

Theory and History in the Human and Social Sciences

Thomas Stodulka
Samia Dinkelaker
Ferdiansyah Thajib *Editors*

Affective Dimensions of Fieldwork and Ethnography

 Springer

Theory and History in the Human and Social Sciences

Series Editor

Jaan Valsiner
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Series Foreword

The Real Humanity of Research

From Suffering to Knowledge

This volume is a breakthrough—overcoming the consensual social norm that denies the affective origins of scientists’ passion for knowledge. Contributions to this book—coming from the framework of anthropology—prove to all social sciences that the basis for all new knowledge is the affective goals-oriented subjective striving by researchers who are dedicated to their fields and are ready to endure various kinds of hardships in their ways. Whether this entails anthropologists’ frustrations during their fieldwork or a “number crunching” sociologist who tries to understand how society works from meta-analyses of “big data,” the human affective relating with the desire to find out something new is shared across all sciences.

Maintaining the image of *rationality of science* is possible only through its opposite—that of the deep irrationality of the researchers who take risks of being burned on a stake or discredited for offering seemingly unrealistic ideas that, decades later, become recognized as major breakthroughs. Such passion for objectivity in science is admirable in its persistence. We find ways to experience the curious pleasure of the pain of our grant applications being turned down, submitted papers only to be rejected by journal reviewers, and our university administrators forcing upon us mundane tasks that have no connection with knowledge creation. We feel frustrated—yet ready to go on, as our intellectual goals are personally, deeply crucial for us. Science is a subjective and affective solution for personal life dedication. It is a kind of liminal plane of existence from which there is no return; we can only forge ahead towards knowledge. If we are lucky and persistent in suffering through the hardships of such pilgrimages, these hardships may reach their destination. But they also can be aborted halfway, as we see promising researchers turning into administrators or perpetuators of existing knowledge.

The process of creating new knowledge is inherently ambiguous—requiring researchers to accept their role of constantly moving ahead amidst all the uncertainties of their exploration. Scientific discovery:

...reveals new knowledge, but the new vision that accompanies it is not knowledge. It is *less* than knowledge, for it is a guess; but it is *more* than knowledge, for it is a foreknowledge of things yet unknown and at present perhaps inconceivable. Our vision of the general nature of things is our guide for the interpretation of all future experience. Such guidance is indispensable. Theories of the scientific method which try to explain the establishment of scientific truth by any purely objective formal procedure are doomed to failure. Any process of enquiry unguided by intellectual passions would inevitably spread out into a desert of trivialities. (Polanyi 1962, p. 135)

Tolerating this permanent state of liminality is the life-course adaptation task for researchers. It is far from being an easy task.

The present volume provides many examples from the field of anthropology, demonstrating that affect is the center of all of our knowledge creation efforts. Years ago, George Devereux (1967) pointed to various ways in which researchers adjust to uncertainties. The new material in this book adds to our basic understanding of the real world difficulties in the field and of the ways in which ordinary human beings—with the assumed, created identity of *anthropologists*—cope with all the various misperceptions of their roles as outsider-researcher and suspicions about their magical or administrative impacts onto the lives of the communities they study. Development of trust in the *other* is essential for productive fieldwork—yet it is a fragile interpersonal state that can vanish in an instant.

The trust—or its absence—can be mutual. In order to be let into the affairs of the ones a social scientist wants to study—anthropologist, sociologist, or psychologist—a counter-investigation of the researcher takes place¹. Who is she (or he) coming here to penetrate into our ordinary, extraordinary, mythological lives? What is the potential danger of letting the researcher in? There exist regular social norms for accepting a guest—but not for accepting a visitor who wants to peep into our local affairs as a kind of spy. Guests are traditionally honored; spies are despised. And nothing can save the fieldworker from being stigmatized when deviating from the role of guest. The anthropologist's home institutions do nothing to protect the rights of the researchers. They only seem to worry about the people who are being studied, while the people who do the studying are left to their own resources.

The “seem to” here is accurate. The institutional “protection of human subjects” as it proliferates across the world is inherently ambiguous. Even as research institutions have developed elaborate rituals for “protection of the research participants” and are telling us about these, why should we—the objects of their research—trust them? They say our participation is *for science*, but what does this mean? Our ordinary lives are filled with practical needs within which such claims make no sense. Are there any benefits for us? Here these institutional gatekeepers would be the strange visitors: do they understand what creates importance for us?

¹See Günther (1998) for a vivid description of such counter-investigation in a US university context.

The answer here is of course “no.” They cannot; there are very few connections between their worlds in ivory towers (nowadays perhaps better called data manufacturers) and ours in the jungles of everyday life. And we keep living in ours, not theirs. They may come for short visits, but we remain. What they call “participation” with “informed consent” while asking us to sign the appropriate forms is a confusing act. We have agreed to be studied by our word of honor, so why a piece of paper with our signatures? We may sign these—for us that is an act of generosity towards the guests and a part of our normative hospitality towards people who visit us from afar. So the first—and maybe only—benefit we get from participation is proof of our own hospitality.

What the reflections by the anthropologists tell us about the affective saturation of the field experience in the social sciences goes far beyond the practical questions about how to survive the fieldwork and what kind of evidence it might bring. Phenomena similar to the fieldwork experiences are there in psychologists’ consultation offices, and in the realities of sociologists trying to solicit a “random sample” from a nonrandom social community. It is a version of the general process of human communication that has been posited by Karl Bühler (1934) under the notion of the organon model. It antedates the Shannon-Weaver model of technical communication—widely but inadequately applied to interhuman communication—by around a decade. A version of the model for the special case of the research act is given in Fig. 1.

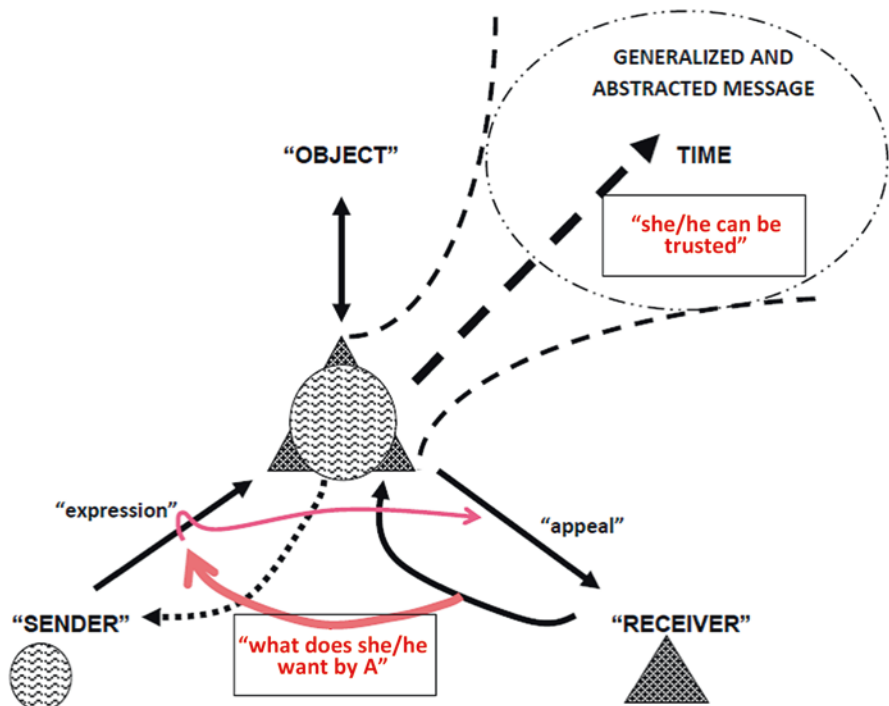


Fig. 1 General model of communication in the research process

The original Bühler model involved the object (about what communication was going on), the sender (who encodes message A—denoted in the figure by a circle—about the object), and the receiver (who interprets the sender’s message in one’s own terms—denoted by the triangle). The critical, crucial implication of Bühler’s model is that interhuman communication is always approximate—we may refer to the same object but its meanings for the sender and the receiver ordinarily do not fit one another’s. This has two consequences: the constant need to specify what the other meant and freedom for possible innovation of the meanings of the object. Our capacities to leave the immediate objective reality of the object to encode it in humorous, sarcastic, or moral terms is an indicator of this freedom.

When viewed as an example of research encounters—in the field or in a laboratory—Bühler’s scheme includes two further features. First, the goal orientations for encoding a message by the sender are present. The researcher *wants something* from the receiver (the “expression” entails suggestions of how to receive the message), and the latter—in addition to receiving the message—is involved in the detection of the meta-communicative agenda involved. (“What does she/he want?”) It is dependent upon the *interpretation of the intention* that the receiver responds—with very different possible tactics ranging from joining in the communicative act to pretending to join in (“empty talk”), redirecting the conversation, or outright challenging the intentions.

If the communication process continues over time, then the second feature, trust, may emerge. Trust is a meta-communicative field-like sign (see Valsiner 2014 on the point-like and field-like signs) that is generalized by the person (in sender and receiver roles) to mark the messages communicated and their intentions. Statements like “I do not really understand what the author of this Preface wants to say *but I trust him*” is an example of such meta-communicative marking. As can be seen from Fig. 1, the trust is a catalytic condition that makes substantive communication possible. It does not cause any of the phenomena discovered in the research process, but its presence makes it possible to bypass or inhibit the barriers that protect the insiders’ knowledge from outsiders “peeping in.”

The dialogue about affectivity in the research process that is initiated by the present volume is a result of an intellectual revival in the social sciences. The new collective interdisciplinary effort—Berlin School of Affective Scholarship—is a good example not of the return of the center of intellectual gravity of the social sciences to Berlin but a manifestation of transnational, collaborative efforts floating within and between globalized academic landscapes. The young, globalized, and cosmopolitan Berliners of today—a multicultural and transnational group of social scientists—are working in our twenty-first century towards a real synthesis of ideas in the social sciences. The readers of this volume have the privilege of entering into an intellectual dialogue with this new wave of scholarship that is likely to lead to new—affectively completed—understanding of the world.

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Natashe Lemos Dekker Through ethnographic fieldwork in nursing homes in the Netherlands, Natashe Lemos Dekker's PhD in Medical Anthropology at the University of Amsterdam research addresses the social processes and the moral values in death and dying with dementia. She assesses the politics of death and dying by questioning normative conditions for the production of lives worth living. As a Research Fellow at the Leiden University Medical Centre, she studies the implementation of palliative care in nursing homes through a critical evaluation of palliative care tools for observing and marking the end of life.

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Marcos Andrade Neves' research is located in Medical Anthropology. His current research is about transnational mobility in the context of organized assisted suicide, mainly among Switzerland, Germany, and the United Kingdom.

Judith Okely's research interests include gypsies/travelers. Professor Okely has published widely on these issues. Her research interests also include anthropological fieldwork methods. Her *Anthropological Practice* (2012) draws on extended

dialogues with 20 anthropologists (of 16 nationalities), having done individual fieldwork around the globe. Recent publications also focus on gender issues. Professor Okely has lectured and taught in more than 20 countries. She has also given distinguished lectures at the London School of Economics and Helsinki, and she is Deputy Director of the International Gender Studies Centre at Lady Margaret Hall, Oxford University. In 2011, Judith was awarded a medal by the Faculty of Philosophy, University of West Bohemia, as a “World Scholar” and also received the Seal of the City of Pilsen, in the Czech Republic.

