that the vignette usually provides. Gluckman first presents a scene in length and then engages in a detailed analysis. A good vignette, in contrast, should be understood as a *miniature version* of a social situation that enables the reader to retrace the author's insights through the narrative but does not represent the main text. There are other examples of the vignette that resemble Gluckman's approach, in regard to length and position. In *Visayan Vignettes: Ethnographic Traces of a Philippine Island*, Jean-Paul Dumont (1992: 1) presents an account of his fieldwork through 'a plurality of images – vignettes – that superimpose

2 Gluckman's (1940a, 1940b) approach later became known as the situational analysis or extended-case method, which was then further developed by the Manchester School. A situational analysis represents the first step of ethnographic analysis, based on the paradigm of arriving at the general through the dynamic particularity of the individual case (Evens and Handelman 2005: 1).

themselves upon each other'. He interprets them as 'fragmented realities' (Dumont 1992: 1) which he renders accessible to the reader as narrative bits of text. His approach thus also centres the analysis around rather lengthy fieldwork vignettes.

Often vignettes are used right at the beginning of a text to introduce the topic and let the reader dive right into the issue, as I did for this paper. Alice Jacobsen (2014: 37) similarly uses a vignette to represent a 'close up of actors and interactions' to start her text. Vignettes can, however, also be positioned later in the text to support the author's previous analytical conclusions. Tom Perreault (2015), for example, places two vignettes of individual scenarios in the last third of his essay on public consultation processes in Bolivia, following a discussion of the legal and situational framework and his analysis. He thus uses the vignettes in an illustrative way later in the narrative to support his argument. These examples illustrate how vignettes can vary and take different roles and positions within ethnographic texts. But how can vignettes be constructed and written up?

From fieldnotes to vignettes

As stated before, the main sources for vignettes are the experiences of the researcher in the field.³ When returning from the field, an anthropologist has usually collected several different types of material in the form of interview transcriptions, observation protocols, and fieldnotes. Vignettes, by contrast, present already processed data, where particular situations are selected and carefully described in order to embed direct quotations into broader narratives or put them into context. The main source of such data are usually fieldnotes (Erickson 1986: 150; Ghodsee 2016: 58; Jarzabkowski et al. 2014: 277).

Much has been written on fieldnotes during ethnographic fieldwork (for example, Emerson et al. 2011; Hammersley and Atkinson 2007; Hoey 2014; Kalthoff 2013; Wolfinger 2002). Erickson (1986: 150) describes the vignette as 'a more elaborated, literarily polished version of the account found in the fieldnotes'. Yet not all fieldnotes will become vignettes and the first step is to carefully choose the fieldnotes that are suitable for this purpose. The crucial part for writing a vignette is the process of converting the fieldnotes into meaningful text. Fieldnotes are usually written down immediately after a situation has occurred and contain many bits of text which must be modified or cut out to turn them into vignettes (Ghodsee 2016: 33; Hammersley and Atkinson 2007: 196–197). In the first place this means removing abbreviations, explaining certain concepts, and adding or expanding background information from one's own memory as long as it is fresh (see Erickson 1986: 150–151).

3 In some exceptional cases vignettes have also been used as a method for autoethnography (see Humphreys 2005).

Whilst Kristen Ghodsee does not use the term *vignette* in *From Notes to Narrative* (2016), her detailed instruction on how to incorporate descriptions of events and dialogues into the text reads as practical advice on how to construct vignettes. According to her, the task is to choose a suitable vignette from the notes, depending on which aspect of the analysis is represented, emphasised, or illustrated (Ghodsee 2016: 58). This requires interpretative work. Before the most appropriate vignette can be identified, the author must have established an interpretative account that is to be communicated to the reader (Jarzabkowski et al. 2014: 277–278). Once this theoretical approach is determined, a situation can be identified through which that particular insight was gained in the field and the style and length of the final vignette can be determined. To do so, the author should go back to the fieldnotes and search for a scene which represents that topic and results. Subsequently, decisions on details and style have to be taken. For a dissertation of several hundred pages, a vignette can well be several pages long, whilst one paragraph might already be too long for a brief research paper.

There are more questions that need to be asked. Should, for example, the vignette contain dialogue? In the vignettes in my own dissertation (Schöneich 2020), dialogue is often part of the described scene and should therefore be integrated into the vignette to make it understandable. If audio recordings of a certain situation exist, the vignette can draw on these to help the author stay closer to the original wording. Should, to pose another question, the vignette contain a form of inner monologue? In that case, self-reflective notes on and memories of personal impressions in the field can be helpful. If the researcher has kept a personal diary during the field research, it is possible to look there for inspiration. Finally, in a third question, could the vignette include a photograph, perhaps to support descriptions of place or landscape? The complexity of an event contains more information than a researcher could possibly ever write down; one which becomes more concise during the interpretive process. Written descriptions might highlight some features and omit others, depending on the analytical message the author wants to convey. Also, descriptions in vignettes are generally much denser than fieldnotes and do not provide the large amount of background detail that the field notes contain. Here photographs could indeed be of assistance to capture aspects that words could not do justice to.

Final reflections

The art of writing reflects a writer's personal experiences and is thus shaped by their own writing style. Vignettes allow for creativity and increase the readability of the often rather sober academic papers. However, it can also pose a risk of allowing a text to drift from an academic analysis into an anecdotal description.

After returning from fieldwork, memories soon become anecdotes the researcher tells colleagues and friends. They become stories worth telling. In my own experience of writing ethnographic texts, there is often that story that one really wants to tell the reader, even before one's analysis is complete. It may then happen that, after examining the data and identifying new analytical and theoretical connections, this specific story is not significant anymore; the vignette, intended to illuminate a particular situation, is no longer suitable for illustrating the analytical contribution. This can happen when the contribution itself has changed or when the researcher realises that a certain event was not as enlightening as originally assumed. At the same time, other fieldwork memories may appear in a new light. At this point it can be helpful to return to the interviews and fieldnotes. Sometimes other situations come to mind that are much better suited as vignettes than the one originally considered as insightful. Considering all these aspects, vignettes significantly enrich a text because they provide not only a personal note but also an ethnographic detailing of the theoretical contribution, thus improving the comprehensibility of the ethnographic text.

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'The Disease will Come!' Contingency, Irony, and Challenging Closures in Ethnographic Writing

Rosalie Stolz

The man had something *very important* to tell us, he said. Ma Man¹ and I were sitting on the veranda of a wooden house, taking a brief rest from the excitement of the wedding feast that was going on downstairs, when this middle-aged man approached us. He climbed up the stairs to the veranda and sat down next to us on a low stool. As the wedding was taking place in another village, we did not know him, or, in fact, any of the other guests, except for the groom's brother and a few emigrated kin from our village. In a rambling and animated monologue, the man recounted that a relative of his, who lived in the neighbouring province of Bokeo, had been told by a monk from a temple in the town of Mueang Sing (in the northern parts of our province of Luang Namtha) that there was a way to avoid contracting Covid-19 that was slowly but surely encroaching on Laos: one should eat a boiled egg *today!* Every person should eat one egg, not tomorrow, not the day after tomorrow, but today, he repeated emphatically. He held his mobile phone to my ear and wanted me to talk to his relative, so I could receive further evidence for his claim, but nobody answered. I was wondering what Ma Man, my closely-related companion, was thinking about this man and his proclaimed momentous message. She seemed to take an interest in what he was saying, yet I assumed that she listened to the words of a drunken man from a foreign village mainly out of courtesy. In other words, I related the man's enthusiasm and Ma Man's attention primarily to his inebriated state and her politeness but did not give much thought to *what* he said – erroneously, as it turned out later.

This brief encounter took place in early February 2020 just as the Covid-19 outbreak in neighbouring China was attracting increasing attention in local media coverage. What I want to focus on here is the role that such brief and fleeting encounters play in reorienting the fieldworker's attention to topics and developments she did not expect or of which she was not initially aware. It is this initial unawareness that I want to focus on in my contribution to this special issue on 'How to Write Ethnography': becoming explicitly aware of social situations that have sparked one's attention reveals anthropological fieldwork as an encounter and social process in which ethnographic knowledge production is entangled in the sociality of the field. That which is the typical stuff of backstage stories, floor talk, or ad hoc lessons in teach-

1 All names have been replaced by pseudonyms.

ing can have its place right in the centre of established ethnographic writing genres (see Gottlieb 2016).

Beyond a neat repertoire of ethnographic methods, various knowledge processes in the field are triggered by brief encounters, impressions, and non-discursive processes that are hard to pin down and difficult to translate in written form (Hastrup and Hervik 1994; Nielsen and Rapport 2018). It is the anthropologist's quest to try to make sense of them. Of course, the knowledge gained through formal methods and that acquired from informal sources is mutually enforcing. Narrative forms of ethnographic writing might be able to mediate knowledge and perspective whilst showing the richness of experiences from which this knowledge is drawn. Carole McGranahan (2020: 1) emphasises the epistemic relevance of writing: 'We figure things out in part by writing about them. Writing is thus not merely the reporting of our results but is as much process as product'. Kirsten Hastrup and Peter Hervik (1994: 7) point to the connection between anthropological knowledge and stories: 'While the scope of anthropology lies beyond the retelling of local stories, these and their experiential grounding remains the foundation of anthropological knowledge'. Writing the field, we could say, is to some extent thinking and memorising the field.

Literature on ethnographic writing, as Julia Pauli points out in the introduction to this special issue, is relatively scarce when compared with the number of publications on anthropological field methods. Recent years, though, have seen an increase in the number of publications on this subject. Several authors call for more explicit training on ethnographic writing in anthropology and graduate programmes or describe the advantages of mastering such writing (Gottlieb 2016). Others engage with literary anthropology (see the contributions in Pandian and McLean 2017; Waterston and Vesperi 2009) and with local forms of storytelling in ethnographic writing (see, for instance, Cruikshank 2005). Some advocate experimenting with genres and expanding the limits of conventional forms of writing (Gottlieb 2016; Wulff 2016). Others call for a more conscious narrative approach, tied to the ever-growing demands of a competitive publication market where stories sell (Gottlieb 2016). In an article on how to write for publication, Niko Besnier and Pablo Morales (2018: 169), former editors of *American Ethnologist*, point out that anthropologists 'have capitalized on storytelling techniques since the reflexive days of the 1980s'. Guidebooks such as Kristen Ghodsee's (2016) *From Notes to Narrative* give detailed advice on constructing plots, describing scenes, and portraying an interlocutor's mimics. Ghodsee suggests capturing details with which to form convincing and rich ethnographic narratives:

You might find it difficult to write about people that you don't know [. . .]. But you must endeavor to capture specificity in even the most fleeting encounter. [. . .] Describe gesticulation

and posture. Remember Sherlock Holmes; he could surmise the most intimate details of people's lives by small clues in their dress or behavior. (Ghodsee 2016: 38)

Like the famous investigator, one should find and present clues of people's selves and lives. Whilst the call for ethnographic detail is suggestively posed and this famous investigator is merely referred to for illustrative purposes, the figure of Sherlock Holmes in fact stands in vivid contrast to ethnographic practice: the aloof and socially detached detective is the exact opposite of the socially enmeshed participant observer.

Brief encounters such as the one above might appear fleeting and contingent and may easily be forgotten if not recognised as noteworthy and jotted down in the field notes. Some, however, might hold a revelatory or transformative character or, as is the case here, trigger the anthropologist's attention (at least at a later point). Whether one calls them fleeting moments or chance meetings, such encounters can come in many different guises: what I wish to point out is their contingent character. Contingency is often alluded to in works commenting on the 'partial truths' (Clifford 1986) that anthropology can come up with, the 'provisional' nature of ethnography (Cohen 1992, cited in Rapport and Nielsen 2018: 200), or the historical contingency of phenomena. There are different ways in which ethnographic encounters can be described as contingent (Holmberg 2012: 100), amongst which the social contingency of encounters (developing in a field of social relations) and contingency as outcome of serendipity or fortuity are relevant for the present undertaking. The chance in contingent encounters lies in their potential to bring unforeseen topics to one's attention. What is of particular interest for this contribution is the potential of such encounters in ethnographic writing: drawing on a brief ethnographic case from recent fieldwork amongst the Khmu in northern Laos, I argue that writing about contingent encounters can highlight how encounters unfold in the field, that they can be part of sociality, and that they can assist the anthropologist in making sense of the field.

The run on eggs

The brief encounter with the man was quickly pushed aside by many other conversations Ma Man and I had during the wedding. It was brought back to my attention, however, in the evening, once we had returned to our village. Ma and Yong Khwaay, with whom I was staying at the time, and I had just started our evening meal when Ma Man arrived. Holding a plastic bag with eggs, she adamantly demanded that I come to her house to eat. Taking care of close kin is indeed common, often expressed through meals to which members of the same house-group (*muan kaan mooy nuay*)² are invited. But her

² If not indicated otherwise, all foreign language terms are in Khmu. The transcription of Khmu words largely follows Svantesson et al. (2014).

persistence was certainly unusual. Ma and Yong Khwaay hid their curiosity as far as possible. I ate a few more spoons of vegetable soup but then decided to follow Ma Man. I apologised, left, and went across to her house. I assumed that she wanted me to eat with her after having spent two whole days in each other's company.

When I entered Ma Man's house, she handed me a boiled egg and demanded I peel and eat it, right then and there, and alone. This was indeed unexpected. Eggs are not eaten whole or by one person; they are served mashed in a bowl and shared by all, each person taking a few bits to add to a ball of sticky rice. But then Ma Man reminded me of what the man had told us during the wedding feast and determined that each of us had to eat an egg today; in fact, she had already eaten hers. I smirked and said: '*Kulaak*'. This is a widely used term to indicate that one believes a statement to be false. Her brief reply: 'Who knows' (*muah niŋ*). A number of thoughts crossed my mind: if there was a chance that this man was right, we had better eat an egg today; all Ma Man was doing was taking care of me; and a boiled egg – dipped in chilli sauce – is a delicacy one should not pass over. And so I ate the egg. Naan, a young mother of a baby boy who married into our village and regarded Ma Man 'like a mother' (*muam ma tee*) was curious to hear about the prophecy and Ma Man filled her in, with a tone of conspiracy, as we sat at the low table in front of the kitchen, ate the remainders of the evening meal, and chatted.

On my way back to the house of Khwaay, I briefly stopped at the fireplaces in front of some of the neighbouring houses. It was the cold season (*mong hrnim*), when the temperature falls markedly after sundown, and everyone was trying to find warmth, both the physical and the social one, sitting with kin and neighbours around log fires. At one of these, the congregated neighbours asked me for news from the wedding, the village where it took place, and the people who had attended it. Just as I was finishing the odd story of the man's prophetic hearsay account, Teen Sen, one of the women assembled, received a mobile phone call. She became increasingly upset and, when she hung up, instructed an older child to hurry to the kiosk to buy eggs. Her husband, who was staying on a plantation for wage labour, had warned her that 'the disease is coming' (*pnyaat cə rɔɔt*).³ To prevent becoming its victim, he stated, one should eat a boiled egg that very night at nine p.m. and so he instructed his wife to prepare eggs for their much beloved adoptive daughter and herself. Her sister, Teen Khæew, asked why she was not told so earlier and clearly also wanted to follow this advice. Though many of the neighbours assembled around the fire were laughing at this story, the phone

3 The word *pnyaat* is a Tai/Lao loanword. Amongst the Khmu, it is commonly used for chronic diseases that have a more or less clear diagnosis and for which biomedical treatment is sought (often complemented by consultation of local healers). For common pains and malaise, irrespective of their cause, the word *cu* is used.

call clearly gave the prophecy more weight. It was at this point that I began to wonder whether the prophecy might not in fact already be a widely circulating rumour (now with the added ‘before nine p.m.’ specifying a particular time). I was used to rumours and circulating stories, especially with regard to kinship- and spirit-matters (Stolz 2018), but what appeared quite novel here and what caught my attention was the speed by which the rumours about an approaching disease, or a calamity of any kind, were spreading and that they were doing so via mobile phone.

When I arrived back at the fireplace at the house of Khwaay, I told the people seated there about the prophecy but earned only a few sceptical glances. Yet, before we had even finished with the topic, we heard the loud voice of Ma Kham as she hurried to the kiosk located opposite our fireplace and asked the shopkeeper’s elder son for some eggs. This was followed by roaring motorbikes and adolescents arriving at the shop, most likely sent by their mothers. Like an audience at a play, we observed the spectacle of the sudden run on eggs unfold in front of our very eyes and to our great amusement. Ma Phoon, another member of the house of Khwaay stood on the veranda facing the kiosk and shouted to the small but growing crowd that if one truly wished to avoid the disease one should not eat eggs but rather stop working on the Chinese melon plantations. An argument ensued, with Ma Kham insisting on the truth of her claim and the shopkeeper’s son challenging it. But what kind of a shopkeeper would he be if he would not take advantage of this situation?

As we sat around the fire, we joked that this misinformation had been spread by no one else than the shopkeeper herself in order to sell the eggs from her growing chicken farm and commented on how the price of eggs had quickly increased from K1,000 to K5,000.⁴ Would the excitement dissipate as soon as it had come? Was it a triviality that would not even be talked about the next day? There was a wedding negotiation planned for the next evening that was at the forefront of the attention of everyone in the house of Khwaay. The ritual procedure for it had been innovated, and everyone was concerned about what to expect and how the new procedures regarding the ‘entering of the room’ would compare with the previously used ones. As the evening progressed, however, it became evident that everyone knew about the approaching viral disease and the egg antidote. Teen Maa stated deadpan that now those of us who had failed to eat an egg were doomed to die. We laughed even more when he quipped that this healing power of eggs meant we did not need hospitals anymore! The rumour indeed increased not only the sale of eggs but also the number of jokes circulating: with whomever one talked that night and the next day, boiled eggs were omnipresent.

4 The Kip (K) is Laos’s currency. At the time of the fieldwork, around K10,000 were worth €1.

The story seemed to have its source in a WhatsApp post, later repeated on Facebook (or 'Fatebook', as it is pronounced in Lao and Khmu) where advertisements by local shop owners with special offers for eggs proliferated. The daughter of the shopkeeper in our village showed me the initial post on her smartphone (Figure 1). According to this post, a newborn baby had miraculously spoken and warned its parents of the impending coronavirus disease. It then announced to them that boiled eggs eaten on 6 February would act as antidote. The post ended with the call: 'Give each person a boiled egg. Eat it today. This is the 6/2/20. Boil the egg and eat it!' Whilst the baby's words were not repeated in the accounts that circulated in our village, the part of the egg was taken literally. A few days later, Yoŋ Hak said he had heard that the police were searching for the person who initially published the post and had caught a young man who, as far as this rumour went, sold eggs in the provincial capital Luang Namtha.

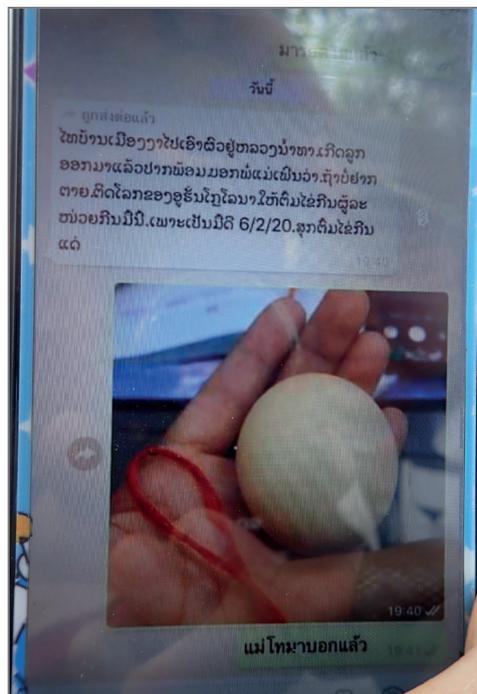


Figure 1: Photograph of the post on a newborn's prophecy of the impending coronavirus disease and the antidote effect of boiled eggs that was allegedly the original source of a run on eggs in northern Laos in early February 2020. Photograph: Rosalie Stolz

Unfolding the run on eggs

The conversation with the man at the wedding took place during a field stay in early February 2020. The main aim of my fieldwork was to expand on previous research on the changes of building practices, especially the use of concrete, in this rural upland Khmu village in north-western Laos

(Stolz 2019). The Khmu are Mon-Khmer speakers. Their livelihood is based on shifting cultivation, the collecting of vegetables, and hunting and trapping, and increasingly includes wet rice agriculture and cash crop production (Évrard 2006; Tayanin 1994). A tradition of temporary labour migration is now complemented and partially replaced by wage labour on often Chinese-run plantations in the wider region. Kin-based sociality is very much predicated on the difference of relations amongst kin group members and the hierarchical wife-giving/wife-taking ties between kin group (Stolz in print).

Although the events around the prophecy actually happened in rapid succession and in the sequence described above, their selection is based purely on chance: we happened to be in this village where this man happened to confide in us; it was a coincidence that I was sitting at the fireplace when Teej Sen received her husband's phone call; and it was serendipitous that I stayed at a house located directly opposite the kiosk. I am sure there were many more conversations about eggs at log fires in the village and other telephone calls that I did not become aware of. Yet the story as told here is not the mere outcome of a narrative strategy: it is part of how I made sense of and remembered what happened. Already during the various encounters, they appeared to condense into a story, and my field notes as well as my 'head-notes' – those 'impressions, scenes and experiences' that are not jotted down (Ottenberg 1990: 144) – already had a narrative imprint. In its narrative form the meanings, but also the contingencies surrounding the reactions to the approaching coronavirus disease, drew my attention – showcasing aspects that were already familiar to me through previous fieldwork but also pointing me to issues previously unknown to me.

This instance of a story spreading via WhatsApp in this rural, upland place made me aware of how new media have entered local life over the course of a short period. Whilst smart phones were a rarity during my fieldwork in 2014/2015 (though simple mobile phones did exist by then), now socialising on Facebook or WhatsApp is a cherished pastime amongst the village youth – though they have to bear with annoyingly slow speeds. The WhatsApp post that was regarded as the source of the rumour spread furiously on the evening of 6 February. Though the post was a stepping stone, the usual avenues of spreading news and stories were also used. Novel was that the information spread so rapidly, connecting far-flung places and people such as a monk in Mueang Sing, a relative in Bokeo, the purported author of the post in Luang Namtha, Khmu labourers on plantation sites, their kin in various Khmu villages, and an unknown number of further recipients and spreaders. The story was also shared beyond the provincial borders of Luang Namtha: my colleague Oliver Tappe, who at the time was staying in the north-eastern province of Huaphan, reported that he had heard the story there too. It even

entered Lao news coverage.⁵ The knowledge that the message came from elsewhere and received attention across the province seemed to have produced a sense of urgency and thus accelerated the speed with which the story made the rounds. Yet, the reason *why* it spread amongst my Khmu interlocutors so rapidly seems to be related to the specific matter of the eggs.

Rather than the baby's prophecy, which was unknown to most of my interlocutors, what seemed to contribute to the story's persuasiveness was the curing power assigned to eggs. Eggs (*ktor*) are not any foodstuff: they are symbolically highly laden, evoking the image of the healthy unity and wholeness of entities. The role the story accorded to eggs, thus, rang true for local recipients of the rumour as it did for me as I was observing the run on eggs and tried to put it into narrative form. Eggs, I had learnt during my earlier research, are part of the ritualised gift exchange between wife-givers and wife-takers: at a certain point during the marriage exchange wife-takers give gifts of eggs to the wife-givers; at some point these gifts are then 'answered', when the wife-givers hand over gifts of dried squirrels or rats to the wife-takers (Stolz 2020b; Sprenger 2006: 154). In the context of this gift exchange, the gift of the egg mediates acts of mutual recognition amongst kin.

Eggs are also used as ad hoc healing items to prevent souls from getting lost. Illness, exceptional emotional states, travels, and accidents can loosen the tie between body and soul, with the effect that the soul wanders around and can get caught by ever lurking spirits who might abduct them to the spirit land. Children's souls, less firmly tied, are assumed to get lost and fall prey to spirits more easily. This is when the ritual of 'calling back the souls' is conducted, mainly though not always for child patients. During this ritual, a boiled egg and the incantation of verses by the healer are used to attract the wandering soul. At the end of the ritual the patient consumes this boiled egg. It is not only true for the Khmu but also for the Tai that the egg as 'a symbol of life in embryonic form' (Ngaosyvathn 1990: 294; Collomb 2008: 160) plays a crucial role for the reintegration of the soul into the body. Eggs contribute to well-being, both in terms of binding the soul to the body and cementing the relations between wife-givers and wife-takers. Thus, the rumour of eggs as antidote against the coronavirus appeared to resonate with local understandings of eggs as healing and generally socio-ritually significant food.

5 In an article in the *Laotian Times*, Francis Savankham (2020) compared the account of the (prophetic) powers of a newborn with the birth story of King Fa Ngum – founder of the Lan Xang kingdom in the fourteenth century who was miraculously born with thirty-three teeth – and that of Sang Sinxay – a figure from a famous epic poem who was born with bow and arrows. Interestingly, it is not because of this association with these legendary figures that the egg story caught the attention of the Khmu of Pliya and elsewhere.

Mocking the rush on eggs

Recognising the healing potential of eggs might lessen the peculiarity of the story of anti-coronavirus egg eating. Yet it would be far from correct to state that my interlocutors actually assumed that eating boiled eggs would really prevent an infection. At the time when I captured the run on eggs in my field notes, Covid-19 had not yet been declared a pandemic and I felt increasingly uneasy: do I expose my interlocutors to ridicule when I present them as ostensibly susceptible to peculiar rumours about eggs as immunisation against the coronavirus? But, just as much as the rumour led to frantic attempts to source eggs at the local shop, it also evoked a spontaneous voicing of disbelief, scepticism, and mockery. When I reconstruct the unfolding of events in my head, it is not only the initial encounter with the man at the wedding but also the voices mocking the rumour that form part of the picture.

The rush on eggs led to a proliferation of laughter and ridicule – even amongst those who made an effort to eat an egg. A young teacher at the local primary school, who herself had grown up in the village, admitted with a self-deprecatory smile that she had taken her motorbike that night and driven to the district town to find enough eggs for her large house-group. Self-ridicule, mocking, and joking are established repertoires of local talk and part of what makes social intercourse enjoyable. She knew right away that the claim about the egg antidote might be nonsense but she nevertheless wondered, ‘what if there is something to it?’ Irony, Hans Steinmüller (2016: 2, 5) argues, ‘refers to an incongruity between an appearance or an utterance, and that to which it refers’; it ‘emphasize[s] openness and contingency’ (see also Fernandez and Huber 2001). The latter differentiates irony from cynicism, as ironical statements or gestures (such as an ironical smile) ‘leave space for interpretation, whereas cynicism closes this space and implies radical criticism, denial, or resignation’ (Fernandez and Huber 2001: 2).

This openness of meaning, this persistence of ambiguity is perhaps particularly vibrant with regard to elusive phenomena such as spirits (Willerslev 2013). Ironical statements that allow the inclusion of contingency, what is otherwise, and doubt are part of local discourses on spirits but also on imminent social changes (Stolz 2019). Yet, *self*-deprecatory statements carry a singular importance here. Showing one’s ability of self-deprecation and joking about oneself is part of humorous talk and, in some situations, ritually prescribed (for its role in traditional songs, see Lundström 2010: 157). In the story of the eggs both come together: self-deprecatory gestures and comments in relation to the run on eggs express the speaker’s inadvertent epistemic uncertainty about the rumour’s content and their inconclusive stance towards it. Again, the rumour could be nonsense, but what if it is not? Ma Man put this dichotomous ‘double-think’ succinctly when she uttered, ‘who knows’: it is quite likely that eggs will not help, but it is worth a try.

Those who were more sceptical let themselves be entertained by the events and scoffed at them. Teenj Məə jibed that with the eggs hospitals would no longer be needed. This mockery, different from self-deprecatory statements, does not leave space for Ma Man's double-think but rather exposes the seeming dichotomy on which it is founded. The mocking comments entail a moment of critique. Ma Phəən's comment falls into this category when she scolded those rushing to the shop in search of eggs to rather quit their work on the Chinese-run plantation site if they were concerned by the virus. Watermelon plantations have been mushrooming in upland Laos, attracting labourers from far and wide. At least since 2014 a plantation has been in operation in walking distance from the village on fallow wet rice fields for the time between cultivation periods. The wage of K50,000 per full working day is an incentive for large numbers of villagers to give it a try, including mothers whose children are at school or cared for by their grandparents. Although many feel ambivalent about the working conditions on the plantation and the harmful effects of the chemicals that are widely in use, the mushrooming number of concrete houses being built in the village gives evidence of the wages that people rely on. The general sense that *khon chin* (*the Chinese*) might take advantage of Lao soil and labour was now being topped by the feeling that they brought 'their disease' to Laos. Ma Phəən's comment unveiled this ambivalence: everyone is afraid of contracting *pn̄yaat chin* (the disease of the Chinese) but still enters their service on a daily basis. Ma Phəən's critique was raised smilingly – she too frequently works on the watermelon plantation.