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Julia Rehsman working on liver transplantation, is exploring “waiting” as a dynamic and contingent phenomenon in a high-tech medical field. Her interests lie in the political, moral, technological, spatial, and intimate configurations of “waiting for a liver.”

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Paul Stoller is Professor of Anthropology at West Chester University. He has been conducting anthropological research for more than 30 years. His early work concerned the religion of the Songhay people who live in the Republics of Niger and Mali in West Africa. Since 1992, Stoller has pursued studies of West African immigrants in New York City. Those studies have concerned such topics as the cultural dynamics of informal market economies and the politics of immigration. This extensive record of research has led Stoller read and think deeply about the anthropology of religion, visual anthropology, the anthropology of senses, and economic anthropology. Stoller’s work has resulted in the publication of 15 books, including

ethnographies, biographies, memoirs, as well as three novels. He has won many awards, including the Anthropology in Media Award from the American Anthropological Association in 2015. His most recent book is *Adventures in Blogging: Public Anthropology and Popular Media*.

Annika Strauss areas of interest include the social anthropology of psychiatry, social anthropology of organizations, gender and sexuality studies and methodological reflexivity, and self-reflection in the context of social anthropological fieldwork and teaching.

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Foreword: Pathways of Affective Scholarship



James Davies and Thomas Stodulka

This book is the result of considerable struggle, within the social sciences and beyond, to both acknowledge and determine the enabling role of affect and emotion in social science research. Drawing together a group of relatively young scholars in the realm of affective research, this volume offers powerful examples of how the affective dimensions of fieldwork have empirical and methodological worth.

For the past 6 years, scholars at the Freie Universität Berlin and beyond have undertaken a systematic and empirical-rooted analysis of fieldwork experiences, experimented with alternative fieldwork methodologies, and, in the process, *have* generated critically important qualitative and quantitative data that further illuminates how our subjective reactions to the conditions of the field can be epistemologically informative. The approach adopted in this volume is a relatively young one in the history of fieldwork methodology, which, at its most basic level, can be broadly divided into two sub-streams of enquiry—*traditional empiricism* and *radical empiricism*. As each of these traditions has approached the researcher's subjectivity in the field in distinct ways, it is important that we identify where this current volume sits within these streams of enquiry by way of first providing a brief synopsis of each.

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Traditional and Radical Empiricism

Traditional empiricism is generally regarded as a methodological approach more concerned with studying “things themselves” than the *relations* between things. As an approach, traditional empiricism—in both quantitative and qualitative research—thus considers subjectivity as something to be controlled and restrained, rather than as phenomena to be considered empirically informative (Davies 2010b). Traditional empiricism has, therefore, drawn firm lines between the researching subject and the researched object, carefully defining across the social sciences what attributes of the researcher can usefully contribute to the activity of knowledge construction—namely, rationality and the capacity for detachment. This approach therefore implies that attributes such as encroaching feelings or affects have to be methodologically removed or subdued, as they invariably introduce irregularities that cloud and bias research (ibid.). For traditional empiricism, then, the researcher’s emotions or affects are seen as impediments to understanding, more disabling than enabling of the work researchers do. Traditional empiricism, if discussing emotions at all, only does so from the empirical standpoint of offering advice upon how emotions could be “managed” and “tamed” in ways freeing fieldworkers to undertake more unclouded research. While this approach has dominated official fieldwork manuals written not only for ethnologists but also for psychologists, cultural theorists, sociologists, and educationalists, many anthropologists have, however, remained privately—if not always publicly—committed to taking seriously the value of fieldwork’s intersubjective and experiential dimensions. Indeed, as we have written elsewhere:

Many of these anthropologists [who take the intersubjective nature of fieldwork seriously] share affinity with feminist theorists who have fought to retrieve emotion and subjectivity from marginal spaces. The abandonment of emotion into zones of pathology, radical and racial otherness, into the feminine, the outlawed, the exotic, the mad or the bad, is part of a wider traditional empirical movement where the emotional, as Catherine Lutz has criticised, is “considered as an unfortunate block to rational thought” (Lutz 2001: 104). If emotion is linked with irrationality, and the irrational with a kind of distorted vision, then emotion is simply grit in the eye of rational inspection. The syllogism misleads (as all syllogistic fallacies do) when empirical work produces data that contradict the syllogism’s first premises. And such data now increases, if only on the margins. (Davies 2010b, p. 12)

The “data” referred to in the above quotation is what *radical empiricism* embraces. This is illustrated by first noting that radical empiricism has its roots in the work of the philosopher and psychologist William James, influenced by a suite of earlier theorists and philosophers working in phenomenological and interpretivist traditions of the late nineteenth and early twentieth centuries. Intellectuals such as Gottfried Herder, Martin Heidegger, and Wilhelm Dilthey invoked the ideas of *Einführung* (feeling into the world), *Gestimmtheit* (attuning to the world), and *tonalité* (adjusting to the pitch of the world), all urging that participation and detachment were methodological postures that could each reach distinct species of fact and that therefore *both* belonged in social research. This view was also implied in Max Weber’s insistence that the observer and observed were constituted of the same

human essence—an idea grounding the concept of *Verstehen*: knowing through empathic attunement (Davies 2010b). This stream of radical empirical thought has been taken forward by more recent anthropologists such as Michael Jackson (1989, p. 3), who “stresses the ethnographer’s interactions with those he or she lives with and studies, while urging us to clarify the ways in which our knowledge is grounded in our practical, personal and participatory experience in the field as much as our detached observations [and] makes the interplay between these domains the focus of its interest”.

While many social/cultural studies scholars have conceptualized the aforementioned “interplay” as the site where the “intersubjective” dimension of ethnographic work resides, what has often been overlooked within paradigmatic debates on ontology and epistemology is reflection on the role of the ethnographer’s emotional role within intersubjectivity and its epistemological relevance. For example, hands-on methods to aid translating the researchers’ emotions into a valid set of complementary data have rarely been pursued beyond ethno-psychoanalysis case studies (e.g., the use of psychoanalytical concepts such as “transference” and “countertransference” to facilitate understanding of emotions in the field (Crapanzano 2010; Devereux 1967) and ethno-psychoanalytical data interpretation workshops (Bonz et al. 2017)). However, more recently, Davies (2010a), Spencer (2010), Svašek (2010), and Burkitt (2012)—by highlighting the methodological significance of emotions as arising between ethnographers and their interlocutors—have set the path for an inquiry into the pragmatics of how to practice an emotional reflexivity that does not begin at the period of writing up or post-fieldwork supervision (hence “after the fact”) but starts at the very onset of fieldwork itself.

The aforementioned move can be classified as *radical empiricism*, as it implicitly questions the traditional empirical advocacy for the researcher’s emotional detachment from his or her data. It requests a research methodology that is pragmatic insofar as it aims to translate the emotions that arise from and influence field relations into a complementary set of ethnographic data for further analysis and interpretation. Further questions still remain, however, regarding how best to practice such emotional reflexivity in the field and how to communicate it via ethnographic analysis and writing. These problems are methodologically and institutionally challenging. With few exceptions (Robben and Sluka 2012), method handbooks and academic emotion regimes still reproduce narratives that fieldwork is a *rite de passage* that novices must traverse “no matter what.” Whatever the personal and professional cost, it is a methodological requirement that anthropologists immerse themselves and affectively relate to others’ life-worlds as empathetic and compassionate fieldworkers, in order to “blend in” or “grasp” informants’ ways of feeling, narrating, and navigating through their local worlds. And yet, fieldworkers are also expected to metamorphose into detached analytical scientists upon return to the academic site, where their emotions and immersions are transfigured into scientific disturbances (Davies 2010b; Stodulka 2015). To counter this contradiction, radical empiricism advocates an epistemological position that places the researcher’s empathy and emotions at the heart of ethnographic knowledge production, whatever the site (ethnographic/academic) from which that knowledge is produced.

A methodology that takes the ethnographer's affective disposition and positionality into account assists in translating affecting and affective field experiences into language that speaks to those who have not "been there" and so do not share the ethnographer's privileged, involved, and long-term fieldwork experience. Radical empiricism opens up alternative ways of researching and writing about field relations. It pursues a thorough documentation of emotions related to encounters with research partners, interlocutors, and informants. By fully acknowledging affects and emotions' relational dimension (i.e., the *in-between*)—as they arise from and influence the social relationships and encounters with informants, interlocutors, collaborators, research partners, and physical or virtual field sites—radical empiricism highlights their epistemic dimension.

This Volume

Many of the scholars of this present volume associate with what has been dubbed the "Berlin School of Affective Scholarship"—a network that has been driving more recent developments in combining methodological aspects of traditional empiricism with the epistemological tenets of radical empiricism. This volume, as does the network more broadly, comprises anthropologists, sociologists, philosophers, psychologists, and literature scholars (e.g., Dinkelaker 2019; Keil 2019; Liebal et al. 2019; Selim 2018; Shah 2018; Stodulka 2017; Stodulka et al. 2018; Thajib 2019) who have gone beyond merely theorizing on the epistemological relevance of emotions in the field, venturing into the pragmatics of translating diverse affective experiences into useful anthropological data, by way of developing for fieldworkers a suite of fieldwork techniques (Lubrich and Stodulka 2019; Lubrich et al. 2017; Stodulka et al. 2019). This book brings to awareness many ways in which the affective reactions to the condition of the field enable rather than impede processes of anthropological and social scientific knowledge construction. It aims to advance methodological reflections and practices of affective scholarship and invites other disciplines from the human, cultural, and social sciences to engage in critical and constructive methodological dialogue. In short, it is a valuable addition to critical epistemological debates that embrace the body, the personal, and the emotional in empirical research, and in particular fieldwork and ethnography (Behar 1996; Briggs 1970; Favret-Saada 2012; Haraway 1988; Koivunen and Paasonen 2001; Okely 2012; Stoller 1997; Wilce 2004). It attempts to translate the researcher's subjectivity into empirical and theoretical insights through self-reflexive engagement with ethnographic methods and fieldwork experiences. Moving beyond the dialectics of scientific objectivism and sentimentalist subjectivity (Cerwonka 2007), traditional and radical empiricism, this book delineates the researcher's affects and emotions as critical processes of anthropological analysis and representation.