Contingence and co-presence in ethnographic writing

When I was invited to write a contribution to this issue on ethnographic writing, I instinctively decided to focus on the vignette of the boiled eggs against the coronavirus. But why? I have asked myself during the revisions. One reason why I hesitated was the above-mentioned worry that this story could misrepresent my interlocutors as naïve country bumpkins who believe fake news spread on WhatsApp. More critically, however, I wavered because, as it took, the story did not have an immediate effect on me, did not force itself on me to alter or trigger a new understanding of sociality in the field. Despite this, I have always retained the feeling that there is something to the story – something that perhaps illustrates particularly well some of my concerns with ethnographic writing. These concerns are about the role of contingency, the related problem of co-presence, and the insights that the comical can offer.

Let me begin with contingency. The story of the run on eggs began for me with the brief encounter with the man at the wedding. It was merely by chance I was sitting on the veranda with Ma Man when this man approached us. Not particularly noteworthy at first, the situation could have well been

drowned out by the wealth of impressions gained and conversations engaged in at a feast. Just as fortuitous was the fact that I was at the log fire exactly when Teeng Sen's husband called her and urged her to buy eggs. Similarly opportune was the fact that I was living at a house that faced the shop that was called upon so frequently that night. Like pearls on a string these snippets embedded themselves in my headnotes, and were later captured in my field notes as that which became known as 'the run on eggs'. Whilst I cannot anticipate whether this story will be remembered for a long time, what is quite typical for ethnographic encounters is the contingency of its genesis.

Contingency entails not only such a moment of chance but also the eventuality that defines the social relationships established during the fieldwork process. The way the researcher is enmeshed in social ties in the field is far from a side effect of fieldwork but bears an imprint on the fieldwork process and how knowledge is built. Ethnographic accounts often confine reference to the relational side of fieldwork to special sections, such as the discussion of the methodology employed or the acknowledgements (where the author's power to relate to prestigious institutions, grants and prominent 'names' is also displayed [see Callaci 2020]). In this, Judith Okely (1992: 5) notes, anthropologists 'produced accounts from which the self had been sanitised'. It is a divide that prevails in ethnographic writing, attracting continued calls to explicitly reflect on the relational and affective nature of fieldwork (Stodulka et al. 2019) and the social persona of the fieldworker and their company in the field (Braukmann et al. 2020; Pauli 2020; Stolz 2020a). Indeed, social contingency, or what Liana Chua calls 'co-presence', goes to the heart of ethnography: it points to 'the wider relational field through which ethnography is generated – and in which it remains enmeshed' (Chua 2015: 646). The places mentioned in the narrative presentation of the run on eggs are not any places: they are closely entwined with kin-based sociality to which I am not an unattached, or 'invulnerable' (Chua's 2015: 646), observer. Being positioned in the local web of kin allows intimate insights (into matters of relevance for close kin) but might also limit the knowledge (about distant wife-givers/wife-takers or of all those kin whose house one cannot easily enter). In any case, and this is relevant for the run on eggs: it shapes the conditions under which one encounters phenomena in the field.

The degree to which the persona of the fieldworker and her involvement in social relations in the field should enter the ethnographic narrative varies strongly and is a distinguishing mark of the different ethnographic writing styles that John van Maanen (2011) describes in his *Tales of the Field*. He identifies three writing styles ('tales' in his parlance): a realist, a confessional, and an impressionist writing style. These styles coexist and, in fact, contemporary ethnographies are still commonly shot through with all of them. Realist tales have a detached tone of description and the author does not appear as fieldworker persona; it is a style often used when providing overviews

(such as the mandatory historical or regional context) and when the author feels like 'merely' delivering information – such as when I described the role of eggs in ritual gift exchange above. The confessional style, in contrast, lives off the exhibition of the fieldworker person and her feelings, attitudes, and struggles. This is no less touched by strategic considerations and editing: a shared theme of confessional tales is the telling of struggles and trials, often during the early stages of fieldwork, and how the fieldworker managed to overcome them. The impressionist narrative style also allows a glimpse into the situation in the field but is not as obsessed with the ethnographer's self as the confessional narrative. Impressionist tales are dense and lively narratives of often dramatic or comparatively unusual events or series of events, told in a way that pulls the readers into the setting. The perspective of the fieldworker and how she is involved and impressed by the course of events forms part of the narrative, yet the focus is as much on what happened as on how the ethnographer reacted to it. Lively narratives that transport a sense of 'being there' and provide glimpses into the social backstage of ethnography, so to say, become a well-established element of ethnographic writing.

Challenging closures

Contingency and constraints apply not only to how the fieldworker relates to 'the field' but also to how other people co-present in the field and their statements and actions are presented (Chua 2015: 655). Over time, the impression one develops of one's interlocutors and companions in the field gains in complexity and so does one's sensibility towards the situatedness of accounts. Good ethnographic stories aim to express this complexity and challenge closure. As Joanna Davidson (2019: 170) convincingly argues, good ethnographic stories 'tell multiple truths; they create multidimensional characters; they refuse flattened or simplified understandings; and, ideally, they transform the listener through an intersubjective experience'. The editorial choices of composing narratives, especially when they aim at producing a sense of 'immediacy' (Gay y Blasco and Wardle 2019: 78), can be quite profound and certainly extend beyond cosmetic questions of narrative aesthetics. The decision to include, for instance, Ma Phoons exclamation from the veranda or Teen Moo's witty remarks is based on my ethnographic finding that the local articulation of ambiguity through the use of irony and mockery is not merely an epiphenomenon but is part of the story. As irony can allow for contingency and ambiguity of meaning, its capacity to challenge closure is a valuable device in ethnographic writing.

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Negotiating Authority: Experiences with an Interview Series in Montevideo, Uruguay

Mijal Gandelsman-Trier

Introduction

‘Write that down, please!’ my interview partner Teresa told me and began to carefully dictate her view of the history of her neighbourhood.

In this paper I reflect on the way in which specific experiences from my fieldwork can be transferred into ethnographic writing. I start with a short description of the situation in the field. I met Teresa – as I want to call her here – during my fieldwork on transformation processes in the port-related quarter of Ciudad Vieja, the historic centre of Montevideo, Uruguay.¹ At the time of our encounter, Teresa was an eighty-three-year-old woman who immediately showed a strong interest in my project when I called to ask her to be my interview partner and to arrange an appointment. When I prepared our first meeting, I decided to conduct a biographic interview. I knew that Teresa had lived in Ciudad Vieja all her life and that she was very committed to the neighbourhood. It seemed appropriate to ask her about her childhood memories and the changes she perceived to have happened in the district as part of her biographical experiences. Biographical interviews open up the possibility of getting to know personal experiences and insights with significant historical depth. This aspect was of particular interest to me. I also expected that it would be a useful way in which the elderly woman could recall her experiences in the neighbourhood at different stages of her life.

Yet right from the beginning, Teresa thwarted my plans and took over the conversation. I opened our first meeting with a question about her childhood in the neighbourhood. She told me that her memories go back to 1925, when she was five years old. She took this as starting point to talk about the history of the district from her point of view. Teresa had a clear picture of how Ciudad Vieja had developed over time and she wanted to convey her perspective to me. She did not seem to believe that her personal memories and experiences were of any relevance in this. She persistently refused to deviate from her style or to digress into a biographical narrative. In our conversa-

1 The ethnographic fieldwork was conducted in Montevideo between 2003 and 2005. The study was part of a research project at the Institute of Social and Cultural Anthropology, Hamburg University, on transformation processes in port cities. For further details on the research project, see Kokot et al. (2008); for further details on the research project in Montevideo, see Gandelsman-Trier (2008) and Trier (2005).

tions, there were a total of six meetings, she switched between a colloquial narrative structure and a way of speaking in which she dictated her views to me ready for printing. In each meeting, she set the topics, sometimes seamlessly taking up where she had left off the previous time. She seemed to have a clear idea of what my work would look like later.

The picture that Teresa created of Montevideo's historic centre in the twentieth century was framed by three major upheavals, which formed the structure of her narrative. She lamented the loss of public memory that had gone hand in hand with these transformations. Since the changes in Ciudad Vieja were also accompanied by a change in the quarter's population structure, she called these upheavals 'diasporas'.

The contact with Teresa was tremendously enriching for me. I was impressed by her perspective, detailed knowledge, impetus, and commitment. But this opened questions for me: How could I work with the abundance of information, points of view, and perspectives she had provided? How could I preserve Teresa's voice without either subordinating my voice to hers or letting her disappear in my ethnography? To a certain extent this is a common problem in ethnographic writing. In the case of Teresa, however, I found it particularly striking: first, because she behaved with such authority; second, because this series of six interviews formed a significant portion of my corpus of research data; and, third, because she was such an impressive person.

Although I could well comprehend the way Teresa depicted the history of Ciudad Vieja in the twentieth century, her account did not guide the way I began to think about the development of the quarter. In particular her use of the term 'diaspora' irritated me extremely. I understood it as a marker for the upheavals that had occurred but was unable to relate her use of the word to the academic concept. Therefore, I could not adopt the story as she told it as part of my account. With this decision I restored my authority over the process of writing.² But this measure alone was not convincing, because I did not want to suppress Teresa's voice: she should find a place in the ethnography where she could represent herself, at least to a certain extent. I did not want to use her detailed narrative only as quarry for data. It was thus important for me to give Teresa her own space in my ethnography. I use this expression to illustrate that the interviewee's perception should get a prominent place in my ethnography. I consider this 'own space' within my narrative flow a stylistic device that can also be applied to other interviewees.

In Teresa's case, 'own space' means telling *her* story, using her words in the form of long quotations, following her emphasis on events, presenting *her* conclusions. Teresa's story is about transformations that have taken

2 In the context of this paper I do not want to reiterate the famous debate on ethnographic authority and representation. On the subject of ethnographic authority, see, for example, Clifford (1983) or Sperber (1989); for the broader context, see the paradigmatic volume *Writing Culture*, edited by Clifford and Marcus (1986).

place in the historic district of Montevideo that Teresa calls 'diasporas' and that are linked to major changes in the physical structure of the area and the composition of the population. It was within this limited narrative space that she could speak for herself. My contribution was to contextualise her story, describe the setting, create a biographical portrait of Teresa, and introduce her as one protagonist of the neighbourhood. By joining all these elements together, narrative passages develop, which can also be denominated as vignettes, even if they are quite extensive. In the best of cases, small thick descriptions, as Geertz (1973) phrased it, emerge from those stories.

Teresa's vignette consisted of four elements that reflected on different aspects of our encounters and interactions. In the following I present the three points of contextualisation or setting, portrait, and practice. I do not go into Teresa's narrative, which I have already briefly outlined and characterised as her own space.

Context and setting

Contextualisation first of all means to introduce the reader to the particular field situation. Where and how did the encounters take place? Under what circumstances? What was the relationship between the interlocutor and the field? How did the interviewee behave? Such a description of the location can help to gain insight into the setting. This applies both to the research situation and the writing process.

During one meeting Teresa invited me to her balcony. From there a wide view of the port opened up. She told me about the past: the sounds of the port, the constant movement of people and goods. Immediately my central research topic moved much closer. It was more tangible than during our conversations in the living room. This episode illustrates that interviews and their evaluation consist of more than the analysis of the spoken words. Our understanding of our research topics arises from the interaction of our senses. We perceive many details, and they merge into, complement, and partly explain what we learn in the interviews (Ghodsee 2016: 31–39). This process should be demonstrated and incorporated into the ethnography in an exemplary way.

Within the framework of the contextualisation, small anecdotes can find a place. Teresa, for example, lived on the first floor of a house in a busy street. When I arrived for our appointments, I rang the doorbell downstairs. Teresa would open the window and throw the front door key on the sidewalk. That way she did not have to walk down the stairs, which was hard for her. I was included in her usual habit in which she received visitors. With this attitude she expressed a certain familiarity and trust, right from the very beginning. Teresa's informal behaviour of making contact with people in her environment relates to everyday practices in the local setting of the neighbourhood.

Such small facets help one get to know the interviewee more closely, at the same time as they condense an understanding of the field in the form of small mosaic pieces, however insignificant these may seem on their own. For me, Teresa's habit gained in importance because it reminded me of how other residents described life in certain areas of Ciudad Vieja as having been one of familiarity.

Biographical portrait

Biographical portraits are an appropriate stylistic device to bring a person in an ethnography closer to the readers. Kristen Ghodsee advocates introducing people in a differentiated manner, presenting them in detail – of course whilst maintaining the necessary anonymity of the person. She thus suggests: 'since personal interactions in the field drive ethnographic research, present your primary informants in three-dimensions, as real people, not as caricatures' (Ghodsee 2016: 34). She stresses the importance of locating the actors in everyday practice. Based on the focus of my research, I complement this with a localisation of the actors in the physical and imagined spaces of the district.

Teresa was a well-known and respected person in the neighbourhood. Even though I was unable to get her to give me a detailed account of her biography, I was able to gather some information about her life story in the course of the long meetings. I also learnt from other people and through written testimonies how her life had always been connected with the neighbourhood.

Teresa grew up in Ciudad Vieja and personally experienced all the phases of change in the neighbourhood that she described to me. Her life was closely interwoven with the area. Therefore, she regretted that houses and streets were torn down over time and, in her opinion, had disappeared from memory. During Uruguay's military dictatorship (1973–1985) she worked in the project group *Grupo de Estudios Urbanos* (Group for Urban Studies). One result of this work was the small book *Una ciudad sin memoria* (City without memory) (Grupo de estudios urbanos 1983). This photographic and textual survey of the situation in the historic district implied a strong criticism of the urban planning policy of the military government.

In Teresa's case it is striking that the memories told are always linked to the physical and social setting of her living environment. This applies, for example, to her early experiences in one specific zone of Ciudad Vieja that was partly demolished for the construction of the *rambla*, a waterfront promenade, as well as to her work some decades later for social services offered by the church. There she learned about the situation of people in need and was confronted with problems of unemployment, homelessness, and the life of street children. Because of her engagement, she told as an anecdote, she was able to move around the neighbourhood without any difficulties, even in

the dark. She knew many of the young people who were roaming around, and they knew her and protected her instead of robbing her.

Practice

Teresa was both a contemporary witness and an activist. Beyond everyday life in the local neighbourhood, Teresa was actively involved in the political and social concerns of the district since her youth, as the examples of her social commitment in church social welfare and her participation in the urban policy group show. It is therefore important to introduce the interviewee as an actor with her (or his) own agency. How does daily practice in the living environment look like? In what way does the interlocutor participate in the process of perception, appropriation, and transformation of the setting? Not all people intervene as directly in the process of change in their environment as Teresa, who has influenced the representation and transformation of her neighbourhood, for example through her participation in political initiatives, church organisations, and state institutions. It is obvious that Teresa has had a great interest in the renewal of Ciudad Vieja and has acted accordingly. The *analysis* of everyday life is a relevant part of anthropological research. In this context, the *description* of the practice of individuals seems a useful approach to represent everyday actions in an ethnography. Ghodsee (2016: 1) emphasises: ‘Ethnography provides a qualitative method to focus on the experience of everyday life, and ethnographers literally “write culture”. Unlike any other research method in the social sciences, ethnography revels in the quotidian’.