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Introduction: Affective Dimensions of Fieldwork and Ethnography



Ferdiansyah Thajib, Samia Dinkelaker, and Thomas Stodulka

I have the feeling that most of the negative emotions come from either cultural differences (in which I am confronted with my ethnocentrism and interpretation of justice) or from the feeling that I am not doing fieldwork well, that I am being too lazy, that I am not getting enough data, that people don't have or don't make time for me and that I have no right to expect that from them. Positive emotions on the other hand are, I think, intimately related to feelings of doing my fieldwork successfully (having an interesting interview/FGD, having gotten to an important insight, etc.) but actually mostly they are related to having special interactions with the people around me, being inspired by them, feeling affection for them and their affection for me, and having a feeling of a real connection and friendship with the people who my research is about. So I guess, very simply put, it is *production* and *affection* that determine both the positive and negative emotions that I feel during fieldwork. (Fieldwork reflections by a colleague analyzing her own emotion diary in retrospect, 2015)

This book explores the role of researchers' emotions and affects in understanding "the field." Whichever methods ethnographers apply during field research, however close they come to be to their informants, and no matter how involved or detached they feel, fieldwork pushes anthropologists to constantly negotiate and reflect their scientific subjectivities and positionalities in relation to the persons, communities, spaces and phenomena they study with. The quote above exemplifies fieldwork challenges and gratifications that anthropologists have widely discussed and debated in terms of fieldwork ethics (Caduff 2011; Caplan 2003; De Laine 2000; Dilger et al. 2015; Fluehr-Lobban 1991; Scheper-Hughes 1995; Stoczkowski 2008), methodological practices (Amit 2003; Beatty 2010; Okely 2012; Rabinow 1977; Rosaldo 1989; Sanjek 1991; Sluka and Robben 2012; Stoller and Olkes 2012), colonial traditions inscribed in ethnographic encounters (Asad 1973; Smith 1999), and modes of

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ethnographic representation (Clifford and Marcus 1986; Fabian 1990; MacDougall 1998; Tierney and Lincoln 1997). This volume focuses on methodological implications that bring to awareness the potentials of researchers' affects and emotions that enable more than they hinder processes of anthropological and social scientific knowledge construction. We found this collaborative book project on our own prospects (Davies and Stodulka 2019; Stodulka et al. 2019; Stodulka et al. 2018) that particularly extend on classic and recent contributions from psychological and feminist anthropology (Behar and Gordon 1995; Crapanzano 2010; Davies and Spencer 2010; Fischer 2018; Hollan 2008; Hollan and Throop 2008; Narayan 1993; Rosaldo 1980; Wolf 1996; Visweswaran 1994). This book is our bid to provide intellectual space for methodological and epistemological debates, and expound the potentials and the limits of affectively aware scholarship by means of comprehensive and illustrative ethnographic case studies.

Affect, Emotion, Fieldwork, and Ethnography

This book focuses on methodological reflections and ethnographic case studies in relation to what the authors of this volume experienced as affects, feelings, and emotions during and after fieldwork. Abstaining from convoluted theorizing, we discuss them in relation to the practices of fieldwork and ethnography, and define them loosely as embodied social and relational processes and experiences (Burkitt 2014; Röttger-Rössler and Stodulka 2014; White 2017). As minimal definition, the authors of this book agree on very basic theoretical assumptions: emotions link affects (as bodily, sensory, inarticulate, and sometimes nonconscious experience) with surrounding local worlds by way of shared or recognizable modes of communication, articulation, and feeling. Emotions are linked to cultural repertoires that enable persons to express their own and label others' observable or imagined affects and feelings. Such expressions can occur through shared and communicable emotion words, through gestures, symbols, or body movement. Emotions can be performed and enacted without significant changes in communicating a person's physiological arousal and experience. They motivate action and interaction, and relate to social, cultural, economic and physiological needs and wants. Their display and articulation are affected by and affect others. They are critical in relating people to or disconnecting them from each other (Stodulka 2017a, b, c).

Considering their experiential, psychological, political and social qualities, we argue that their understanding is vital in navigating and making sense of everyday lives and environments. In short, everything emotions do (and can do) should be considered directly relevant to ethnographic fieldwork practice that we understand as series of encounters with persons, places and objects in complex and diverse constellations of power asymmetries. Affects, feelings, and emotions influence and manifest the experience of these encounters, and vice versa, encounters influence

and shape researchers' and interlocutors' emotions, feelings, and affects. Ultimately, they affect what textbook methods (sampling, surveying, interviews, FGDs, systematic observation, participatory, and artistic approaches, or field experiments, and so forth) researchers apply, and how, when and with whom they engage in them.

As illustrated in the quote that prefaces this introduction, field researchers grapple with a wide range of emotions: the happiness after establishing social relations; the pride of "belonging" to host communities; the fear and anxiety not to produce enough data or to do fieldwork "the wrong way"; the disappointment when we feel that we never really belonged to our host communities; for some, the guilt related to colonial heritage and/or privileged life compared to many of our research subjects; the insecurity how to reciprocate hospitality and shared knowledge; or the panic that sneaks in, when we feel that we are not doing enough, or realize that we have to "wrap up" and leave soon. They constitute, as we have elaborated elsewhere, as "field emotions" (Stodulka et al. 2019), or "field affects" (Stodulka et al. 2018), that are generative in the formation of ethnography, and deeply influence our mode of knowledge production—and that hence transpire as epistemic.

Emotional experiences during fieldwork do not only prevail within the blurred boundaries of research. They transgress such idealized work-life dichotomies, and become part of researchers' lives, where they stick and resurface. When compared to other methodologies, the fundament of fieldwork and ethnography is a participant observation, in which researchers immerse themselves into the lifeworlds of the persons, objects, and communities they study (with). As an academic endeavor that is described as the most humanist among the sciences and the most scientific among the humanities (Sluka and Robben 2012), the anthropological fieldwork *persona* is professional researcher and private person at the same time (Leibing and McLean 2007). Similar to "the field," which is increasingly experienced as unbounded psychological, historical and social relationship between researchers, their tasks, and experiences vis-à-vis the lifeworlds, the people, spaces, and places they study with rather than a geographic entity (see Gupta and Ferguson 1997; Marcus 1995; Spencer 2010), ethnographers' minds and bodies too dissolve into their work—and their work seeps into their personal lives, loves, terrors, and dreams.

We propose that, if attended to systematically, carefully and reflexively, emotions—as relational phenomena between researcher and the researched—can be helpful in constructing, unearthing, and representing ethnographic knowledge. The contributions of this volume convey that attending to researchers' emotions as epistemic, understanding them as relational and complementary data can be scientifically rewarding. This book is an appeal to highlight their importance in ascribing meaning to the phenomena ethnographers study. Training fieldworkers' emotional literacy (the capacity to discern and name affective experience in relation to someone or something), by encouraging techniques to document their emotions systematically, promises to enhance researchers' emotional reflexivity and support affective ways of researching, reflecting and representing "the field" as ethnographic knowledge.

Coincidences of Desire: Emerging Affective Scholarship

The groundwork for this book emerges from a transnational and interdisciplinary research collaboration between anthropology, primatology, and literature studies, titled “The Researchers’ Affects.”¹ After senior colleagues had rejected our ideas at reviewer panels twice as “unscientific,” we were able to acquire research funding in a third attempt. When in addition to the three project leaders a team of five doctoral students had been formed, we went on to explore the roles of affects and emotions in ethnographic and primatographic knowledge construction, from the choice of research subjects, to the researcher’s positionality, the generation of knowledge and data, and their interpretation and public representation. To this aim, we have studied fieldworkers’ affects and emotions from different disciplinary angles (Keil 2019; Lehmann and Stodulka 2018; Liebal et al. 2019; Lubrich et al. 2017; Shah 2018; Stodulka et al. 2016; Suter 2016), as well as refined and developed new methods to identify and make use of these epistemologically (Lubrich and Stodulka 2019; Stodulka et al. 2019).

With regards to our own discipline, we devised semi-structured emotion diaries that could assist fieldworkers in the systematic documentation of affective experiences during fieldwork.² Systematizing documentation, we assumed, helps fostering “a habitual mode of affectively aware perception and attention to the researched phenomena” (Stodulka et al. 2019). To gain empirical insights on the affects and emotions at stake during fieldwork, we collaborated with over 20 early-career ethnographers (mainly from the discipline of anthropology) who employed the provided emotion diaries during their fieldwork, shared emotional episodes during semi-structured interviews, and participated in word sorting tasks, open and closed word listings. The majority of the researchers extended their collaboration with the project by contributing more than half of the case studies compiled in this volume.

This book also owes gratitude to a series of “lunch-break” and other informal discussions of the “Affective Epistemologies” working group at the Institute of Social and Cultural Anthropology, Freie Universität Berlin. The group comprised of fellow junior researchers who all shared concerns regarding the lack of systematic, rigorous and critical reflection on the roles of emotions and affects before, during, and after fieldwork. In retrospect, this confluence of interests and needs was not coincidental given that at this period of time, most of us were either still in the process of preparing, taking breaks, or just returning from our respective fieldwork. As a regular meeting platform for almost 2 years, this working group also functioned as a peer support group among junior scholars, in which they could engage in structured discussions and analytical exchanges around the topics of emotions and affects in the field, independent from the official academic curriculum. The collaboration with members of the working group translates into this book through the majority

¹ The project was directed by Katja Liebal (primatology), Oliver Lubrich (literature), and Thomas Stodulka (anthropology) and funded by the Volkswagen Foundation (2013–2018).

² See Appendix.

of the contributions written by some of the group members opening the different sections.

The stimulating journey of reflecting on the affectivity of fieldwork and ethnography culminated in December 2015, when we reached out and invited the collaborating researchers of both groups—participants in “The Researchers’ Affects” project and members of the “Affective Epistemologies” collective—to join a feedback workshop designed to scrutinize the role of emotions in ethnographic knowledge construction. The workshop evaluated the provided formats of documenting and reflecting on fieldwork emotions, and addressed the challenges of translating researchers’ affects into ethnographic analysis and writing. As we discussed along within world café sessions and walking seminars, many different ways of incorporating fieldworkers’ affects and emotions into anthropological research and writing have been brought to the table. Encouraged by the fertile discussions during the workshop, we invited the participants to compile their case studies into a book, which grew into the volume at hand. Other researchers, who had not participated in the workshop, but whom we had encountered along our journeys, joined the authors and added their perspectives on the entanglements of fieldwork, emotions, and affect. The multiplicity of critical discussions generated throughout these collaborations has demonstrated that new articulations on affective scholarships continue to emerge in various academic landscapes. This book endeavors to render initially isolated approaches visible by providing a shared platform, and hopes to encourage the proliferation of new voices and new narratives around affectively-engaged research. It particularly addresses (post-)graduate students and early-career scholars.

Advancing Affective Scholarship Through Empirical Affect Montage

The authors in this volume pay systematic attention to the complex emotional and political dynamics that constitute research encounters. Issues surrounding the relationships between researcher and researched such as shifting positionalities, power relations, differential demands, and expectations take on special focus in the contributions. To foreground the affectivity of fieldwork positionalities, we invited the contributors to allude to emotions’ ontological as well as methodological and epistemological potentials in encountering, documenting, analyzing, and representing “Others.” In addition to a creative “kaleidoscope of methods” to approaching affects and emotions in the field (logging, journaling, therapeutic training, image theater, tandem research, or collaborative interpretation to name just a few), some authors demonstrate particular strategies that foreground communication, understanding and perspective taking as an empathetic endeavor.

In order to integrate both classic theoretical approaches in social and cultural anthropology’s resourceful history, and yet-to-be-systematized affective experiences

in the field, we have introduced the concept of the “Empirical Affect Montage” as an umbrella term that reflects the project’s methodological trajectory. As suggested by the term, our proposed framework rests on the premise of opening up ways for fieldworkers to communicate what was “at stake” in their multiple encounters with the local worlds of their protagonists to readers who have not “been there” (see Stodulka et al. 2019). We define Empirical Affect Montage as a technique to bring the researchers’ documented affects and emotions in dialogue with more traditional accounts of the phenomena they study (e.g., field notes, interviews, memory protocols, transcripts, photographs, video, mental maps, pile sorting, and field experiments), and argue that the montage of different data dimensions “thickens” ethnographic accounts and increases their methodological transparency. We contend that it is through the technique of montage that fieldworkers can make accounts of their affective experience epistemologically productive, without falling into the trap of self-indulgence. We underline that the use of self-reflexive field material in writing up ethnographies pertains “only to the point that the author shows its relevance to the production of knowledge” (Frank in Leibing and McLean 2007, p. 13). Understanding Empirical Affect Montage in a narrow sense, we proposed researchers to supplement conventional research material with documentations and reflections of their emotions, or juxtapose these assumedly separate sources of ethnographic documentation in their analysis and ethnographic representation (for an exemplification see Stodulka 2014, 2015).

Although we have not explicitly encouraged the authors to follow this *one* methodological proposition, for us, the diverse approaches that frame the different contributions reveal a number of interrelated key insights and affordances that the Empirical Affect Montage is compelled to address, and which we frame as multidimensional nexus of affective scholarship.

Dimension 1: Strategic Documentation of Affects, Feelings and Emotions in the Field The first dimension is indicative of a common tendency in the chapters to analyze and describe crucial research results through systematically practiced emotional reflexivity. A great number of authors make recourse to documentations of their emotional experience in emotion diaries, semi-structured feedback interviews, free listing, card sorting tasks, and questionnaires—tools developed in the context of The Researchers’ Affects project. The chapters demonstrate diverse ways of documentation, and the methodological usage of these devices: some authors analyzed individual passages of their emotion diaries in a hermeneutic-interpretative manner vis-à-vis their research questions; others subjected them to a qualitative content analysis. Still others replaced their conventional field diaries with emotion diaries that they adapted to their own needs and preferences in hybrid terms. We are aware that the tools we provided for collaborating fieldworkers are only partial in covering the full spectrum of researchers’ affects. Questions and concerns that were raised by the authors during the Berlin workshop in December 2015 also addressed the potential negative bias of documenting emotion, the repetitive structure of the emotion diaries producing scripted affects, and the strain of engaging in emotional reflexivity during already exhausting enough fieldwork.

Whereas these drawbacks are being addressed in ongoing collaboration and debate, we want to mention some of the benefits of systematic affective scholarship as they were articulated and documented during the workshop: “When feeling alone, or sad, if no one is there to listen to you then it is better to write a diary”; “It was helpful to write down both negative and positive emotions. Writing negative emotions helps to feel light and fresh. Writing positive emotions also helps to feel good”; “It helps to take my emotions more seriously. Otherwise I don’t have time to listen to my own feelings”; “I have translated the helpful questions from the emotion diary to the structure of my online-field blog and field diary.”³ Most of the participants stressed how important the emotion diaries and the feedback interviews that we conducted during fieldwork were to them to better understand, reflect, and communicate the psychological distress and methodological commotion that they went through while in the field. As consensus among the participants it was agreed that systematic affective scholarship also opens doors for sustainable disciplinary, ethical, and psychological supervision during PhD and doctorate projects.

Dimension 2: Emotional Reflexivity as Epistemic Resource The second dimension of affective scholarship concerns the multiple levels of insights that critical and systematic analyses of affective researcher experience are capable of *providing*. A number of contributions link affective pathways or particular emotional episodes to insights related to their respective research topics. Others address how their affective experience instigated reflections that complexify conventional answers to questions of research ethics and the inextricability of fieldwork as intersubjective and representational arenas of dominance, hegemony, and power struggles. In doing so some authors, importantly, address when to actively manage their emotions for intellectual or ethical reasons, as they are dealing with the epistemic violence ethnographic research can be entangled with. Systematically attending to the affective dimensions of fieldwork brings other authors to refine conceptual critiques of fieldwork, and ethnography and anthropology as a discipline. This is exemplified by accounts critical of the efficacious guiding principles within academic cultures of detachment and immunity to unsettling incidents, as well as the obstinate conceptualizations of researchers as atomized individuals free from social and affective relations with supervisors, academic gatekeepers, and campus regimes. The volume comprises stimulating suggestions to conceive of fieldwork beyond dichotomizing concepts of work and leisure, which, for instance, imply an opposition between data generating fieldwork and “non-productive” care work for children, partners, friends, parents, siblings and loved ones. With the authors’ plead not to seek immunity from unsettling experiences both inside and outside the porous boundaries of “the field,” but to recognize the constitutive role of uncertainty and anxieties in fieldwork (Jackson 2010) as well as ethnographers’ vulnerabilities (Behar 1996), they point to the potentials of affectively aware fieldwork by engaging in the existential question of what it means to remain humane in the face of existentially transformative experiences.

³The quotes were documented during one of the workshop sessions by our colleague Nasima Selim.

Dimension 3: Spatiotemporal Configurations of Data Analysis and Interpretation The third dimension of the affective scholarship paradigm assembled in this volume touches upon the temporalities and spatial configurations that inform ethnographers in addressing the epistemic potentials of researchers' affects and emotions. Three methodological configurations permeate the articles of this volume. Firstly, some authors have interpreted their emotional experiences retrospectively. By revisiting central emotional episodes *after* fieldwork (through taking cue from diary entries or other forms of documentation, recollecting sensorial and visceral experiences as well as synthesizing past impressions and present interpretations of specific moments in the field) they were able to better understand the inchoate messiness that comprises field experiences in a different light. Secondly, in reflecting on the critical role of emotions and affects for their research, some contributions describe moments of insights that we call *synchronous*. They include accounts that rely on capturing firsthand emotional and affective responses. They bring to light how affective experience supports in situ processes of attuning to their protagonists' lifeworlds and experiences, and how it fostered their experiential understanding, sensitized them for attuned analysis, and stipulated ethical practices of representation. These accounts echo approaches of attending to emotions as sources of epistemic insights, described as "Key Emotional Episodes" (KEE) (Berger 2010), "raw moments" (Hastrup 2010) or "emotionally sensed knowledge" (Hubbard et al. 2001). The third way in which the authors ascribe meaning to their affective experience and emotions involves a combination of both *retrospective* and *synchronous* reflections⁴ that emanate the researcher's personal and professional transformation (Jackson and Piette 2015; Spencer 2011) and convey its consequences for *holistic* data construction. The volume includes accounts that highlight how researchers' affective encounters remain existentially inscribed into their embodied realities as persons. They retail how existential emotional experiences linger on and continue to implicate researchers' academic practices and everyday livings.⁵

Dimension 4: Researcher's Affective Experiences, Ethnographic Writing and Representation The fourth dimension of affective scholarship aspired by an Empirical Affect Montage and exemplified in this book concerns the role of emotions and affectivity in ethnographic representation and styles of writing. Readers

⁴In a similar vein, Athena Mc Lean and Annette Leibing highlight in their edited volume *The Shadow Side of Fieldwork*, that fieldworkers' reflections on the blurred borders of their personal lives and their ethnographic work are, at least implicitly, mostly a *processural* juxtaposition of "memoires and *past* data, *present* experiences and observations, and vision for *future* praxis" (2008, p. 4; emphases are ours). We propose the spatiotemporal distinction of ethnographic insight into *synchronous* and *retrospective* approaches as a bid to theorize on *how* researchers can integrate various dimensions of affective scholarship into ethnographic knowledge construction through a systematic lens that speaks to the diversity of research practices.

⁵The *New Ethnographer* movement is highlighting such existential and often harmful experiences and provides a forum for anthropologists to address these often silenced issues (<https://thenewethnographer.org>).

may find that in some of the contributions the writing style and the personal presence of the authors are jarring at times. This impression may be attributed to the impetus among many of the authors to address affective challenges in doing fieldwork that contributed to what they consider substantial anthropological insights of their projects. This is not an easy task considering that genres which engage in affective scholarship when it comes to fieldwork and ethnography have so far positioned themselves as e.g. “*anthro-poetics*” (Behar 1996; Rosaldo 2014) or “*auto-ethnography*” (Ellis 2004), and hence if not at the margins, at least distinctive from mainstream “academic anthropology.” Contrastingly, the book introduces writings that exemplify different ways of navigating emotional vulnerabilities that permeate fieldwork encounters as well as the personal and professional lives of academia. The authors courageously tread their ways through the risks and leverages of engaging with vulnerable experiences in written representation. Thus, they illuminate diverging pathways that are poignantly conceived by Paul Stoller in the Afterword to this book as future challenges in writing affective fieldwork and ethnography (Stoller, this volume).

Although we are aware that reflexive ethnographies run the risk of circling around the anthropologist to an extent that is self-absorbing, this book intends to illustrate that systematic relational and affectively aware methodology can generate ethnographies that illuminate others’ experiences transparently and ethically. Taking into consideration the historical perspective of this volume’s preface, we are inclined to carefully position our intellectual project at the intersection between the methodological rigour of traditional empiricism and the inspirational vigour of radical empiricism. This book stands on the shoulders of its authors, and the anthropological trajectories that perceive of fieldwork and ethnography as complex webs of anthropological encounters that weave through our critical reasoning, embodied, and sensorial and affective ways of “learning through the field.” From an ethical and critical epistemological point of view, we contend that exclusively writing about others’ suffering, loving, grieving, celebrating, or mourning without making ourselves vulnerable as ethnographer runs the risk of “pimping out” (Veissière 2009) friends, interlocutors, informants and research partners. It runs the risk of reproducing simplifying dichotomies by putting *them* into emotional “hot seats,” and presenting the anthropological persona as “cool”, and more “reasonable” in abstracting “thoughts” from “feelings,” or “culture” from “nature” (Rappaport 2008).

The Book Sections

This volume is organized along the six core themes of role conflict, reciprocity, intimacy and care, illness and dying, failure and attunement, and emotion regimes in teaching and doing fieldwork. These topics reflect the themes that traversed most prominently during discussions between the Researchers’ Affects-team and the

innumerable interlocutors and colleagues that had shared their insights with us during the last 5 years. As editors, we are aware that this remains a selection of thematic strands as this “meta-field” has presented itself to us, and that they cannot be fully separated from each other. Yet, in order to apprehend and comprehensively represent the complex phenomenon of fieldwork affectivity, strategic editorial decisions needed to be taken. The conceptual background of the respective themes as well as the productive overlaps and intersections between individual chapters will be addressed by thematic introductions that precede each section. Consequently, we will not forestall these detailed lead-ins here, but provide a brief overview of the entire book.

Section 1 comprises contributions that highlight issues of researchers’ *role conflicts* in the field. Fieldwork encompasses moments when researchers’ institutional, social, and political (self-) ascriptions shift, and their initially assumed social identities and subjectivities conflict, collide, or conflate. Shifting positionalities often point to the need for, or result from, oscillating identifications that compel both researchers and interlocutors to negotiate existing or emerging power relations in order to continue the ethnographic endeavor. Each author reflects on the challenges and contradictions in navigating multiple positionalities in the field and their often acute emotional implications. The chapters share a common thread of understanding that a sustained self-reflection of changing and conflicting roles as a field researcher assists ethnographers in coping with different expectations and ascribed responsibilities, and contributes to nuanced analysis, interpretation, and representation of the studied phenomena.