Conclusion

Teresa’s example stands for itself and, at the same time, serves as an example. To sum up, my approach consists of allowing a number of key informants their own space within the framework of my ethnography. In this space, the actors can, in a way, express their own views on the subject of the ethnography. In other words, I do not restrict my narrative to biographical portraits or contextual information about the persons. Instead, the selected persons, as Teresa here, are given the opportunity of telling their own stories within my larger ethnographic narrative. This allows different approaches and perspectives on Ciudad Vieja and on processes of change in the district to emerge and create ruptures and irritations. They develop persuasive power, stand for themselves, and represent the respective actors. These stories of the interviewees are at the core of the vignettes, which give the respective persons more prominence in my ethnography. The vignettes should form a contrast to my narrative: they should not get mixed up with the flow of my argument and my writing style. Although such vignettes are intended to undermine my ethnographic authority, at least to some extent, no counter-narratives

will gain access to my story. And that stylistic device certainly does not solve the dilemma of representation. As the author, it is me who writes down the stories of the interlocutors, arranges them, and positions them in the monograph. The common practice of writing about field experiences includes the problem of the representation of others in ethnographic writing in itself – a problem that is not, in my opinion, solvable. The way of writing should, therefore, address this challenge and moderate and counteract the dilemma of representation.

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Different Kinds of Storytelling: Ethnographic Writing and Documentary Film-Making

Cati Coe

I turned to documentary film-making because of my disenchantment with ethnographic writing. By this point in my career, I had written three monographs and numerous journal articles and edited several collections of scholarly essays. In some of my more recent projects, my research participants had hoped that the research would improve their lives in some ways. In a project on transnational families (Coe 2013), Ghanaian immigrant parents living in the United States (US) railed against the costs, rules, and bureaucracy of US immigration policies which prevented the reunification of kin for many years; or against the long, irregular, and poorly paid work that made it difficult to raise a child in this country, often resulting in a baby or child being sent to Ghana to be raised there by an aunt or grandmother. In a project on African home care workers in the US, a home care worker from Guinea expressed anger at the agencies for their exorbitant profits by which her patients paid a lot for her assistance, but she received only half of it. She was only willing to be interviewed because she hoped I would be an ‘arrow’ in advocating on behalf of care workers (Coe 2019a). Although I discouraged my research participants from hoping for change, I was sympathetic to their desires and secretly wished that my writing could make more of a difference in their lives. My academic writing did not reach a wide audience because of its length, theoretical argumentation, and anthropological terminology, despite public interest in the topics of immigration, aging, and care. These nagging thoughts stifled my motivation to write another book based on research I was conducting on changes in aged care in Ghana.

And so I began to explore different styles of storytelling. I tried writing opinion pieces (Coe 2019b, 2019c, 2017, 2016). These short pieces consist of an opening vignette, several facts, and concluding recommendations regarding action and policy. Often they felt highly simplified – even simplistic – reducing the nuance and complexity of my more ethnographic writing. Some of them led to other media presentations, such as radio interviews, but in general this writing too left me feeling ineffectual. I began to explore visual storytelling, which I thought would be more accessible to my research participants in Ghana, who primarily read religious literature but also consumed television and talk radio.

Because of my research on changes in aged care in Ghana, I began considering making a film about a social group of older adults, organised

through the Presbyterian Church of Ghana, who met on a weekly basis to sing, tell stories, dance, and play games. The district organisers of the group were an unlikely pair of strong personalities: a dour retired minister who reminded older adults of their upcoming death to bring them closer to God, contrasted by a lively, practical retired kindergarten teacher who wanted to make sure the gatherings were fun. In her practical focus on action, the teacher reminded me of my own mother, and I felt drawn to her. I wanted to make a film about her and the activities of the group, since they were visually and aurally arresting with recitations of jokes, riddles, and Ananse folktales, and participants playing *ampe* (a children's game), board games, and music. Unfortunately, the kindergarten teacher passed away before I had a chance to organise the resources to make the film – one of the problems of studying aging! In the summer of 2019, I saw the opportunity to make a film about another similar social group when I would be in Ghana for another bout of field research.

In preparation for my trip, I attended an intensive film class organised by a community centre in the US city where I live. These classes are offered to the community, including to young people, in a commitment to help residents tell stories about their own marginalised communities. What took me by surprise was that I was expected to make a film *during* the class, and not simply afterwards, as I had planned to do once in Ghana. I thus ended up shooting the footage for *two* films that summer. My student film was based on interviews with Auntie Gifty, a Ghanaian home care worker in the US, which I entitled 'Stories from Home Care' (Coe 2020a). I chose to focus on her because she was an excellent storyteller and because the bitterness she felt about her work had been the guiding spirit behind the book I wrote about home care (Coe 2019a); as I wrote, her voice was always in my ear, encouraging me and helping me see the importance of my efforts. The second film, which I called 'Making Happiness: Older People Organize Themselves', focused on an aged fellowship group in Ghana, organised under the auspices of a Presbyterian congregation (Coe 2020b). With the help of a small grant from the community centre, I was able to work on 'Stories from Home Care' with an editing consultant, Ann Tegnell, who taught me a great deal about storytelling through film.

Making a documentary film based on considerable prior ethnographic research made the process quite different from engaging in new ethnographic research: I already had access and permission from the participants. As the ethnographic research was already complete, I had a clear conception on what I could obtain and thus what I wanted to see and hear for the purposes of the film, although, as in ethnographic research, the results are always something different, pulled by the desires of the participants and the exigencies of the situation. The film-making thus felt much more directed and less open-ended than the prior fieldwork. However, I would argue that even in

fieldwork one has a sense of the end product which guides what one will focus on and collect.

For both films I used a lightweight digital camera (Sony α7s), two high-quality external microphones (lavalier and directional), and a tripod. Because of cost constraints, I was the camera-person, interviewer, director, producer, sound engineer, and grip all at the same time, which was a bit overwhelming. Working in a team would have been easier but might also have damaged the intimacy that was made possible by working alone. In comparison, anthropologist Barbara Myerhoff worked with a film crew on her documentary film 'Number Our Days' (Littman and Myerhoff 1976), which she produced in parallel with a book of the same name (Myerhoff 1978). Working with a film crew allowed Myerhoff as ethnographer to appear on screen, adding to the emotional impact of the film because of her own vibrancy and the ability to document her close and caring interaction with her informants. When contrasting this fieldwork with previous fieldwork in Mexico, Myerhoff poignantly remarks that it made sense to her to study aging in a Jewish community because she herself would become an old Jewish lady (she never in fact had that opportunity – she died in middle age). In some ways one could even say that Myerhoff almost steals the show.

One of the most important differences between fieldwork and film-making is that, both in Ghana and the US, the participants in the films understood the purpose of my presence and activities in a way they never had during my ethnographic research and were noticeably more enthusiastic and supportive of my efforts. In Ghana, doing fieldwork, I am often confused for a development worker or potential patron, or even a language student or instructor because of my fluency in Twi; 'anthropologist' is not a known local position. Making a film, on the other hand, was a clear task that people everywhere understood. Aunty Gifty was thrilled to be in a film and was pleased that it would bring attention to home care, although we had difficulty coordinating our schedules and she was perhaps not sufficiently aware how many hours I would need to be with her for a documentary film. The members of the New Tafo aged fellowship group in Ghana similarly welcomed me, because they felt that the film would contribute to their efforts to attract further support from the Presbyterian Church leadership. I ended up interviewing thirteen participants, including the local chief, because they all wanted to contribute. Although I was not sure the film required so many interviews, I could not decline their enthusiasm.

Despite my previous interactions with research participants, sometimes long-term, I learnt more about them through the process of making the film. The main reason was that the film-making provided the rationale to do a lot more hanging out with people and following them around – in sum, to do more fieldwork. It gave me a reason to conduct multiple interviews, such as with one person like Aunty Gifty, allowing me to hear multiple versions of

her life story at different moments in time with different emphases and nuances, making it clearer what was important to her in such stories. I had a more visceral understanding of her lack of a stable home after we struggled to find a place to sit and film an interview. As for 'Making Happiness', after several participants in the aged fellowship group discussed the boredom they felt in their homes, which the activities of the fellowship group relieved, I asked one of the organisers whether I could visit her household and film its daily activities. Her assent allowed me to get a good sense of her lively, intergenerational household, which contradicted her sense of loneliness and boredom. Across multiple interviews I picked up that the leadership of the Presbyterian Church only half-heartedly supported the group's efforts, leading to dismay and discontent by group members about how aging issues were being marginalised in the church. Thus, film-making seemed to support deep ethnographic research by giving me further opportunities (or excuses) to understand people's perspectives and lives by accompanying them and asking them about their experiences.

In other ways, however, the film-making process seemed to go against my desire to learn ethnographically. I often found that I could concentrate either on being present ethnographically or on conducting the film-making: Did I want to stop an informant whilst talking to adjust the light and achieve a better quality shot or did I want to interrupt to ask them to repeat what they had said without hesitations and distractions, or with more detail and full names so they would be clearer? Should I direct how and where they should stand, or should I just follow them around, with no attempt to control light and noise? Through the decisions I made I found that, at heart, I was an ethnographer first and a film-maker second: I really did not want to interrupt the speakers or change their posture in order to achieve a better film. For example, when filming 'Stories from Home Care', Aunty Gifty was driving a patient home after an event and I was filming their conversation, sharing the back seat with Aunty Gifty's possessions, because she was staying with a friend on her couch. As an ethnographer, I was happy to be hanging out with a home care worker and her patient, listening and occasionally chiming into their jokes. As a film-maker, I knew the footage I was getting from the back seat was terrible – the back of their heads jerking up and down with the unevenness of the road. For the purpose of the film, I was also unsure whether I should be silent or join into their jokes. When they stopped at a McDonalds for a bite to eat, I was excited to have the opportunity to film their faces; but then I was prevented from doing so as McDonalds has a policy that no filming may be done on their premises. The film-maker in me sighed in exasperation; the ethnographer put the camera down and enjoyed sharing the food and interacting with the two without the distraction of filming, resigned to writing up the event in fieldnotes afterwards.

The technical quality of the films suffered as a result of my ethnographic instincts, and yet both films benefitted from the rapport, connection, and ease established by my ethnographic heart. It was clear to me that the form of the final product, whether in writing or in film, shapes the process of inquiry and discovery, affecting what is learnt and what is possible to tell.

As my film class repeatedly reminded me, the power of films is in the emotion they elicit. Audiences feel connected to the central participants presented in the film, who seem to speak directly to the audience. Their humanity is on display visually and aurally. For example, whilst the strong personalities of Myerhoff's research participants are visible in her book, her film makes that impression much more immediate. In films, a story about one person seems more powerful than one about several people, in some ways the opposite of ethnographic writing where some of the ethnographic authority comes from telling variations of a single story and having different kinds of informants. My goal when editing the footage was to tell a compelling story, selecting key incidents from a huge array of footage that would move my audience and manipulating the strongest elements available to construct that story. In this sense, I did not find it so different from ethnographic writing, where I chose incidents and vignettes from a much larger set of field notes, interviews, and archival information to illustrate and flesh out a theoretically organised argument.

What did seem different, however, was how much information I could put into each film. A film is much more compressed and condensed than ethnographic writing. Writing is better at providing context and background. The film cannot be as informative, because not all background information can be communicated visually. Relaying context through text or voice-overs has a distancing effect, the opposite of the connection I wanted the audience to feel with the participants in the film. Ultimately, I decided to cut down on the amount of narration I wanted to include and rather considered the films as supplemented by my written material which provided the context. This approach contrasts with Myerhoff, who narrates much of the context in her film and inserts an interview of herself, the combination of which allows her film to raise the same central insights as her ethnography *Number Our Days*.

What also varies between the two products are the different traditions of editing stories. Documentary film-makers are more willing to discuss the constructedness of their product and manipulate its elements than ethnographic writers, perhaps because they are in the same community as artistic and fictional film-makers. From my perspective, film-makers make edits that would make me cringe as an ethnographic writer, like moving elements around to construct a coherent and concise story. This editing is more than eliding out hesitations, stutters, coughs, and sighs. For example, in 'Stories from Home Care', Aunty Gifty told a deeply moving story of helping one of her patients die peacefully. From audience reactions during the editing pro-

cess, it was clear that this story was the emotional high point of the film. At one point, Aunty Gifty referred to the patient's son as 'whatcha-ma-call-it's son', in an effort to avoid using her patient's real name. My editing consultant, Ann Tegnell, warned that this could make Aunty Gifty seem unsympathetic to her patient and destroy the caring persona that had been established in other segments. To prevent her appearing as an unsympathetic figure to the audience, I substituted the term with the word 'his', which Aunty Gifty had said elsewhere. The switch was relatively easy, given the editing software we had available, and is unnoticeable unless you listen for it. Similarly, when Aunty Gifty tells the story of another patient, I inserted the location from elsewhere in her narrative so that the clip was clearer to the audience. In another clip, Aunty Gifty's patient waved goodbye to her as she dropped him off at his apartment. Because I had the camera focused on him rather than her, I had no shot of her response. Instead, I inserted another shot of her from a different moment. Thus, film-making traditions promote greater manipulation of the elements to produce a desired effect: in this case, to maintain emotional connection to Aunty Gifty and to make her stories clear with the minimal number of words. The manipulations had to be invisible or the attempt would backfire, making the audience aware of the film-maker's presence and wondering about my intentions behind the edits. As my goal in 'Stories from Home Care' was to establish and maintain a connection between Aunty Gifty and the audience, I reorganised Aunty Gifty's stories to make them clear and compelling: the more I left out what was unnecessary, the more what remained attained power.

In contrast, 'Making Happiness' clearly had to focus on a group of people in order to highlight the activities of the aged fellowship group. With the founder of the organisation having died a few months earlier, there was no compelling personality around which to organise the film. I also felt obligated to include a snippet from every interview I did, or participants would feel left out. Instead, the drama lay in the organisation these older people had built and the dominant emotion of joy that they experienced in coming together. Most of the footage came from the activities of the fellowship group, which demonstrate the engagement of participants. These are interspersed with short clips from interviews with participants – just a sentence or two – to establish context and clarify what was happening during the various activities displayed. Editing in film-making, as in writing, is essential: often whittling away and honing in strengthen the core. For 'Making Happiness' this process required clarity and discernment about what the core was and was not; editing was the critical labour of discerning and focusing on the core.