The second section on *reciprocity* discusses the ways of negotiating the asymmetrical relationships of power and privilege in fieldwork through the acts of “giving back.” As a practice which has become one of the central tenets in anthropological tradition, reciprocity is not only tethered to the exchange of material and intellectual resources and compensations. It also involves the affective dimensions of engagement vis-à-vis unequal social roles and divergent motives. The contributions carefully attend to the limits and predicaments of taking reciprocity for granted by transforming affective, ethical, and material values into concrete and practical methods for addressing inherently unequal conditions in the field.

The third section presents *intimacy and care* as a cluster of affective trajectories that continuously blur the lines separating fieldwork as a personal and a professional undertaking. Field researchers often carry out the multiple tasks of sharing intimate information and engaging in caring relationship with those being studied while balancing their familial, conjugal, sexual, and amical relationships, whether separated by physical distance or not. The emotional impacts can be remarkably intricate and ineffable. Moreover, they are often left unexplored or even silenced in the written representation of research outcomes. In contrast to this “customized” representational practice, the writers in this section willfully engage with intimate attachments and caring experiences in fieldwork. In dealing with intimate and caring relationships as affective manifestations of relatedness, the contributors outline

the conditions when intimacy and care can entail both heart-warming moments and heart-breaking dramas, as well as theoretical insights.

Moments of *illness and dying* are fieldwork experiences that are difficult to address. In spite of their increasing salience as a research focus, the chapters in the fourth section confront the emotional difficulties of dealing with research interlocutors, close friends, and kin who suffer chronic illness or face death. A combination of compassion and closeness constitute some of the most prominent feeling-states in studying the role of social dynamics, personal beliefs, and professional investment. At the same time, as the different chapters in this section poignantly illustrate, these feelings also open up questions regarding the emotional consequences for researchers when they become “too close” with the people or situation that they seek to understand. Overall, these chapters offer a sobering reminder to the affective stakes when fieldwork unravels as a site of shared existential vulnerabilities.

The fifth section, addressing *failing and attuning*, brings forth silenced and less jubilant processes of coping with “failure” before, during, and after doing fieldwork. Experiences and feelings of failure may come in varied disguises. They may relate to the methods used, the relationships established, the ethical challenges faced, or the way the physicality of doing research is dealt with, to mention just a few. But as crises can be reappraised as turning points and imply the potential of new beginnings, the contributions also highlight how respective instances of emotional struggle can be overcome and turned into rewarding anthropological insights.

The last section discusses pathways to disrupt hegemonic practices of the social and behavioral sciences that naturalize impartial objectivity and ignore the affective dimensions of fieldwork and ethnography as epistemological project. The authors of the section unpack *emotion regimes in teaching and doing fieldwork* and invite readers to take seriously the affective aspects of anthropological research not only in emotionally responding to environments, places, situations, materialities, and people within “fieldwork,” but also extending them to our anthropological training methods, other forms and arenas of educational practices, as well as our everyday practices and professional interactions as academics.

The authors’ trajectories of systematically reflecting and constructing anthropological meaning from their affective experience during fieldwork cover a variety of thematic strands. Most importantly, they build a methodological cause, and show that courageous, intellectually and emotionally challenging endeavors can very well be worth the (sometimes painful and arduous) extra effort. We hope that readers at different stages of their research and careers will be both challenged and stimulated by these accounts. We are looking forward to critical perspectives and future collaborations that help expanding on what has so far been dubbed as “Berlin School of Affective Scholarship” (Davies and Stodulka 2019). We encourage readers to collaborate and integrate relational methodologies into the design of refurbished research method companions and assert sustainable supervision of PhD, doctorate and other ethnographic research projects.

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Part I
Role Conflicts and Aftermaths

Role Conflicts and Aftermaths: Introduction



Giorgio Brocco and Britta Rutert

Role conflicts and their attendant emotional effects constitute important empirical and ontological points of reflection deserving of scrutiny in anthropological fieldwork. Often, when researchers who are engaged in fieldwork are forced to change their roles and emotional positions voluntarily, or because of external socioeconomic or political circumstances, conflicts and related uncertainties emerge. One example is a nongovernmental organization (NGO) employee who decides to conduct ethnographic research as an independent scholar financed by a university or private institution. Another example is a women's shelter employee who decides to shift to a role as an "objective ethnographer." These shifting roles reflect a conflict surrounding the role of the ethnographer in general, which derives from a continuous oscillation between being a detached observer and being a participant in the field. This conflict can be particularly acute when ethnographic research is conducted in the same setting in which the ethnographer has been employed or has volunteered. Role changes, continuous oscillations from one social, political, economic or emotional role, or "fixed identity," to another throughout the course of fieldwork, may lead the researcher to face personal and collegial emotional conflicts. Anthropologists often conceive themselves as having a distant position and are commonly understood to function in this regard. In this sense, anthropologists assume an "objective-ized" stance toward the people and places they study. Although this objective stance has been widely questioned in academic and nonacademic debate, it remains a prevailing assumption that a researcher will remain "outside" social networks and relationships while in the field, even though anthropological

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inquiry demands shifting positions between emic and ethic perspectives (Daston and Gallison 2007). Others working “in the field,” such as NGOs, company employees, or interns maintain a position of being fully engaged as part of a team, with the all attendant obligations and benefits. Even though the emotionally engaged NGO employee may form part of an idealized narrative when compared to the distanced anthropologist, NGO workers are understood as subjects actively engaged in and modifying their field setting. In contrast, the ethnographer is conventionally required to maintain an observational position without personally engaging with others in the fieldwork setting. An ethnographer’s shift from active to passive team member, engaged to dis-engaged colleague, “insider” to “outsider” may cause irritation and mistrust among former colleagues. Potential future informants and participants may not want to reveal information to the newly declared anthropologist or ethnographer who had been a former work colleague or fellow volunteer. As one author has shown (Bolotta, this volume), it may be easier to convert an ethnographer to an active NGO member. Mistrust or irritation with the anthropologist who shifts from being a team member in the research setting may be due to continuous wavering around the problem of adopting a positivist and distanced position to an emotionally involved position. Shifting emotions, oscillating feelings, and fluctuating ethical and moral viewpoints emerging from multiple role conflicts call into question the anthropologist’s positionality before, during, and after fieldwork with regard to informants, participants, and readers. Both emotions and the researcher’s “rational mind” fully participate in the co-construction of the field (Crapanzano 2010; Davies 2010; Stodulka 2015). Emotions may function as an important source of information during fieldwork and the final data analysis. However, role conflicts and emotional and social oscillation between multiple positions may also pose a challenge when trying to bridge the gap between “insider” and “outsider” for both the researcher and their colleagues in the fieldwork setting.

A closer look at the positionality requires examining role conflicts in anthropological analysis to ensure the epistemological quality of the research process. The researcher must identify, document, and reflect on positionality in their research in order to allow “the researcher to be more open to challenges to [his/her] theoretical position that fieldwork almost inevitably raises” (Turner 2010, p. 126). Positionality became a central theoretical (and practical) concern during the mid-1980s with the so-called “reflexive turn” (Marcus 2009, p. 1; see also Faubion and Marcus 2009; Herzfeld 2009; Taggart and Sandstrom 2011). Literature on positionality has emerged from feminist, poststructuralist and postcolonial traditions that sought to challenge “the methodological hegemony of neo-positivist empiricism,” (England 1994, p. 81; Wolf 1996; Nagar 2002) and question canons of impartiality and objectivist neutrality in research (Lambek 1997). First, a reflection on positionality emerged from the recognition and subsequent critique of anthropology’s complicity with structures of inequality wrought by European colonial expansion and its aftermath (Hymes 1999; Asad 1973; Scholte 1999). Moreover, a feminist critique of anthropology’s androcentric bias, on the one hand, and the subsequent problematization of the objective neutral observer, on the other hand, constituted significant redefinitions of what positionality might be (Chiseri-Strater 1996). In particular,

feminist anthropologists examined and pointed out the ethnographer's position in relation to interlocutors. In so doing, the question of objectivity and representation was further recast by refocusing attention on the form, rather than the content, of ethnographic writing (Clifford and Marcus 1986; Marcus and Fischer 1986). Since this reflexive turn, debates on positionality have become fundamental to ethnographic writing, with distinct and oftentimes personal decisions being reached about how to address it. An analysis of researcher positionality through role conflicts leads to an emphasis on how knowledge is socially constructed, situated, and embedded in particular (unequal) power relationships and structures. Positionality also opens methodological and epistemological trajectories to explore how power may be replicated and brought to light in the research process (McDowell 1992; Stanley and Wise 1993; England 1994; Rose 1997; Mohammad 2001; Chacko 2004).

By reflecting on an anthropologist's various shifting roles, emotions, and potential conflicts during fieldwork, a systematic consideration of role conflicts offers important empirical and theoretical insights related to the research and subsequent writing process. The chapters in this section reflect on the diversity of research positions, unsubstantiated mistrust, and even emotional (ab)use that ethnographers may be exposed to.

Giuseppe Bolotta offers an example of the emerging role conflicts that inevitably arise in the shifting roles and positionalities that unfold during long-term fieldwork. Bolotta engages with a critical analysis of his own position as a researcher and activist in the humanitarian field of child-related NGO work in Bangkok. Assuming a stance of "emotional reflexivity" (Spencer 2010), Bolotta examines his emotions while working with *dek salam* (slum children) in Bangkok. His a priori approach to his understanding of *dek salam* is highly influenced by his work as a psychologist for the NGO Psychologists without Borders years before he started his "anthropological" fieldwork. During his research, Bolotta realized he was involved in reproducing the victimizing language, a "compassionate ethos" (Fassin 2005), used in humanitarian language. This became particularly clear to him when he changed his position from an NGO activist and psychologist to an ethnographer conducting research with *dek salam*. By approaching the children as victims, he was soon "exploited" by one of his interlocutors, a boy who re-invented his history as a grieving child who had lost his family. It took Bolotta some time to understand that although he had been seeing slum children as victims, he was in fact being steered by his own exposure to this humanitarian language. Bolotta's paper shows how the slum children are aware of the willful naivety of Western NGO members, especially in their efforts to save children and their unwitting use of "emotional economy" strategies (Stodulka 2014, 2015). It took many years of living with the slum children that Bolotta's "top-down" emotional relationships with the children changed to friendships and true compassion, resulting in an affective understanding of the protagonists in his research.

Marina Della Rocca reflects on her journey from working at a shelter for migrant women in northern Italy to becoming an ethnographer at the same shelter. Her positional shift entailed emotional challenges and trepidation for her as a researcher, for her former colleagues, and for the women in the shelter. To engage with the shift of

her positionality, she emphatically asks herself how the emotional experiences of attachment or/and detachment concern researchers as persons and anthropologists. She questions how we, as researchers, can manage emotions and subjective positions while pursuing our main aim of producing scientific research, and examines the consequences that entanglements between scientific research and individual feelings can have on the research during fieldwork. Della Rocca is of the opinion that all these issues of positionality are relevant for training the consciousness of anthropologists and ethnographers (Spencer 2010, p. 2–3). She tries to answer these questions by looking at her engagement with her emotions when she re-entered the field in a new position as researcher, and the impact this had on her former colleagues (now informants) and the women at the shelter with whom she had worked. Reflecting on her shifting positions, Della Rocca came to the conclusion that emotions are not just subjective experiences, but rather “social facts” (Stodulka 2015, p. 199) that speak of the various realities lived by the multiply marginalized women and social workers who participated in her research as well as the realities embodied by the engaged researcher herself. The perspective of “empirical pragmatism” (Spencer and Davis 2010), a pragmatic stance toward the empirical value of emotions, allowed Della Rocca to make sense of the multiple roles she embodied in the shelter during her fieldwork and relate these roles to her research findings.

Gerda Kuiper considers her shifting role in understanding traditional healing or witchcraft in Lindi, a small village at the south coast of Tanzania. She describes her emotions and feelings raised by the surreptitious presence of witchcraft/traditional healing in the field. Initially, Kuiper was interested in researching land tenure relations in the area. However, soon after her arrival in Lindi, she encountered witchcraft (*uchawi*). She explores the notion and impact of witchcraft on her work and the trajectory of emotional learning...from the unknown to embodied and emotional experience that she went through over the 3 months during which she worked as an ethnographer in the area. At first suspicious, Kuiper intuitively adopted a positivistic approach to witchcraft, which she understood as a characteristic of greed or jealousy, a position often represented in academic writing (Foster 1972). After her host mother had a personal encounter with witchcraft, and Kuiper’s sudden putative involvement in the witchcraft event, the author engaged in a learning process that made her reflect on witchcraft/traditional healing as an epistemological challenge, gaining insight beyond her preconceived understanding of witchcraft and traditional healing. She concludes that the fear of witchcraft she experienced in Lindi was a barrier in analyzing her data at first, but eventually her fear became valuable tool for gaining an epistemological understanding of witchcraft/traditional healing in Tanzania.

The three contributions to this section try to find answers to one or more of the following six questions: (1) To what extent can information and data be modified by a researcher simultaneously being employed by an NGO or a governmental organization during fieldwork? (2) Can both the researcher and interlocutors’ emotions be viable and valuable for the research and writing process? (3) How does the inter-subjective (Coffey 1999) and objective positionality (Fonow and Cook 1991; Harding 1991) of “academic” scholars differ from that of a social researcher or

NGO employee? (4) Do individual positions affect all ethnographic data and any cultural intimacy achieved? (5) How are feelings and emotional connections between the researcher and the participants modified by shifts and continuous oscillations of positionality? (6) Which writing strategy should an anthropologist adopt to highlight the multiple role conflicts emerging from their fieldwork?

Anthropology has long proposed splitting the objective stance and the researcher's emotions. Anthropologies influenced by empirical pragmatism (James 1912) and related phenomenologies (Jackson 1989) suggest integrating emotions as important empirical sources for fieldwork, data analysis, and scrutiny of ethnographic material. The authors show that (post-)colonial, feminist and traditional perspectives on particular phenomena in the field may shift from the "total conviction" of having the right attitude, e.g. when looking through "humanitarian victimization glasses" or an (implicit) positivistic lens on cultural phenomena such as traditional healing, to a more reflexive and self-questioning perspective. This new perspective eventually leads to a deeper and more profound understanding of "the field" or role conflicts, in the sense that it is not only a "conflict" with the concomitant emotional experiences of anger, vulnerability, fright, estrangement, and embarrassment. Instead, role conflict can be an essential analytical tool for self-reflexivity and data analysis. Role conflict fosters an understanding of interlocutors that might not be possible without a systematic focus on shifting positionalities. All three chapters show this shift in a remarkable manner as they depict a change in a preconceived attitude toward *dek salam*, a perspective on feminist views of clients in women's shelters, and an attempt to understand the fear of witchcraft, all of which produce insights that go beyond mere readings of already existing ethnographies.

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Making Sense of (Humanitarian) Emotions in an Ethnography of Vulnerable Children: The Case of Bangkok Slum Children



Giuseppe Bolotta

Introduction

Self-reflexive cultural critique (Clifford and Marcus 1986; Clifford 1988; Geertz 1973) has become a fundamental principle of anthropological investigation. This principle has long been vitiated by a cognitive emphasis, as if the cultural filters through which anthropologists understand ethnographic realities were entirely made up of intellectual thoughts, concepts, and interpretations (Davies 2010). The volume at hand is an account of the increasing attention paid within anthropological scholarship to the participation of the ethnographer's entire cultural subjectivity in the co-construction of the field, i.e., his/her body and emotions, not only his/her "rational mind" (see also Davies 2010; Jackson 2010; Crapanzano 2010; Stodulka 2015). The ethnographer's theoretical and conceptual pre-comprehensions, as well as his/her lived bodily and emotional experiences, are culturally shaped and thus need to be made the object of systematic scrutiny and self-examination. Once treated from a positivistic perspective as objectively non-codifiable interference factors, the ethnographer's affects are instead best understood as elements of extraordinary heuristic potential.

I would argue that the need of the researcher to systematically fathom the uncharted depths of his/her emotional experience in the field applies to an even greater extent to those contexts where emotions are primary codes of expression, experience, and meaning. Humanitarian environments of compassion as related to human suffering are exemplary cases of such contexts.

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This chapter aims to illustrate the epistemological, methodological, and analytical importance of the researcher’s “emotional reflexivity” (Davies 2010; Stodulka 2015) in ethnography conducted among vulnerable groups exposed to humanitarian interventions. I draw upon my research on the everyday experience and identity processes of children who live in the slums of Bangkok and who are supported, as disadvantaged “slum children” (*dek salam*), by several local and international aid organizations.

When I first got involved with the *dek salam* almost 10 years ago, I was not yet an anthropologist but a newly graduated, and quite naïve, psychologist volunteering for one of these organizations. It was my first time outside Europe. Even before actually meeting the children, I was emotionally moved by the dramatic image of the Global South’s childhood suffering as it was portrayed at that time by most NGOs and media (Mesnard 2004). As a psychologist, moreover, I was prepared to approach children’s alleged disease through a set of purportedly universally valid diagnostic and therapeutic tools.

In the first part of this chapter, I will retrospectively analyze my first humanitarian encounter with the *dek salam*. I will specifically show how reflexively investigating my feelings of a priori pity towards the slum children helped localize these feelings’ historically and culturally specific origin in a western political framework—a humanitarian “ethos of compassion” (Fassin 2006)—and, ultimately, helped me avoid an ethnocentric interpretation of these children’s emotional experiences. This emotional self-awareness also marked my decision to distance myself from my role as a psychologist, to leave the NGO I was working for, and eventually to adopt an anthropological perspective in exploring these children’s lives. In the second part of the chapter, I will focus on my experience with the children as a doctoral student in anthropology. By means of ethnographic case studies, I will show the role of “humanitarian emotions” in molding specific patterns of inter-affective interaction between sympathetic social operators and pity-seeking slum children. Finally, I will stress the scientific and ethical importance of an ethnographer scrutinizing his/her affective experience in order to identify the subtle, yet important, differences among the multiple and interconnected polarities and sources (socio-cultural, political, individual, inter-personal, private, public, etc.) of both the researcher’s and local social actors’ emotional lives.¹

Compassionate Ethos: Emotions in the Humanitarian Field

In his study of the Naven, Gregory Bateson (1936/Bateson 1958, p. 11) described ethos as “the expression of a culturally standardized system of organization of the instincts and emotions of the individuals.” Referring to this notion, anthropologist

¹In this chapter, I look at emotions as a polythetic class of bio-cultural, inter-subjective events, which vary according to ethnographic contexts, and are co-produced by both the ethnographer and local social actors. For a conceptual discussion on emotions, see chapter “[Introduction: Affective Dimensions of Fieldwork and Ethnography](#)” to this volume.

Didier Fassin (2006) argued that contemporary politics are shaped by an “ethos of compassion”: an extreme attention to human suffering produced by the continuous staging, production, and circulation of discourses and images concerning grief and pain.

Suffering is continuously displayed, commented, and exhibited (Boltansky 1993): powerless victims of “terror attacks,” the corpses of migrants in the Mediterranean Sea, civilians escaping cruel wars, communities affected by natural disasters—an endless sequence of victims reach our imagination. Television, image-media technology, social networks, and the Internet have also given an unprecedented aesthetic poignancy to human grief (Mesnard 2004). Suffering is not just a private feeling but also a fundamental dimension of the public sphere (Fassin 2006). It demands an answer from us, it demands our listening and compassionate attention, and it demands humanitarian interventions.

The predominance of the compassionate ethos in contemporary global politics has clear historical foundations. Humanitarian reasoning has its most obvious roots in European experience beginning in the eighteenth century, and continuing with the formation of the Red Cross in 1863, through the efforts of administrators and missionaries to care for colonial populations, and in relation to larger trajectories of Enlightenment rationality, secularism, colonialism, and capitalism (Redfield and Bornstein 2011, p. 13). As a number of scholars have observed (see, e.g., Pandolfi 2011; Fassin 2012), humanitarianism is a secular semantics, genealogically rooted in Judeo-Christian categories,² for the moral legitimacy of western nation states’ and development and religious organizations’ post-colonial interventions in (and beyond) the Global South.

Emotions play a central role in the humanitarian paradigm. In the public sphere, the legitimacy of international interventions (not only humanitarian but, significantly, also military) increasingly depends on the public’s perception of such interventions as essential forms of universal solidarity for the defense of certain “victims.” During the last few decades, for example, several countries’ decisions to enter wars have been officially motivated by the need to provide relief, restore democracy, or protect human rights (Fassin and Pandolfi 2013). The raising of public awareness with regard to the legitimacy and urgency of the intervention is achieved through de-historicizing the “crisis” and its public moralization and “emotionalization”: essentialized media representations of innocents’ suffering galvanize an emotional reaction of compassion on the part of the “audience,” which in turn serves to morally legitimize political actions.

Fassin (2006) invites us to consider how suffering and compassion are used in public action and policy-making:

As the compassionate ethos implies interventions which are focused on the weakest and most undesirable segments of society (migrants, urban poor, etc.), there is always an intrin-

²As Calhoun (2008) notes, the term “humanitarian” was first used in the early nineteenth century to describe a theological position stressing the humanity of Christ, and subsequently efforts to alleviate suffering or advance the human race in general.

sic tension in the public interpretation of their problems that oscillates between compassion and sanction, suffering and deviance” (ibid., p. 94, translation G.B.).

In this framework, the mobilization of emotions in the public sphere corresponds to the emergence of a new form of “governmentality” (Foucault 1994), a politics of compassion that defines who has to be helped and how. A set of institutions in charge of listening to victims’ suffering and of “normalizing” their condition has been established. These range from philanthropy and mass education to medicine and social work. NGOs, in particular, are at the forefront of the West’s “forces of compassion” (Redfield and Bornstein 2011), especially in post-colonial contexts where the state is not such a consolidated political reality, and where its weakened sovereignty leaves space for international actors’ humanitarian, political, and economic endeavors.

Victimization processes are an important dimension of this politics of compassion (Fassin and Rechtman 2009). Not only does the compassionate ethos establish hierarchies of victims, but the figure of the victim itself also neutralizes individual differences as it points to a universal—and ethnocentric—representation of human suffering. At the same time, and quite paradoxically, the widespread use of psychological notions within humanitarian and social intervention identifies the causes of social suffering in the individual rather than in political and economic processes that produce social inequalities. Through clinical categories such as trauma, and practices of therapeutic listening, marginalized groups’ “social suffering” (Das et al. 2001) is “psychologized” and individualized, that is to say, depoliticized. Within the contemporary compassionate ethos, “Psychology became the instrument through which aid actors think about poverty, violence and marginality” (Fassin 2006, p. 105, translation G.B.). Indeed, as Nikolas Rose (1989) argued, “Psychotherapeutic solutions aimed at governing subjectivities are connected to the political reasons at the basis of the welfare state crisis” (as cited in Fassin, 2006, p. 108, translation G.B.).