I am still learning the technical aspects of film-making, whereas I have become a more proficient writer over the past forty years, honing my craft continuously over that time period. With this my writing has become easier, taking less time and eliciting less anxiety. Filming and film editing are

new and exciting. Editing is time-consuming, with minor edits improving the piece with each review of the film. The process reminded me of writing essays in high school, when I needed to read an essay multiple – sometimes a hundred – times before it was ready to submit, because I needed to train myself to see the problems.

At the moment I do not know whether my films will be any more successful in moving my audience emotionally or bringing awareness to the problems faced by my research participants. Distribution of films is not easy. Even if freely available on YouTube, people are busy and do not necessarily have the time to watch a film of ten to fifteen minutes unless they find it compelling. Ultimately, I see my films as complementary to my ethnographic writing, where the films can be viewed on their own but also reflected in association with my accompanying books. My ethnographic writing provides more context, assuring my audience that Auntie Gifty is not the only home care worker to experience foreclosure on her home, and illustrating the variations in older people's organisations and activities in Ghana. I came to documentary film-making as a result of my dissatisfactions with ethnographic writing, but I have realised that film does not necessarily resolve those concerns. Nor does it replace writing. Rather, for me, writing and film-making are tandem processes, with different goals, emphases, and possibilities.

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Narrative from an Old Photograph: How Absences Make the Story and Inspire Research on Craft Apprenticeship in Benin

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Photography in ethnographies

One of the functions of photography in ethnographic research is to provide a visual representation of a situation. This can assist readers in understanding past situations. Old photographs are also considered vital memories of the past. They inspire emotions and orientate narratives in ethnological research. Elisabeth Cameron, for example, gave her Zambian research participants photographs of themselves to build relationships with them. As she was doing so, she noticed that her interlocutors used portrait photography to capture a fixed ideal of the self or a desired self rather than current reality. The photographs were also spaces where people displayed actual relationships (Cameron 2013: 141–155). One of the most important aspects of using photography is the insight photographs can grant into the context in which they were taken. This context plays a major role for the analysis of the image (Farahmand 2017: 31). Analysis means first of all description of the picture and its visual aspects. For John Pepper (2013: 16), colonial photographs show that already in the nineteenth century African people embraced photography as a means to create images of themselves, sometimes as cosmopolitan subjects, sometimes as traditional leaders, and sometimes to adapt older roles to modern media. Photographs can be used to position bodies and faces within history but can also act as a means of escape. To illustrate this argument of the surface of the image, Christopher Pinney (2003: 218), for example, shows how Indian and Yoruba photographic practices use ‘photographic cutouts [. . .] made by pasting photographic images’. He critiques postcolonial photographic practices as projecting a materiality on the surface in contrast to colonial representations that he characterises as ‘depth’ representation.

The second dimension, as important as the first, is the photograph’s non-visual dimension: the context or the story behind the photograph. This dimension is very important. Elisabeth Edwards sees in recent theorisations by anthropologists working on visual and material culture a renewed interest in the social history of art and in the recreation of the far more inclusive field of objects and images (Edwards et al. 2006: 10). Before and after the moment the picture is taken, many things take place that cannot be seen in the image itself, because a photograph is just a fragment of the past even if

it appears to be transported as an apparent entirety into the present (Barros and Wunenburger 2015: 55; Edwards 1992: 13). In this way interpreting a photograph is a very delicate exercise. There are always fewer things to see in front of an open door than one can imagine behind a closed window. But the non-visual is not only speculation. In the case of photography, it implies the reconstruction of the context and the story behind the photograph. This story can be characterised by personal circumstance, vision, and intention that are present within this overall framework and exist in a reflexive relationship with wider cultural frameworks (Edwards 1992: 13). So many photographs become famous not because of the picture in itself or the person of the photographer but because of the story behind the image. Think, for example, of the photograph from the 1960s of an attractive air hostess standing in front of a Zambia Airways aeroplane that came to stand for the boom of Zambia's economy, its urbanity, and its moves to open up towards the world and its air travel in the 1960s (Ferguson 1999: 237). Similarly, the photograph of East German policeman Conrad Schumann jumping across coils of barbed wire to West Berlin has become an eminent symbol for escape from socialism to democracy. The photograph, which has been widely used to express this escape, was taken two days after the socialist regime in East Germany started building the wall separating East from West Berlin (Voigt 2010).

In ethnology, photographs are often used as testimony of living in the field site and proof for field experiences (Beaugé and Pelen 1995: 8). As representations of results, these photographs allow comparisons with similar or contrary situations (Belden-Adams 2017). For posterity, these photographs are stories, documents, and archives: they show things or situation that cannot be seen anymore. Recently, anthropological research has been conducted on photographs that were exchanged between Africans in the diaspora and their families and communities back home. The importance of these photographs is that they reconnect recipients with the source and show social change, such as shifting notions of personhood, the emergence of new social imaginaries, or the reworking of collective memories (Vokes 2012: 13). Behind each photograph there are stories that denote relationality and temporality. The perspectives that appear in photographs differ from one period to another or from one generation to another. This can be seen quite clearly, for example, in terms of kinship expressed in family photographs. But social change captured in photographs also depends on what concept and perspective the people depicted and/or the photographer wanted to highlight. A researcher who has taken photographs in the field knows the context in which he took them and the stories behind them (Cameron 2013). But in the case of a single old photograph or old collections and archives of images, the difficulty lies in correctly reconstructing the context in which they were taken. In most cases it is impossible to reach all of the protagonists depicted in the images to ask for a full reconstruction. And if it were able to contact them, many

protagonists would find it difficult to remember exactly the context in which a certain image had been taken and the stories behind it. Jo-Anne Driessens (2003: 17) experiments with this on a journey of photographic discovery as she was trying to track down her blood relatives. One method is to ask a protagonist: ‘What does this photograph bring to your mind?’ It is indeed a question that many of us have used in research contexts and also in private life. We know how difficult it is to remember the context of a photograph and the story behind it. We know that reconstructing such a context happens bit by bit as people try to remember what had happened around the taking of the photograph. Often people remember situations that carried important meaning for them: one person might have forgotten the names of persons depicted but might remember something important about a wedding dress worn. Most of the time protagonists cannot narrate all the stories behind a photograph in which they featured. As a result of such partial memories, it is necessary that as many protagonists as possible should be interrogated about each specific photograph. The interactions and possible contradictions between what each of them says help the researcher build up information about the photograph (Schwartz 1989).

A special and rare situation arises when the researcher forms part of the scene depicted in the photograph. For seasoned researchers, it is possible to find older pictures in which they are depicted interacting with interlocutors. Such instances can also happen for anthropologists who do research on their own society. It could even be photographs from the researcher’s own past or their childhood. For my research on craft apprenticeship in Benin, I am in such a special situation where I have a childhood photograph in which I am depicted and that speaks to my research topic. The story behind this photograph has become a focus of my research.

The photograph: from analogue to digital to narrative

The photograph in question was taken in analogue format in Benin in October 1996. I did not take the picture but featured in it as protagonist. There are nine persons in the image, seven of them relatives of a man known in town as ‘Welder Kiki’: two of his brothers, one of his aunts with three of her sons and one of her nephews, and my brother and I. It was a spontaneous photograph, taken in an informal, everyday situation when the welder’s aunt had not yet properly dressed.¹ My brother and I were on school holidays and were visiting the city of Abomey-Calavi² where we stayed next to the welder’s aunt and played with her children. That is how we got included in this image. The photograph was taken at the initiative of the aunt’s nephew who wanted

1 For ethical reasons I do not include the photograph in this article. The welder’s aunt and one of her sons felt it was inappropriate to make it public.

2 Abomey-Calavi lies adjacent to Cotonou, Benin’s central economic hub located on the Gulf of Guinea.

to immortalise us all playing together during this holiday and called a photographer to the house. The photographer invited everybody to join in, which is how we came to be nine people in the picture. Some years later I receive a copy of the photograph as a gift and placed it in a photo album for safekeeping. It is a rare photograph from this time for me and for the others in it for in the 1990s the taking of photographs was an expensive endeavour in Benin. When I was a child, only special events such as Christmas, New Year's Eve, First Holy Communion, a wedding, or a funeral ceremonial were captured in photographs. I rediscovered the photograph when I asked my brother in September 2009 – I had just arrived in Germany – to scan in all my old photographs and send them to me. It is not a special photograph or in any way extraordinary. It is just one of hundreds on my computer. But it has become central for my research on traditional apprenticeship in Benin. It all started when, by chance, Welder Kiki saw the photograph and recalled that it was taken the very day he started his apprenticeship to become a welder.

Fascinated by his memory, I wanted to explore it more and organised a focus discussion with as many of the people depicted in the image as possible. And so we met in May 2018 at the house of Welder Kiki's aunt. My aim was to recreate as closely as possible the situation in which the photograph had been taken. We were four people: Welder Kiki, one of Welder Kiki's younger brothers, one of his aunt's sons, and I. I placed the picture on the table before us and asked each person to say what they could remember from the photograph. I then opened up a general discussion to see what other revelations we could elicit. The entire discussion took about three hours. Even though we had all been children or teenagers when the picture was taken, it was amazing to see that the information raised in the discussion was concordant and I was excited by how it was directly relevant for my analysis. I transcribed the discussion and analysed the information it provided on the context in which the photograph had been taken.

The photograph's context and story

In March 2018 I was in Benin for my fieldwork on craft apprenticeship. Like many in Benin, I have family members or friends who are artisans. These are persons who produce goods or offer services with the use of simple technologies, such as being a tailor and a hairdresser. A childhood friend of mine is a welder: Welder Kiki, as we met him above. I decided to do participant observation in his workshop. One day he came to my family home to repair a metal staircase. As we were talking, I turned on my laptop to show him some old pictures. When he saw the picture in question, he looked startled and exclaimed: 'Do you know why I was not part of this picture?' Indeed, I did not. Generally, individuals not in a picture are not considered in descriptions of a photograph. If they are mentioned, then it is to show what they missed out

on. Richard Vokes, for example, discusses the case of scholars absent from school photographs. In his example, learners were obliged to be present for the class photo in order to qualify for a photograph identity document or passport and to be included in school membership documents. If they were absent on the day, scholars were disqualified from receiving their identity document and were excluded from their school (Vokes 2012: 212). Nowadays, absence from a school photograph no longer carries the same consequences, but it does affect the narrative told about the photograph in the future. Thus, discovering the reason why a person who should have been part of a photograph was absent can be insightful for constructing the narrative behind the photograph, a point critical for our photograph. Welder Kiki explained: he had been absent from the photograph because on that very day his uncle forced him to start an apprenticeship to become a welder. His information opened up a completely new perspective on the photograph: not those featured *in* the photograph but a person *absent from* it was instrumental to its analysis. The photograph was not a simple family picture: it was a story.

The question arose why two of Welder Kiki's brothers were in the photograph – for they were not there by mistake. Their father had been a military officer in Parakou, the largest city in northern Benin. He had passed away in May 1996, a few months before the image was taken, and left behind a widow with seven children: the oldest sixteen years old, the youngest about three months. After his death, the widow mother and all the children came to Abomey-Calavi to stay with an aunt (one of the father's sisters) for a while, until the family could decide on a more permanent solution. All paternal aunts and uncles were involved in organising the care for the children and their mother. The decision finally was for each aunt and uncle to take over the care for one or two children. It was in this way that the two brothers in the photograph were identified to remain with the aunt in Abomey-Calavi – and how they came to be in the image. Welder Kiki, in turn, the second son and fourteen years old at the time, was to stay with an uncle who was a welder. It is from him that he learnt to become a welder. But until he was able to move to his uncle's place, he remained at his aunt's house and that is how we got to spend the holidays together. The day we took the photograph was the day of his departure: in fact, he had already left by the time the photographer arrived. Early in the morning his aunt had informed him that his uncle would come and pick him up that day. He remembered having been very unhappy: 'I cried all the time. The problem was not to go and stay with the uncle, but the real problem was that it was also decided that I must stop going to school and start an apprenticeship as a welder'.³ In fact, when the aunts and uncles decided for the care of the children, they did so with a clear plan for each of their social integration. The oldest brother was to continue schooling until he was old enough to get a job. His younger brothers were

3 Welder Kiki, interview with author, 25 May 2018.

also to continue their schooling. Their mother, with the baby, was to return to Savalou, her and the father's birthplace.⁴ Kiki, however, was to leave school – he was still attending primary school despite already being fourteen years old.⁵ He recalled how he experienced this as a cruel decision, had cried and said: 'My father is dead. This is the reason why they take me out of the school to do an apprenticeship'.⁶

But he could do nothing to change the decision; at that point he had no choice: he had to follow the family's decision. So, at 11 o'clock on that morning, the uncle arrived to collect him. He did not want to, refused to go, and started crying. The aunt's husband and other adults tried to persuade him to accept: 'They told me this is a good decision for my future. They told me also that the apprenticeship has many positive aspects, and I can find very early a job and earn money'.⁷ After arguing with him for a little while, his aunt then gave him some presents to motivate him to go with his uncle.⁸ Shortly after lunch they finally left the house. So when we took the photograph around 2 p.m., they had already left. This is the reason why he did not feature in it. But why is this context so important for my research on apprenticeships in Benin?

The photograph and research on apprenticeships

The fact that Welder Kiki was not present in the family photograph was because of the family's decision to send him to learn a craft as an apprentice. In the context of my research on craft apprenticeship in Benin, this particular photograph reveals two aspects central to anthropology: kinship and education. The photograph was taken at a time when Welder Kiki and his brothers were set in a context of kinship fostering. This context also was the reason for Kiki's enrolment in a craft apprenticeship. What this context reveals is the manner in which relatives arrange not just for the physical care of children after the demise of their father but *about the future* of these children, particularly on the subjects of schooling and apprenticeship.

Kinship fostering is a common practice in many African societies. It has been linked to concepts of parenthood and childhood, and different forms of this practice have been identified. Erdmute Alber (2014: 99) shows for the Batombou in northern Benin that children often grow up with non-biological parents (see also Martin 2015: 47). Esther Goody (1982: 38) points out how, in Ghana and other African countries, kinship fostering allows aunts and uncles to take care of the children of their brothers and sisters:

4 Savalou is a small town in central Benin.

5 Generally, children finish primary school at the age of eleven.

6 Welder Kiki, interview.

7 Welder Kiki, interview.

8 He did not, in fact, have a choice for the decision had been taken, even if the family did engage with him to some extent and tried to make it a bit easier for him.

The claiming of rights in children is expressed in the institution of kinship fostering that is prevalent throughout Gonja, taking somewhat different forms in different parts of the country. The institutionalised pattern is for a daughter of marriage to go to a father's sister and a son to the mother's brother.