The first time I went to Thailand, I was working for an Italian NGO as a psychologist. At that time, I was not aware that I was a representative of the western compassionate ethos. Children living in the slums of Bangkok were the “innocent victims” we were trying to save from an environment we did not really know anything about.

The Research Context: Moral Economy of Childhood in the Slums of Bangkok

Between 1984 and 1994, Bangkok was the city that registered the most rapid economic growth in the world (Unger 1998, p. 1). It was a time of tremendous transformation, which turned the Thai capital from a canal-based settlement into “a key industrial city, a city of the poor, a city of the middle classes and a tourist city” (Askew 2002, p. 49). During this time, Bangkok became, in rough numbers, the city

with the highest percentage of inhabitants residing in slums in the world. Slums³ are a side effect of an economic and territorial policy that favored urban elites, and the products of structural violence that ran along numerous lines and created inequities in the control of urban space. Most of Bangkok's slum residents are ex-farmers with no formal education, originating from every region of Thailand, especially the ethnic minority areas of the north and the northeast. For many, migration to the city was the only chance of survival in a country where the urban-rural gap remains a major problem, both economically and politically. As the capital breaks the promise of socioeconomic mobility for ethnic migrants and former peasants, the majority of slum dwellers end up as peddlers of every kind. An increasingly relevant part of the informal economy of the slums is also made up of gambling, prostitution, and drug dealing.

Public authorities describe the slums as a place of degradation, crime, drug trafficking, and threats to urban and national safety. The criminalization of the slums as spaces invisible to the surveillance of the state—potentially subversive because they exist outside of the panopticon—represents the main rhetorical strategy used across numerous regimes in the Global South to justify massive eviction campaigns officially rationalized as measures to fight criminality and restore the safety and “beauty” of cities (Davis 2006). Within this discourse, children occupy a special place: they are depicted as either victims or a social danger. If *dek salam* are not protected from negative influences, it follows, they are likely to become immoral, undisciplined, dangerous subjects, deprived of a “natural childhood.” According to such analysis, school and a “healthy” (nuclear) family life—in contrast to the slum and the peer group—emerge as necessary answers to the promotion of happy children (Bloch 2003). NGOs, together with (or against) the state, became major actors in the effort to turn *dek salam* into healthy and happy children (Bolotta 2017a, b).

Since the 1980s, various childcare international NGOs, state organizations, religious charities, and development actors began operating in the slums of Bangkok, with diverging political, economic, and social ends. Many of these organizations focus on a transnational discourse centered on children's rights and are specialized in assisting “disadvantaged” categories of childhood like “street children,” HIV-positive children, and specifically *dek salam*. In many cases, NGO interventions are based on a conception of “childhood” as a time of innocence, vulnerability, and enthusiasm (Boyden 1997, p. 190). This western, middle-class cultural representation contrasts with local conceptions of childhood, despite the emerging legal and juridical definitions produced by the Thai government in an effort to conform to the global mainstream (Bolotta 2014).

³The UN identifies a slum household “as a group of individuals living under the same roof in an urban area lacking one or more of the following: (1) Durable housing of a permanent nature that protects against extreme climate conditions. (2) Sufficient living space, which means not more than three people sharing the same room. (3) Easy access to safe water in sufficient amounts at an affordable price. (4) Access to adequate sanitation in the form of a private or public toilet shared by a reasonable amount of people. (5) Security of tenure that prevents forced evictions” (United Nations 2006).

The emergence of NGOs has transformed the slums through development scenarios centered on childhood care: heterogeneous social arenas characterized by the proliferation of interactions between different social and institutional actors, each with differing objectives and resources, to which the protection of children's rights is "an opportunity, a profession, a market, a bet, a strategy" (De Sardan 2004, p. 11). The conception of *dek salam* as innocent victims, the emotional reaction triggered by the media staging of their alleged suffering, and the moral imperative of aid responses, these all constitute the heart of humanitarian activities in the slums. The economic implications of such activities are also clear.

NGO programs are funded, thanks to donors' financial support, in the form of child-sponsorship or long-distance adoptions programs (Bornstein 2001, 2011). Moral, emotional, and economic domains are interwoven in these programs. The strategy to mobilize western donors' support is based on the commodification of the sponsored children's dramatic pictures and biographies (Fig. 1).

Mails and letters with the "victimized" children's pictures, accompanied by stories that emphasize the dramatic conditions of *dek salam*, are effective in eliciting the donor's compassion and, through this moral and emotional response, the donor's economic solidarity. Following Fassin (2013), I call this exchange a "moral economy of childhood."

In many cases, the "real" children NGOs deal with suggest a radically different "childhood" from that proposed to donors. Nevertheless, the image of a joyful child—smiling, and showing many more "adult traits" than children in Euro-American cultural contexts—has little economic value: it is rather the portrait of the innocents' suffering that can effectively capitalize on donors' empathic responses and promote, accordingly, the best humanitarian profit. Such a portrait, as argued by Luc Boltansky (1993), is generally hyper-individualized and disqualified at the



Fig. 1 UNICEF Thailand's "Child Protection." Image courtesy of UNICEF Thailand/2011/Athit (retrieved on 24 February 2017 from: <https://www.unicef.org/thailand/protection.html>)

same time: while the character's suffering is represented with details, it could be anyone. This contradiction between singularity and universality, the "interchangeability of victims" (Mesnard 2004, p. 16), reveals the ambiguities of the humanitarian logic at work in NGO operations.

When I first went to Thailand in 2008, I was volunteering for one of these NGOs: Psychologists without Borders (PSF). PSF supports the work of various local NGOs dealing with *dek salam*, sending to the field European psychologists who are called upon to enrich the NGOs' childcare assistance programs with psychological expertise. I was specifically asked to volunteer at the Saint Joseph Center, a Catholic charitable foundation which provides residential care to about ninety *dek salam* of the age of five to eighteen (Bolotta 2017a). Let me now step back in time and take a reflexive look at my (humanitarian) emotions in relation to the beginning of my volunteering experience in Bangkok as a psychologist.

The Humanitarian Encounter

During a pre-departure training event designed for new volunteers, PSF senior staff members—mostly psychologists used to dealing with Italian patients—described the children we were about to meet as neglected, abandoned, traumatized, and suffering. I'd yet to meet them but I already felt deeply touched by these descriptions. Having never been outside Europe, I was also influenced by the visual representation of children's misery in the Global South, continuously displayed on Italian television screens, to which I had unconsciously attributed a character of trans-cultural universality. It was the *dek salam* that gradually undermined my ideological and emotional convictions and that moved me to begin a critical self-reflection, which resulted in my decision to conduct doctoral research in anthropology. Our first encounter was already quite revealing.

In August 2008, I was at the entrance of the Saint Joseph Center when, along the pathway leading to the NGO's huge playground, a hundred children were haphazardly chasing one another. They seemed imbued with an irrepressible enthusiasm. Once they realized I was there, they hurried toward me to hugging me, tugging on my ears, pulling me in all directions, and shouting in a language that was still obscure to me. They were playfully fighting over my attention. I felt like I was the target of a hunt. I was very confused and disoriented by the vitality of children who had been described to me by the Italian psychologists of PSF as suffering and traumatized "victims."

While remaining the center of the children's interest, I noticed that Chiu, 5 years old, had moved away from the group. Sitting on the sidewalk with his hands covering his face, he seemed to cry. I immediately moved towards him. I instinctively perceived that expression of sadness as the only recognizable element of the whole situation, something I had been emotionally and professionally trained to handle. When I got closer, I realized that Chiu, hiding a wry smile behind his hands, was actually just pretending to cry. Even before I caught him in the act, he had tightly

clung to my back, writhing and screaming aloud so that his friends would see him on the shoulders of the *farang* (white person). The Saint Joseph's guests were accustomed to visiting European volunteers and seemed able to manipulate them. They really did not seem the "innocent victims" I was eager to assist.

The chaos unleashed by my presence was suddenly interrupted by the arrival of Thai educators. The *dek salam*'s behavioral and affective attitudes abruptly changed. Quiet, and in perfect order, they quickly settled in parallel rows, with older kids as head-rows. One by one, they reached their usual positions on the courtyard and reverentially greeted educators with the *wai*. The Thai greeting referred to as *wai* consists of a slight bow, with the palms pressed together in a prayer-like fashion.⁴ The *wai* was part of a culturally shaped repertoire children used to show in presence of Thai adults, as *phu-noi* (small people) must do in relation to *phu-yai* (big people).⁵

I slowly started to observe significant discrepancies between the image of *dek salam* I had in mind and the ethnographic reality. First, the category of *dek salam*, like other categories of disadvantaged children, merged extremely different cases into a single reified conceptual container.⁶ Some of the children, for example, did not come from the slums of the capital. Not all of them were orphans or had severed relationships with their family. On the contrary, some of the kids had an excellent relationship with both parents or, when parents were absent, they still had strong bonds with grandmothers, aunts, or other relatives within matrilineal kinship structures quite dissimilar to the bourgeois standard of the western mononuclear family (Bolotta 2017a). Chiu's parents were not languishing in unspeakable poverty but chose to entrust their son to the NGO in order to grant him a good scholastic education. Therefore, the category of *dek salam*, to which all children are invariably connected once placed in charitable institutions, obscured the children's biographical, ethno-linguistic, and class differences, as well as the wider political and economic processes in which their marginalization is historically grounded.

Second, the children's affective, linguistic, and behavioral attitudes towards us, *farang* volunteers, were very different from those expressed when they were relating to Thai adults. Finally, Thai operators, rather than considering the children

⁴The *wai* has its origin in the Indic *Anjali Mudra* and is present, in similar versions, in several Asian countries (Anuman 1963).

⁵Within the Thai hierarchical social system, social interactions are terminologically mediated by the use of linguistic markers of status (big/small people, *phu-yai/phu-noi*; elder brothers/younger brothers, *phi/nong*) that refer back to a vocabulary of power. *Phu-noi* are not only children but also, more generally, anyone relating to big people (*phu-yai*). Children in relation to parents, laity to Buddhist monks, as well as citizens to the state's representatives, are *phu-noi* who must demonstrate obedience, respect, and gratitude to *phu-yai* (Bolotta 2014, 2016).

⁶Several scholars have observed the mystification of reality produced by depreciative categorical definitions of disadvantaged children. Glauser (1997) and Panter-Brick (2002), for example, have deconstructed the category of "street children," explaining how this label tends to flatten a huge variety of cases into a one-size-fits-all political concept, which tends not only to distort the children's family and social situations, but also to cover up the economic and political roots of their marginality.

“innocent victims,” in many cases emphasized their “normality.” Accordingly, they seemed to show much more emotionally detached behavior towards the children.