Kinship fostering plays an important role in providing care for orphans. In situations of crisis, such as death or divorce, the entire family acts as a support structure to provide care for the family members in need (Hollingsworth 2012: 22). The importance of kinship fostering goes beyond mere help for a few family members in difficulty. As social structure, it is of crucial importance for survival in African countries like Benin where there is no universal care system provided by the state. In the context of the story behind the photograph, the way in which Welder Kiki's uncles and aunts acted is a common way of caring for orphans in Benin. So, kinship fostering is a form of care system for orphans. What about the decisions made for children?

The place of children in society is always a controversial question not only because of their rights but also because of the duties expected from them. Parents are responsible for their children. At the same time, they take into consideration the child's will or at least the child's own interests. Many international conventions on the protection of children highlight this aspect (Bello 2015; Wanitzek 2013). In the case of Welder Kiki, his aunts and uncles decided to take him out of school, a decision he considered degrading and disagreeable. However, he had no choice and had to follow the decision made by his relatives. The universal recommendations regarding the rights of children are not really applicable because of the local context. Decisions made for children by adults are not always bad in the long term. In Welder Kiki's case, this decision in fact led to success. Today he is the only successful one amongst all brothers: he has his own workshop, is married, and has two children. Even if he has no advanced educational qualifications, he has a stable life, an income, and a family. This observation tells us something about the perception of apprenticeship in Benin: here, as elsewhere in West Africa, craft apprenticeships are considered appropriate only for children who are not good at school, or for children whose parents cannot finance their schooling (Adekola 2013). The low social status accorded to people with an apprenticeship plays a considerable role in decisions whether to send a child into an apprenticeship. Many youths consider it as negligence shown towards them when they are pushed in this direction and a refusal to invest in them. Nevertheless, as empirical investigations show, apprenticeship is one of the best ways to have a chance of getting a stable job (Fajobi et al. 2017).

My research into craft apprenticeship in Benin was oriented by this family photograph of kin of one of my interview partners. It shows a kinship context in which children were living with non-biological parents. My interview partner was absent from the photograph, but the story behind the image

dominated his life. My analysis shows that the photograph cannot be taken as a single, unchanging representation but has to be understood as a succession of meanings attached to the practice of kinship fostering in order to ensure orphans are cared for. The story behind the photograph also shows that decisions that affected children might consider as bad at the moment can be what saves them in the future.

The photograph speaks not only to the issues of social life. It also refers to the debate on the postcolonial perspective in photography (Pinney 2003). This specific photograph is not based on a kind of ‘vernacular modernism’; rather it was made impulsively in the normal course of life and included everyone who was present in the household at that time. In this manner it gives a depth representation of the familial situation at this moment. Where, some years ago, photographs from colonial or missionary archives were the only available sources of ‘depth’, this is no longer the case today. Through the appropriation of the technology of photography, African natives can engage in this area by making photographs that highlight not only the social imaginary or that rework collective memories (Vokes 2012; Pinney 2003) but that have real depth and capture ordinary social context.

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I am a PhD student at the Bayreuth International Graduate School of African Studies (BIGSAS). After a bachelor's degree in labour administration at the University of Abomey-Calavi in Benin, I moved to Germany where I achieved a bachelor's degree in sociology at the University of Heidelberg and a master's in sociology at the University of Osnabrück. I joined the University of Bayreuth in the winter semester 2017. My PhD project focuses on craft apprenticeships in Benin.

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No Magic! Teaching Ethnographic Writing

Julia Pauli

Introduction

The first time I realised that it makes a difference how ethnography is told was during a conference on social networks in Charleston, North Carolina, in 1996. At the invitation of our teacher and mentor, the late Thomas Schweizer, Michael Schnegg and I gave a paper on how high school pupils in a multi-ethnic classroom in Cologne, Germany, viewed each other's friendship networks. I had collected the ethnographic data as part of my master's research.¹ The focus of the presentation was on the analysis of the perceived networks. To complement the graphs and tables, I described a scene I had observed whilst sitting in the classroom with the learners. This scene was not in any way unique but rather typical for classroom interactions. The fifteen- and sixteen-year-olds were divided into segments and the pupils were performing their affiliations to these groups along gender and language lines. They did so through elaborate acts of expressing boredom. Whilst the teacher stood at the blackboard, not noticing what was going on behind her back, a group of pretty, young girls combed each other's hair, some German-speaking boys sparked a lighter, and a group of learners fluent in Turkish kicked a little ball made of paper. The students gave the impression of being decidedly bored by the teacher and their classmates. At the same time, I observed how they were observing each other intensely. Obviously, this scene was nothing special. Still, I sensed that it captured an aspect of the students' social networks I could not have otherwise expressed.

In the discussion following our presentation, some questions specifically addressed the classroom scene. As an unexperienced, young researcher at her first academic conference, I was astonished that an ethnographic scene could stir such interest. Although my alma mater, the anthropology department at the University of Cologne, Germany, is well known for its focus on research methodology, during the five years of anthropological training no one had instructed me on how to write ethnography.² Of course, we had extensive debates on 'writing culture' (Clifford and Marcus 1986), but these fo-

1 I follow Kristen Ghodsee's (2016: 3) definition of ethnography as a 'qualitative method to focus on the experience of everyday life'. Ethnography is both the act of fieldwork and, based on that fieldwork, the 'written representation of culture (or selected aspects of culture)' (Van Maanen 2011: 1).

2 This has probably changed in many anthropology departments, at least for doctoral students. Many PhD anthropology colloquia now also focus on questions of writing and style.

cused on deconstructing what famous anthropologists had written before us. What was missing was concrete advice on how to move from deconstruction to construction.

Realising the importance of writing, I started searching for ethnographic role models to learn from. Reading, admiring, and imitating what I read in other anthropologists' ethnographies was how I tried to tackle writing my PhD thesis. This approach was exciting because my reading led to invigorating 'discoveries' on how to write. I was electrified, for example, when in 1997 I read Ruth Behar's *Translated Women* (1993); I had never before imagined that dialogue could play such a central role in an ethnography. Yet these discoveries were also frustrating: seeing the elegance a Ruth Behar had achieved, I doubted that my own writing attempts could have any value at all. At times, ethnographic writing appeared to me like magic, a supernatural gift that some very talented anthropologists had been granted, but not something one could learn in any anthropology courses.³

More than a decade later I came across John van Maanen's book *Tales of the Field* (2011). The book was an eye-opener. I realised that figuring out how to write ethnography was a problem shared by many anthropologists: 'This lack of tutoring is perhaps most telling at that still point in our studies when we have returned from the field and sit before the blank page that must eventually carry the story of what we have presumably learned' (Van Maanen 2011: xvi). Struggling like Van Maanen (2011: xvi) to 'simply "write up" what I had "discovered" in the field', I turned to other PhD students. We started reading and discussing each other's work and with time I realised how important respectful critique by academic peers is for developing one's ethnographic writing.

My next step was to move from learning how to write to teaching what I had learnt. It was a fortunate coincidence that in 2010 the Department of Social and Cultural Anthropology at the University of Hamburg, where I was now employed, decided to revise its master's programme. This opened up the chance to integrate ethnographic writing into the curriculum. We designed two new courses on reading and writing ethnography, the first called 'Reading and Writing Ethnographic Texts' and the second 'Ethnographic Writing Workshop'. Encouraged by their success, we later developed a third course titled 'Observing and Writing'. Whilst the first and third of these courses aimed to prepare students before they engage in their fieldwork, the second

3 In the last years, the situation has changed. Today, anthropology students can find helpful publications on ethnographic writing and the role of the anthropologist as writer (Wulff 2017; McGranahn 2020c; Waterston and Vesperi 2009). Some anthropological contributions explicitly focus on storytelling (McCormack 2000; Davidson 2019; Gottlieb 2016; Besnier and Morales 2018; Narayan 2012). A number of publications provide hands-on information on how to move from fieldwork to deskwork (Ghodsee 2016; Atkinson 2020; Gullion 2016; Nielsen and Rapport 2018).

one aimed to help them work on their own ethnographies after their return from the field.

Now, after years of teaching ethnographic writing, I am convinced that an early engagement with ethnographic writing is most helpful to students. I continue to be surprised that remarkably few publications address how to teach ethnographic writing. Taking up the writing culture critique of the mid-1980s, David Hess' (1989) contribution is an early exception.⁴

In this special issue my perspective as a teacher is juxtaposed by the contribution of graduate student Charlot Schneider who describes how she has experienced the three writing courses taught at Hamburg. I hope that our contributions will foster further dialogue on different approaches to teaching and learning ethnographic writing. In the following, I first describe the two reading and writing courses taught in Hamburg in preparation for fieldwork. I then introduce the course on ethnographic writing that we teach for those returning from fieldwork. I conclude with some general reflections on teaching ethnographic writing.

Reading ethnographies

The course 'Reading and Writing Ethnographic Texts' is taught in the first semester of our master's programme. Course participation varies between ten and fifteen students. The primary aim of the course is to encourage students to read ethnographies. Michael Lambek (2020: 63) has observed: 'Anyone trying to write – a letter, novel, dissertation, poem, or ethnography – knows that it is a skill to be cultivated and to be learned through the sheer doing. This cultivation occurs in part by means of reading' (see also Behar 2020; Gay y Blasco and Wardle 2007). At the beginning of the course, students are asked to provide lists of ethnographies they have read. Presenting the books to the group, the students reflect on their reading experience. I then ask them to compile a list of the books they would most recommend, which we then use to figure out why certain ethnographies are more popular than others.

The pleasure of reading ethnographies is further explored in another exercise. At the beginning of each course, I select ten of my favourite ethno-

4 There are a few blogs that provide advice on and syllabi for (critically) reading and writing ethnography. Erin Gould and Anne Allison host a blog for the Society for Cultural Anthropology on which they make available syllabi for teaching critical ethnography: <https://culanth.org/fieldsights/syllabus-archive-critical-ethnographies> [accessed: 23 July 2020]. I thank Caroline Jeannerat for the information. Carli Hansen from the University of Toronto Press hosts another interesting blog on teaching anthropology and ethnography: <http://www.utpteachingculture.com/five-simple-steps-for-helping-students-write-ethnographic-papers/> [accessed: 23 July 2020]. Communication researcher Nick Trujillo (1999) provides some additional advice on teaching, practicing, and writing collective ethnography.

ographies from my bookshelf. The ethnographies are diverse in topic and style, ranging from classics (Shostak 1983; Crapanzano 1980; Behar 1993) to ethnographic community studies (Cancian 1992; Argyrou 1996) to auto-ethnographies (Greenhalgh 2001). Each week, each student picks one of these ethnographies and reads as much as possible. In the following week, they present their reading experience to each other and return the book for someone else to read. Only few ethnographies are so exciting that students will ask to borrow them again after the semester has finished (these generally include Holmes 2013; Green 1999; Scheper-Hughes 1992; Shah 2019). The book that has been the most popular ethnography throughout has been Shaylih Muehlmann's *When I Wear my Alligator Boots* (2014). In her work Muehlmann describes the lived realities of ordinary people at the margins of the drug economy in the US-Mexico borderlands. Students are deeply moved by the stories, the political economic background, and the presentation of the anthropologist's own vulnerabilities during fieldwork.

The second aim of the course is more experimental: by imitating different ethnographic styles, students experiment with writing. They learn that they have choices for how to write. Each week we read a chapter from John van Maanen's *Tales of the Field* (2011). Van Maanen distinguishes three established and a number of emerging ethnographic writing styles. More established styles are what Van Maanen classifies as realist, confessional, and impressionist tales. Emerging styles include critical, formal, literary, and jointly told tales.

After we have read the chapter on the realist tales, I ask the students to write several pages in that style.⁵ Until the 1970s, realist tales were the dominant way of writing ethnography. The cultural expertise of the anthropologist lies at the core of this form of writing. The written presentation of an anthropologist's expertise and authority is achieved through folk terms, long quotes, lots of cultural details, and an 'interpretive omnipotence' (Van Maanen 2011: 51). For their imitation of this style, students are free in their choice of topic, time, or region – there are no limits to their imagination. Some students imagine themselves as being Bronislaw Malinowski, others as Margret Mead. They describe how they do research in faraway places and on exotic rituals. Others decide to stay closer to home. They choose places like senior residence homes, eco villages, or migrant communities. After we have read everyone's text, we search for the similarities and differences between them. We discuss whether some are better examples for realist tales than others. After some initial reluctance, students tend to put a lot of effort into writing these texts. Some have remarked how this exercise showed them how much power is involved in writing. Writing creates an imaginative space that did not exist before.

5 Nick Trujillo (1999: 710) describes a comparable writing exercise based on Van Maanen's tales.

The next step of the exercise is to rewrite a text. For this we read Van Maanen's chapter on confessional tales. Confessional tales focus on the researcher and his/her social positioning during fieldwork. Often these tales narrate how a fieldworker overcomes initial fieldwork troubles, turning from a stranger into a friend. When I ask students to rewrite their realist tales into texts using the confessional style, they are baffled. In our discussions they often express surprise about the different possibilities of writing. They are captivated when they realise that many ethnographies mix realist and confessional tales. When we reach the third style, the impressionist tale, students are again astonished. Impressionist tales narrate the unusual. They are dramatic stories that catch the reader's attention. By now, students have lost their fear of writing. With verve they rewrite their texts once again, imagining crucial turning points and dramatically accentuating fieldwork incidents. Their initial astonishment and apprehension have turned into excitement to try out these various styles. Playfully imitating realist, confessional, and impressionist tales helps students prepare for their own ethnographic writing. They become aware of different writing styles and learn how to mix them. This leads to more confidence about their own choices in writing.

Observing and writing

The second course that prepares students for fieldwork is titled 'Observing and Writing'. The course has been inspired by Kristen Ghodsee's *From Notes to Narratives* (2016). Ghodsee's book gives conceptual and practical advice on how to write readable ethnographies (see also Menzfeld in this special issue). One of the writing exercises she suggests is to ride in an elevator and then to describe everything that happened whilst doing so (Ghodsee 2016: 49). When students attend this course, they have not yet collected their own ethnographic data. Ethnographic exercises like the elevator ride are a good way to introduce them to observing and writing. The course's structure is twofold. Reading and discussing Ghodsee's book is complemented by three ethnographic exercises. For the first exercise, students are asked to spend half a day on a public playground and to take notes on everything they consider important. Afterwards they have to turn their notes into an ethnographic narrative of approximately four pages in length. We begin with the students discussing their narratives in small groups of three or four. They then revise their texts on the basis of this feedback. The revised texts are then discussed in the class as a whole. Students often observe that they find the mixture of critique by their peers and critique by their teacher most helpful.

The aim of the course is to encourage students to observe and write, not to make them feel insecure. It is thus essential to start all discussions of their ethnographic narratives with a respectful acknowledgement of their texts. We then concentrate on questions of style and grammar; the title; the

opening, closing, and development of the narrative; and the way the authors present themselves. We reflect on what is being emphasised in the text, what we feel is missing, and what might be superfluous. These reflections often extend into more general thoughts on fieldwork. Not all students, for example, enjoy the playground exercise, feeling rather awkward sitting there alone, watching, and taking notes. 'People thought I was stalker', a male student complained. After more than an hour of uncomfortable participant observation, he thus decided to approach everyone on the playground to explain that he was an anthropologist. Female students and students doing the exercise in pairs are generally more at ease with the situation.

The second exercise, based on Ghodsee's elevator ride, follows a similar course of action. Students ride in the elevator for approximately two hours. It is up to them to choose where they do so. Elevators in shopping malls, office buildings, train stations, airports, university buildings, banks, and even a paternoster lift in a municipal building, with its incessantly moving open compartments, have been settings for student observations. Similar to the playground exercise, some students tend to struggle with the task. They are troubled by the irritation shown by other passengers; they feel uncertain as to how much of their role they should reveal to them. Despite these challenges, the ethnographic narratives the students produce are often remarkable. In ethnographic vignettes, choreographies of avoidance become visible. People in elevators use their bodies, gazes, shopping bags, and children to prevent getting in touch with one another. The unwelcomed physical proximity produces revealing, frustrating, and funny stories. These stories comment on gender roles, rituals of consumption, place-making, and social hierarchies.

The third exercise is done as a collective. The aim is to reflect on similarities and differences of observation when people experience the same social and physical space. We all, including me, spend our lunch hour together in one of the university's cafeterias. Our task is to observe, take notes, and then write an ethnographic narrative on eating lunch in a university cafeteria.⁶ University cafeterias are generally very large dining halls. When we enter one of these halls around 11 a.m., there are only a few people eating a meal. We spread out across the room, take our seats, and place our notebooks in front of us on the tables. Some might start drawing a map, others might look around, searching, and then avoiding each other's gazes. Slowly, the hall fills. By 12:30 p.m., the hall is packed, loud, and smelling of fried food. Balancing trays of steaming food, careful to avoid bumping into each other, students search for a place to sit. Around 1:00 p.m. the stream of hungry students begins to thin out. When most students have finished their lunch and left the cafeteria, at approximately 2:00 p.m., we end the exercise.

6 Anne Lamott (2020: 62–69) suggests writing about school lunches, whether from memory or observation, as this encourages reflection on human similarities and differences.

Similar to the other two exercises, each student writes an ethnographic text after the observation. Students perceive the cafeteria observation as not so difficult. No awkward feelings trouble them. Although they do not eat and only take notes, they fit in and are at ease with the situation. Yet when they get to compare their ethnographic narratives, they are nevertheless surprised. Many of them will remark on the social relevance of saltshakers: to ask for salt is the main form of interaction between strangers in a university cafeteria. In general, this brief verbal exchange does not lead to anything. But sometimes this banal request turns into a flirt, a conflict, or even an insult. Some students notice this, others, because of their position in the room or some other sort of distraction, do not. The saltshaker vignette is an example for the observational similarities and differences produced in collective ethnography (see also Pauli 2020; Trujillo 1999). Students realise that they do indeed share a social and physical space during the observation task. This sharing frames what they can write. Their own ethnographic writing has to resonate with what the other students write. At the same time, their shared perceptions are fuzzy and vary. What a student eventually writes depends in which direction they are looking, where they sit, and how attentive they are. The ethnographic variations are the result of each student's observations and also the way they craft the ethnographic writing.

All three exercises increase the students' appreciation for ethnographic details. Students who have taken the course and then started their own fieldwork observe that the course helped them to look more closely and listen more carefully. Students also realise how crucial detailed and extensive note-taking is for doing ethnography. They are more aware of the insights they can gain from drawing maps and taking photographs. Finally, they are also better prepared for the many awkward moments fieldwork brings with it.

Ethnographic writing workshop

The third MA writing course is titled 'Ethnographic Writing Workshop'. The course is mandatory for students who have finished their fieldwork and start working on their ethnographic analysis and master's thesis. The course has three goals. First, we want to help students to start writing, moving from fieldwork to deskwork. Second, we aim to encourage students to understand their ethnographic writing as cultural analysis. And third, we hope that students meet peers and form writing groups. Reading and commenting on each other's ethnographic texts helps students get through the ups and downs of writing their theses.

Since we started this curriculum in 2011, we have revised the course several times, incorporating new work on ethnographic writing (for example Ghodsee 2016; Narayan 2012; Gullion 2016; Atkinson 2020). Currently, we concentrate on three narrative forms: based on their fieldwork, students

write a key scene, a portrait, and a dialogue. We always start with the key scene. To think about ethnographic experiences in key scenes has been inspired by Sherry Ortner's (1973) work on key symbols. Ortner proposes that certain symbols are at the 'core' of cultural systems. Symbols can include metaphors, practices, rituals, events, or scenarios. The 'keyness' of a symbol, Ortner (1973: 1343) writes, depends on how the symbol relates to the cultural context. She distinguishes between summarising and elaborating key symbols. In an emotionally charged way, a summarising key symbol encapsulates and stands for the broader cultural context (see also Menzfeld in this special issue on *pars pro toto* scenes). The American flag is an example for this kind of a key symbol. Elaborating key symbols, on the other hand, derive their key status primarily by their recurrence in practices and other cultural symbols (Ortner 1973: 1340).

Building on Ortner's insight, we discuss key moments during the students' fieldwork. Most students easily remember a scene, a symbol, or an event that in one way or another was remarkable during their fieldwork. I encourage the students to narrate the scene in as much detail as possible. Most students are excited to talk about their fieldwork in this way. Discussing their key scenes helps them acknowledge how much they have actually learnt through the fieldwork. It helps them deal with the insecurities that often plague them upon their return from the field. The interest expressed by their fellow students stimulates their ethnographic self-confidence and eventually helps them to write. The key scenes described by students often capture events that were turning points for them in their fieldwork when initial confusion transformed into cultural understanding. A few years ago, a student conducted research on political authorities in Costa Rica. At a certain moment in her fieldwork she recognised cacao as a key symbol. During the course she wrote a key scene on how the *cacique*, the indigenous leader of the community, invited her to his house to drink cacao with him. By describing the scene and discussing it in class, she realised the peculiarity, almost sacredness, of cacao in the village. Going back to her fieldnotes she noticed that all political and religious events included the preparation of cacao, a practice only shamans were allowed to do. Although the student knew intuitively that drinking cacao with the *cacique* was crucial for her understanding of the local situation, only by writing, revising, and discussing it as a key scene did she begin to understand the wider cultural implications of her ethnographic observation.

After discussing in class what could be potential key scenes for each student, they write a first draft of the scenes they have chosen. They then present this draft to a small group of fellow students. They revise their drafts, send the revised texts to me and then we discuss them all together during the next class. Students can only pass or fail the course; they do not get any grades for their key scenes, portraits, or dialogues. This frees them from

anxiety about not meeting my assumed expectations for their writing. I do, however, provide them with detailed individual feedback, commenting on style, structure, development, title, opening, closing, and analytic depth of each key scene, portrait, and dialogue. I also recommend some further reading. Verlyn Klinkenborg's (2013) *Several Short Sentences about Writing*, for example, is an excellent way to think about writing sentences and is a book I often recommend at this stage. He suggests viewing each sentence as entering a stage, saying its piece, and then leaving the stage. After reading his text, students tend to be much more careful how they craft their sentences.

The second exercise is to write a portrait. The course of action is similar to writing the key scene in the first exercise and the dialogue in the third. First, we discuss how to write a portrait in class, then the students write, discuss in small groups, and revise. Finally, we meet again in class. Ghodsee (2016: 35–40) gives some suggestions on describing people (see also Gullion 2016: 83–86). Her most important advice is to characterise people not simply with adjectives but rather by describing their actions. Many students chose to describe their key informant – a Puerto Rican priest interacting with her parish in the aftermath of a hurricane, a migrant mother from The Gambia struggling to survive in Sweden, or a woman selling her products on a market in Ghana. To write about people encountered during their fieldwork helps students to analyse how social structures and individual agencies interact. Writing a portrait is an opportunity to understand how a person can or cannot change the wider circumstances in which he or she is embedded.

The third and last exercise is to write a dialogue. Ghodsee (2016: 62–70) gives some advice on writing a dialogue. She outlines options of dealing with foreign language citations and describes how to mix descriptions of people and place with dialogue. Many students nevertheless experience writing a dialogue as quite difficult. At first, writing dialogue is similar to writing a key scene. Students go through their notes, interviews, and memories to find some telling interaction they can write about. The trouble starts when students have no tape-recording of a verbal exchange. 'I am afraid of making things up', a student commented recently. This resonates with Ghodsee's (2016: 38) caution: 'Where novelists imagine, ethnographers must observe'. I encourage students to go back to their fieldnotes and any other material they might have on a particular interaction and dialogue. We discuss how to deal with the impossibility of tape-recording 'everything' during fieldwork (see Kroeker in this issue). Although there is no easy solution for how to incorporate dialogue from participant observation and field notes, students nevertheless see the value of dialogue and monologue for ethnography: 'Dialogue brings a manuscript to life, allowing your informants to directly speak to the reader' (Ghodsee 2016: 62; see also Gandelman-Trier in this issue).

When students evaluate the course at the end of the semester, they are often enthusiastic. I believe that one reason for the students' enthusi-

asm is the course's format. It has a clear structure, but it also encourages individual experimentation and creativity. The course addresses students' need for guidance, tutoring, and advice without pressing them into the same template. There is enough room for the peculiarities of each of the students' ethnographic projects. Writing an engaging dialogue, a telling portrait, or a convincing key scene might in the end feel magical for them: it is the magic of creating a world by words, something that lies at the very heart of anthropology. To demonstrate and discuss how one might get there is not magic. It is teaching.

Conclusion

The lack of instruction on ethnographic writing strongly shaped my coming of age as an anthropologist. No anthropology course guided me. Instead, I learnt from books and peers. The reading of ethnographies gave me a sense of what was possible. Fellow students helped me analyse social life through writing. However, this approach had many shortcomings. I had no idea of the different 'tales from the field'. I admired the writing in certain ethnographies without being able to identify why the styles so impressed me. I was also completely unaware of what to consider when writing a portrait or a dialogue. The ethnographic writing courses taught in Hamburg address these needs of graduate students, helping them cope with the many questions and uncertainties of writing ethnography. The courses have been developed in line with my colleagues' and my conviction that a space to write ethnography is absolutely essential in an anthropology curriculum. We continue developing and revising the courses, closely looking at how helpful they are for the students.

My involvement with teaching ethnographic writing has opened up a number of issues that I would like to explore further with students and colleagues. In the opening chapter of her edited volume on writing in anthropology, Carole McGranahan describes a conversation she had in 2016 with novelist Lily King (McGranahan 2020b: 2–3). King is the author of *Euphoria* (2014), a novel about the fieldwork and love life of Margaret Mead, Gregory Bateson, and Reo Fortune in Papua New Guinea in the 1930s. McGranahan asks King how she managed to convince her readers that she had really been there, in Papua New Guinea. King answers that this was not her goal; her goal was rather for the reader to feel that they are there. This episode pointedly highlights some important differences between ethnography and fiction: 'Establishing credentials as a scholar differs from demonstrating skill as a writer of fiction' (McGranahan 2020b: 3). It would be worthwhile to inquire how anthropologists imagine their readers. Do they write for fellow anthropologists? Other academics? The wider public? Their supervisors? Their interlocutors? To reflect more deeply on the (imagined) readers of ethnography

and how this influences the writing is an issue that needs further exploration (see also Van Maanen 2011: 25–35; and Coe in this issue).

Another issue concerns the link between fieldwork and deskwork. Several of the contributions in this special issue (Kroeker, Luncă, Riedke, Stolz) discuss the ethical and conceptual consequences of incomplete ethnographic knowledge. Incomplete knowledge of a story, a person, or a social situation is very common in ethnographic research. Building on these insights, I suggest that a more in-depth reflection on how ethnographers listen to their interlocutors and the world around them could help to better understand some of the gaps. Marnie Jane Thomson (2020) has pointed out how important listening is for ethnographic writing. Numerous methodology books give advice on how to ask questions; how to listen, remarkably, is hardly mentioned. When I was a graduate student, I took a course on asking and listening offered by Lilo Schmitz, an anthropologist and a person-centred therapist. In her course, we applied Carl Rogers' person-centred interview technique for ethnographic questioning. Rogers' reflective listening and his technique of mirroring and summarising what an interlocutor has said worked rather well. I believe that these insights could be further developed for teaching and writing ethnography. What we write about very much depends on our ways of observing, participating, asking, and listening.

A final issue relates to the often troubling sense of loneliness and insecurity when writing. In an interview with Carole McGranahan, the exceptional writer and anthropologist Kirin Narayan says: 'Writing along with others is a wonderful way to get past the sense of one's own crushing limitations' (McGranahan 2020a: 92). Ethnographic writing courses can provide a space for this supportive writing.

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Space to Write: A Student's Perspective on Ethnographic Writing

Charlot Schneider

The first time I had to write an ethnography was in 2016 during the final term of my bachelor degree at the University of London. I was writing about a human rights charity. I remember sitting in the library with my field notes, theoretical ideas, and diagrams and being at a complete loss. Until that point writing had been about short, concise, and interrogative argumentation. It was about setting authors up against each other, using the literature to create a strong position; a position that did not require reflection about oneself or the description of a real individual. Instead, it was a disembodied, anonymous position from which I critiqued, endorsed, or developed on the distant theories of faceless strangers. Of course, this sort of writing is necessary in anthropology and I am grateful that I was able to hone this skill. But it is also a skill which proved to be of little service when it came to writing up my fieldnotes.

Lacking guidance on how to write ethnographically, I turned to those which had already been written. Throughout my three years of undergraduate study I had read many of these, but rarely had I focused specifically on the writing. Feeling rather panicky now, I flicked through them, looking for clues that might help me craft my own. At the time it seemed almost impossible to pin down a definition of an ethnography. There is such a vast array of ethnographies – dealing with individuals or groups, ranging from wholly subjective narratives of personal experience to objective accounts, taking into account the writer's own ethnocentricity or writing as perched on the outside. It is only now, having been forced to ask what ethnography is, that I am even beginning to have an idea. What I believe they all share is the aim of relaying an experience, be it the writer's own or that of an individual or group the writer is studying, through the use of different techniques, including metaphor, narrative, and dialogue, and embedding that experience within a wider theoretical framework. At that time though, still holed up in the library, I felt out of my depth and the reading seemed too little too late. I lacked the confidence a writer needs to flourish on paper – confidence that would have enabled me to go with the flow, to write without fear that my words were irrelevant or inadequate. And lurking somewhere further below was the knowledge that I would be marked for this first attempt.

My feelings of bewilderment and the absence of support for how to write up my field notes was not something unique. The anthropologist Van Maanen (1988: xvi) describes a similar situation to my own: 'this lack of tutoring is

perhaps most telling at that still point in our studies when we have returned from the field and sit before the blank page that must eventually carry the story of what we have presumably learned'. Looking back now, after having just finished writing an ethnography, this time as part of my master's in anthropology at the University of Hamburg, I realise how different things can be – how learning to write an ethnography does not have to resemble a clumsy walk in the dark but can and should be a rewarding process, one that is fun, collaborative, and liberating. Whilst my first ethnography was not a complete disaster, it did not leave me with a taste for more. In fact, I was glad to put my pen down at the end of it all; I'd had enough of writing. This time I had the privilege of discovering the joys of writing and the endless possibilities of words.

The first step of learning to write ethnographically is tackling the question of what ethnographic writing is and how it is both similar to and different from other forms of writing. Many anthropologists have remarked on how ethnographies encompass a range of narrative forms (Narayan 1999; van Maanen 1988; Jordan 2001). It is unlike any other type of writing in the sense that it weaves in and out of the evocative and the analytic, the self and the other, the everyday and the unexpected. As Narayan (2012: xii) argues, ethnography needs to engage the reader, emotionally, intellectually, and aesthetically, to have an impact on them. For me the best ethnographies are those that evoke a connection on a personal level, make me grin or frown, push me to interrogate something I had assumed or place an issue into a new light. The aim of ethnography is to see the world from another perspective, to look through the eyes of the individuals around whom the ethnography pivots. To achieve this the ethnography has to draw in the reader, make them feel situated right in the middle of the narrative, have them think, feel, and experience as another person. This requires skill, where language is employed in a way that seamlessly transports the reader into this world.

Unfortunately, it is not uncommon that excellent theorists with exciting data fail to draw the reader into the energy and atmosphere of their fieldwork. Their overuse of jargon and complex sentences leaves the reader rereading sentences with a feeling equivalent to masticating a mouthful of gravel. Whilst most anthropology students get a good dose of guidance on how to write intellectually, few will be exposed to active teaching on how to weave together narrative and analysis.

It is perhaps useful then to think of ethnography as 'real fiction' (Asad and Dixon 1985) or as 'creative nonfiction' (Narayan 2012; Tedlock 2011). Despite some obvious differences between ethnography and fiction, such terms highlight the important overlaps in the effect that both should have on the reader. Both must give the reader the impression of being inside the story: it should be 'writing that produces the presence it describes' (Tedlock 2011 p.331). This presence is more than just what is 'seen'; it is what is felt, understood, and

appreciated. Ethnography requires the ability to evoke a total sensory experience, a variety of voices and emotions, and a range of compelling theoretical insights. We therefore need a skill set that can capture more than ‘just-the-facts’ but can evoke mood, the obscure, the unseen (Van Maanen 1988: 5). We need to be able to sketch out characters in a way that penetrates into the depths of their lives. ‘Moh is 25. He is a Syrian refugee. He is a student’ just did not do it for me. I did not want to reduce my informants to a simple series of facts; I wanted to capture their individuality and personality. By engaging the reader through small details, such as the way they smoke, their proficiency at a particular game, the way they laugh or stare into the distance, the reader can picture a real human being, who comes off the page and is seen as immersed in life. The facts just do not do that in the same way.

Unsurprisingly then, ethnographic writing is not something that anthropologists can magically do. It is, like everything else in anthropology, a skill which must be honed and trained (Goodall 2000; Narayan 2012; van Maanen 1988). The University of Hamburg offered three seminar series on ethnographic writing over the course of three semesters. In the first we learnt to capture an imaginary ethnographic scene with different writing styles, enabling us to see the impact that style has on a text but also giving us space to experiment with different techniques that each style encompasses. After the first seminar we were asked to invent an ethnographic situation and then capture it in a text written in realist style. I spent the whole journey home on the underground forming ideas and sentences and, most of all, just thinking about combinations of words. When I got home, I whipped out my notebook and began to sketch out some opening lines. Very quickly a paragraph took shape and I felt the same feeling I had felt in art class, a feeling I had never felt for writing before. So, although I was definitely less concerned with the specific style we had been assigned to use and based my scene on an actual event, I had, I believe, discovered two important things – that writing can be enjoyable and that it is not so dissimilar from other artistic processes.

Whilst I was a little too excited during this first assignment to concentrate on style, it is of course something very important to think about, especially when writing an ethnography. The style chosen affects a range of factors – from what sort of audience we will attract to the very story that we can tell through our data (Van Maanen 1988; Kroll 1984). Writing is thus a core part of our data construction (Goodall 2000). In our first seminar series we concentrated on the realist, confessional, and impressionist styles – used to a greater or lesser extent in most ethnographies (Van Maanen 1988). But style is also something that we all possess on a much more personal level. It quickly became clear that even when we were assimilating different styles, everyone had their own distinct style. When reading each other’s work every week, we were able to see how each author’s unique writing style began to shine through.

In this first seminar series, we read a range of ethnographies and reflected on the particular styles and structures of writing each used. What were their first and last sentences? How often did they use the 'I' form? How did they describe a scene? Was their analysis separate from or embedded within the text? The more I read, the more I came to realise which styles and structures I enjoyed and which did not appeal to me. Whenever I found myself struggling to get into an ethnography, I asked myself why this was the case. What was making the text such a chore? Was it the content or rather the way it was communicated? I learnt to read ethnography on an entirely new level, a level that did not ignore format, style, and other literary practices but recognised how writing and fieldwork are tightly interconnected in an ethnography (Goodall 2000).

For swots like myself, being told that we would be graded would have completely changed this experience. I probably would have spent hours reading exactly how to write in a positivist style. Much like writing the ethnography during my bachelor, I would have been preoccupied by the concern of getting it wrong rather than simply concentrating on the practice of writing itself. But free from the fear of a bad grade, I experienced for the first time what it means to simply let go. Even at school, creative writing was always set within a framework of requirements, with long lists of rules and techniques that had to be ticked off. I am not using this article to call for the abolishment of all grading, but in cases such as this one its disadvantages outweigh its benefits (Peckham 2011). Its tendency to confine and even obstruct creative and experimental work would make an ethnographic writing seminar redundant. It was for me and many others the freedom from grading that transformed these seminars into spaces where we could set aside the need to perform and concentrate on creating with words.

The second seminar series was both more intimate and more practical. Every few weeks the group was assigned a social situation to observe and participate in and then to write up our field notes in three pages. Suddenly ethnographic writing was about more than just readable sentences and exploring various styles. It was about the self and the other, relationships, uncomfortable situations, smells, a mood, a thought, a question – a total experience. Describing it drew on one's assumptions, both implicitly in the characterisation of a scene and explicitly in the analytical statements formulated. Writing up these social situations created the space to practice the entire process that defines ethnographic writing. Everything was reduced to a micro scale –two hours in a confined field such as a lift, canteen, or playground and perhaps three hours writing up the notes and thinking about what the social situation means – but that did not lessen any of the key aspects of writing ethnographically. In fact, the brevity of the exercises and even the banality of the social situations (such as riding a lift for an hour) allowed one to notice the less conspicuous details of social interactions and the

broader implications and meanings they have in society. After all, we needed to find enough to fill three or four pages. When we read each other's texts in class, it was enlightening to see how such a narrow field could engender so many different features and perspectives.

The extract below shows how the seemingly banal task of describing our university canteen allowed me to experiment with my position as an observer. I tried out how I could bring my own past experiences into the text and how these biographical memories could help me capture the scene I was observing:

It is the smell that hits me first as I walk into the Hamburg University canteen. As it wafts up my nostrils I am transported back to the dining hall of my primary school, where two crooked dinner ladies slopped slices of grey turkey in congealed gravy onto my plate, before ladling fluorescent custard into my dessert bowl. Although I can't see any turkey, gravy, or custard, it is that same sticky scent of sugar, oil, and meat that greets me here.

In this description I tried especially hard to draw the reader into the atmosphere of the canteen. As the opening of my text, I wanted the reader to feel immediately present, not just seeing but feeling, breathing, smelling, and tasting the canteen as I had. In doing so I hoped to engage my reader through a total sensory immersion.

Since Clifford and Marcus's (1986) edited volume on writing culture, the presence of the anthropologist in both research and writing has become an unquestioned fact in the discipline. However, learning how to integrate oneself into the text is another matter. It was here that discussion and collaboration in class became core to learning how to achieve the balance between the I and the other. As Hess (1989: 169) points out, through discussion students 'learn to grapple with their own ethnocentrism as well as how to avoid writing with either "too much" or "too little" I'. This was indeed demonstrated in our class. Observing similar or sometimes identical situations, yet producing such varied texts highlighted just how subjective ethnography is. By comparing our accounts, we challenged each other: Why did you write about this? Why did you ignore that? What drew you to this specific observation? It forced us to question and account for every single description and understand the implications of every word chosen. This opened up new challenges. When and where does one write oneself into the text and how does one do it without swamping the reader with too much self-reflection? The feedback offered by the other students allowed us to see our own compositions through their eyes and enabled us to grasp the delicate balancing act between the ethnographer and the informant.

Another advantage of these short exercises was the considerable sense of detachment I felt. First, I did not have to identify a theme: the field was decided for me. This allowed me to focus on what was in front of me rather than worry about whether what I had chosen was worth observing. Second, it was not part of a larger text and did not require a large amount of research or grand theoretical postulations. Third, it was not something that had to be rewritten or that I had to fret about. It was simply an exercise of asking myself what I saw in a short, discrete scene, a snapshot of everyday life, and identifying what can be said about it more broadly. This sense of detachment from, and lack of ownership for, the whole process was incredibly useful as it gave me more space to experiment, to try out things that I was not sure about, and to see how my readers – my co-students – reacted to them. It also meant that when somebody critiqued my work, it did not feel like a stab in the heart. It was not a piece to which I had become emotionally or intellectually attached – it was a practice run, a time to have some fun, to try out new techniques and to build confidence in using different styles and structures. It was a freedom engendered by a lack of expectation both from myself and others.

The final seminar series brought everything together. By now most of us had completed our respective fieldwork, so that we were beginning to gain a sense of who we were as fieldworkers as well as writers. With our data at hand, we were almost ready to write our own ethnographies. Whilst these seminars still offered room for experimentation, they were more about how we could transform our raw data into something that was interesting and readable. But, and this is important, it was still a ‘safe writing space’ in the sense that our writing was not assessed, there was no right or wrong, and everyone treated each other’s work with respect and sensitivity. We used three themes/structures that are core to most ethnographies to take our first writing steps towards our ethnographies, namely ‘key scene’, ‘portrait’, and ‘dialogue’.

By this point I had finished my fieldwork, for which I had interviewed a number of refugees whom I had met through a number of non-governmental organisations. I focused in particular on the objects that the refugees took along when they fled and the roles these articles now played for them at their place of refuge in Hamburg, in particular in the act of homemaking. This was the first chance I had to practice characterising a key informant. In the following example I tried to capture something that was key to understanding my informant through the way he moved:

I can always recognise him from afar, not from the way he looks but from the way he purposefully glides across the ground, like a puck forcefully propelled over ice. Zafar is always doing something or is on his way to do something with an aura of determination that seems to seep out of every inch of his body.

The way Zafar moved was indicative of his drive and determination. I wanted to bring this out without resorting to bland statements but rather by capturing his essence through the use of a simile.

This third seminar series was also a chance to turn field discussions (ranging from sporadic chats to planned interviews) into precise dialogues:

Though I never asked about Zafar's mother, she somehow always made it into our conversations. It was his very first response when I asked him what he brought with him from Syria: 'I wanted to bring my mother'. One rainy afternoon when Zafar was feeling a little down, wondering whether he would ever be able to find a wife, he stopped walking and said to me: 'But first my mother must be here – mother comes first! More important than wife, children'. Zafar, like many of the other Syrian men I have got to know, lived with his mother until he had to leave Syria.

Conversation requires more than the reimposition of dialogue: it must capture the way conversations stop and start, the ways themes recur, and what is hidden behind or implied by the simple words.

Our seminar group was much smaller than for the two preceding seminar series, with just eight students. We split into smaller working groups and met every few weeks to discuss our first drafts, and sometimes again to reflect on second drafts. In all seminars we read each other's work and, in a profoundly collaborative manner, had lengthy discussions and gave feedback both inside and outside the classroom. In what was a very intimate engagement I learnt the unique processes that each of my peers went through when writing ethnographically. Some wrote their best pieces on the train, in a stream of consciousness, whilst others liked to think things through first. Experimenting with different settings, structures, and writing aids, I discovered that chatting prior to writing was central to my own process. The ease of speaking words and not having to see them on paper took away some of the pressure that can come with sitting in front of an empty page. Having a casual chat with a patient listener allowed me to access crevices of the mind where thoughts and ideas lurked that I did not even know I had. Speaking acted like a valve, facilitating the flow for when pen went to paper. Having someone listening, questioning, and exposing my weaknesses gave me a way of navigating my own ideas before writing. Likewise, having someone to speak to after a first draft helped me identify things that had not made it onto paper or that, in the excitement of the process, had become unclear or unnecessary. These friendly and informal discussions with peers were crucial to my learning process; it is a process that is often less possible with lecturers.

These writing courses were without doubt the highlight of my time in the Hamburg anthropology master's programme. They provided me with a

core skill for anthropology, a skill that until that point had been ignored in my studies. It is the skill to transform a scene, a conversation, or an idea into a readable and enjoyable narrative. With each class my confidence in writing grew. I discovered a passion for words, metaphors, and scene setting. I now have a fuller understanding for the potential that words carry, a sense of how to narrate a complex experience in the field so that the reader can feel present and engaged.

These seminars provided a space that is not always available at university, a space where freedom, experimentation, and collaboration were given priority; a space free from grades, strict guidelines, and the pressure of conforming. This freedom created an environment in which I could experiment with different styles, structures, and themes. It was a space in which collaboration was central from beginning to end. It was not collaboration in the sense of doing a team presentation but a much deeper form of collaboration, a continuous reflection on each other's work, ideas, and writing processes. Whilst creative writing is an incredibly personal process, learning how to do it and to improve it requires the reactions, inspiration, and opinions of others. We need more spaces like this, not just in the anthropology master's programme but also in its undergraduate programme. Ethnographic writing presents many challenges, but with the right environment, fledgling writers can blossom into talented storytellers, creating ethnographies which can immerse the reader into the lives of others.

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