During my first stay in Thailand, I came to care deeply for the children. Still affected by my role as a psychologist and by the representation of *dek salam* as “victims to be saved” (Mesnard 2004), I wanted to help them and I was determined to understand the meaning and the multiplicity of their life experience. Back in Italy, I abandoned the project of starting a specialization in psychotherapy. I harbored a certain dissatisfaction with the profession for which I had been trained with so much passion. The psychological categories through which I initially tried to codify the *dek salam*’s world, such as trauma or attachment, had proved inadequate. I had begun to sense their socio-cultural specificity and political-moral value as connected to the humanitarian moral economy of childhood and the Western compassionate ethos. In the course of my fieldwork as a doctoral student in anthropology, and through reflecting my own as well as the research protagonists’ involvement in this humanitarian moral economy, I would gain a more comprehensive understanding of the effects of this compassionate ethos. Within this ethos, as I discussed previously, emotions act as public collective phenomena, which are entangled in specific historical discourses and political-economic practices. Emotions, however, are also embodied by individual subjectivities as private, relational, and situational events.

Many of the volunteers I met in Bangkok’s slums seemed to experience their own emotional reactions towards the “victims” as “authentic,” subjective, and spontaneous facts. Moreover, feelings of compassion for the poor children often constituted NGO workers’ motivation to enlist in the “goodness army” of humanitarian organizations. These consciously expressed and emotionally connoted intentions could also be associated with unconscious motivational dynamics that are self-rewarding rather than resulting from an empathic recognition of the children’s supposed suffering: in some cases, for example, solidarity towards victimized individuals promotes in the helper a psychologically rehabilitative, and coveted, self-perception of moral value (see, e.g., Vaillant 1977). At the same time, the helpers’ need to nail the image of help receivers—slum children—to the archetype of the victim implies a cultural and emotional distortion that is realized through a devaluation of the one to be helped: victims must be vulnerable and dependent individuals incapacitated to help themselves and, as a result, they are placed in an inferior position vis-à-vis the helper’s.⁷

Such complex emotional experiences, as I described in presenting my first encounter with the children, often precede the first meeting with the “real”

⁷This happens even more in the presence of substantial economic and power differences between the helpers and the helped. Sociologist Richard Sennett’s book *Respect: The Formation of Character in an Age of Inequality* is an important contribution on these hierarchies in the context of US American welfare policy. In describing his upbringing in the Cabrini-Green housing project in 1940s Chicago, Sennett (2004, p. 13) has pointed out: “The project denied people control over their own lives. They were rendered spectators to their own needs, mere consumers of care provided to them. It was here that they experienced that peculiar lack of respect which consists of not being seen, not being accounted as full human beings”.

beneficiaries of the humanitarian aid. Indeed, they are rather elaborated in connection with the media-conveyed and psychological imagery of the “victim” that social workers came into contact with before actually taking their mission. This “victimhood imagery” is subsequently projected on local social actors who are interpreted in such a way as to confirm both their status of victims and the moral necessity of the humanitarian intervention. It is essential to emphasize that there are differences in aid workers’ attitudes towards aid recipients as well as in the media reception of children’s lives in countries of the Global South. The victimhood imagery is not the only expressive model that media and NGOs draw on. There is an increasing awareness among European NGO workers of the political impact of the victimization of marginalized parts of society. Some view aid action as advocating social justice and solidarity, rather than as benevolent governance and charity. However, as Fassin (2006, p. 109) emphasizes, “these voices go normally unheard,” especially in dominant public discourse and media representation. The sinister charm of the victim and the language of suffering remain politically, aesthetically, and emotionally prevalent. The dominant role of psychology within the humanitarian sector, moreover, reinforces aid workers’ predisposition towards identifying individual vulnerability rather than socio-political potentiality.

When, in 2011, I returned to the Saint Joseph Center and the slums of Bangkok as a researcher in anthropology, I had the opportunity to live with the children for longer periods. One of the first issues I decided to think about “anthropologically” was that of my own emotions—in particular the compassionate ethos marking my relationship with the children—which I perceived as incongruous with the social reality I came in touch with and inopportunistically boosted by NGOs’ humanitarian rhetoric. Over time, it became increasingly evident that not only my emotional experience, but even the children’s, was influenced by the humanitarian compassionate ethos both I and they were reproducing on a micro-social scale through our inter-affective exchanges. In the next sections, I will present a few ethnographical examples to show this and, more generally, to discuss how “the subjective and emotional quality of the relationship established between researcher and participants, once examined, brings a deeper level of understanding and a greater degree of objectivity to findings obtained during ethnography” (Kisfalvi 2006).

Little Bon’s Strange Grief

In 2011, Bon was an 8-year-old boy, sweet, and always smiling. He was addressed by all as Bon *lek* (little Bon) because he was frail and quite short compared to his peers. When I first met him, he was walking alone on the dirt road leading to the Saint Joseph Center. Back from school, he was carrying a backpack that looked much bigger than him. Father Adriano, the Italian missionary in charge of the Catholic NGO, told me that Bon had been accepted by his organization only few months ago after his mother tragically died from AIDS-related illness. According to

the Italian priest, Bon had always lived with his mother in Tuek Deang, one of Bangkok's biggest slums.

I quickly grew fond of the kid. The compassion I felt for him, far from expressing an exclusively personal set of emotions, represented a shared experience among volunteers at Saint Joseph's. It was an emotion inscribed in the political register of the humanitarian compassionate ethos. Initially, I tended to interpret Bon's life story according to moral and psychological schemes that made him the quintessence of victimhood, the innocent, traumatized child whose parents have died, and whose seemingly serene and joyful appearance had to conceal a secret suffering. Bon was quick to notice the cultural prejudices whose trap I had apparently fallen into, although at this point I was more aware of the discourses that impacted the encounter between *farang* fieldworkers and *dek salam*.

During dinner, he was careful to sit next to me, pour my drink, and to wait until I swallowed the last bite of meal, getting ready to pounce on my empty plate in order to free me from having to wash it myself. After dinner, when all the children usually gathered around the common TV, Bon went to clean up my room and did my laundry. Before I went to bed, he insisted that I let him massage my back, an art that all Thais are generally trained in from childhood. His subservient behaviors made me feel uncomfortable and embarrassed. I was supposed to take care of him rather than the other way around. At that time, I did not yet clearly understand that Bon was behaving as a proper *phu-noi* (small person). In Thailand, indeed, to be a good child (*dek de*) means to take on the role of *phu-noi*, which is to act in service of adults conceived of as *phu-yai* (big people) (Bolotta 2014, 2016).

During the first weeks of my stay at the Saint Joseph Center, especially in presence of Thai educators, the *dek salam* related to me deploying culturally informed behavioral and affective repertoires that reflected the local role-dynamic between *phu-noi* and *phu-yai*. My answer to the children's efforts, on the other hand, was quite dissimilar to that normally provided by Thai *phu-yai*.

Convinced that Bon was particularly in need of affection due to his mother's early death, and moved by Father Adriano's dramatic description of the child, I told Bon that I loved him even if he did not wash my clothes every day, and that the role of caregiver should have been mine. Bon replied to my statement with a chuckle. Only later I realized that with his chuckle the child had placed me in the same category as most of Westerners⁸ dealings with *dek salam* in Bangkok, that is *farang*. Bon had witnessed the emotional discrepancy between the strange adult I was in his eyes and local *phu-yai*, and he began to play with it.

⁸The terms Westerners or "Western social workers," just like *dek salam*, are problematic because they could rigidly suggest the existence of something like a homogenous and essentialized category of people. In western contexts, instead, ideas such as childhood, giving, and suffering might vary according to a multiplicity of factors including class, gender, and individual trajectories. Nevertheless, the majority of the international NGO social operators I came to know in Bangkok are Caucasian, Euro-American, middle-class professionals for whom the compassionate ethos constituted a unifying (although individually differently modulated) moral and emotional framework.

One evening, while we were sitting on the sidelines in the courtyard of the Saint Joseph Center, Bon told me about his mother: “*Phi* (elder brother), do you know that my mother died?” According to Father Adriano, Bon had never before talked with any adult about his mother’s death. When I asked why he was reticent to address the topic with the NGO’s educators, and how he was working out his loss, he replied: “I talk about these things with friends. Mom left me with Father Adriano. Then she promised she would come back to get me. But instead, she died.” Why had he chosen to share this with me? Did he now see me as one of his friends? I sensed some anger in between his words, but I was still troubled by Bon’s apparent emotional detachment.

The death of a child’s family member was quite a common event at the Saint Joseph Center. Right after his mother’s death, Bon had shaved his head before wearing the orange robe of novices (*samanaen*). According to Thai Buddhism, sons temporarily ordain as *samanaen* at the Buddhist temple where the cremation ritual takes place in order to pay off their karmic debt to parents (Tambiah 1976). At the temple, *samanaen* have the opportunity to understand the natural character of death as well as the concepts of finitude, transience, and the illusory nature of life. They are also trained to deal with “the unavoidable” through a religious posture of acceptance, which must be as serene and emotionally detached as possible. Feelings like despair, signal attachment, and weakness must be neutralized through Buddhist meditation.

The “cultural constructions of childhood” (James and Prout 1997) that shape western operators’ care-giving models underlie different interpretations of both the concepts of “childhood” and “children’s suffering.” The emotional pathos produced by the portrayal of children as victims, and the reference to a western standard (thought of as universal) of children as vulnerable yet-to-be individuals, lead the NGO’s *farang* to a disproportionate emotional response compared with what children like Bon are normally exposed to while interacting with Thai adults. The inter-affective dynamic between *farang* and *dek salam* might indeed qualify as opposite and antithetical to that proposed by Thai Buddhism. In describing his mother’s death, Bon seemed anchored to a culturally normative, Buddhist-informed elaboration of his emotional experience. At the same time, he seemed to understand the “emotional productivity” of the “drama” if played out with a *farang*.

After he told me about his mother’s tragic death, and after having gained my worried solicitude during what I thought would be a challenging emotional transition for him, Bon started to raise demands. Every evening, after curfew, Bon sneaked out of the children’s dormitory and stealthily reached my room to ask for the key to the pantry, that I was responsible for. Showing up with a smile—I could not tell if this was mocking or tender—he used to timidly ask: “I would like to drink a glass of Ovaltine with you before going to bed. May I?” I was not even able to say no. In the deserted and unusually quiet courtyard of the Saint Joseph Center, the secret night rendezvous with Bon became a sort of ritual. During those nights, while we shared the transgression of being out after curfew and sipped Ovaltine, Bon used to tell me in even greater detail how difficult his life had been. Without realizing it, I soon began to favor Bon among the children. He began to ask me for additional tips, or to accompany him to the market, certain as he was that I would pay for him. An

oft-requested demand was to use my cell phone to call his aunt. When I finally had the chance to talk directly with the woman, I discovered that Bon—contrary to what I had been told by Father Adriano—always lived with her and that he never really knew his mother. I felt like I was the little kid, not Bon.

Like many other *dek salam* whose parents come from the North or North-East rural areas of Thailand, Bon was not placed in a mononuclear family structure, but was raised by his maternal uncle's extended family after his mother entrusted him to his grandmother because of her illness. In the case of Bon, child–mother attachment relationship, theorized by western psychology as the cornerstone of child development (Bowlby 1988), was not at all the child's main emotional framework (Bolotta 2017a). Over time, I understood that, because of our “special” relationship, Bon had sidestepped a number of rules to be observed by *dek salam* at the Saint Joseph Center. I also realized that the other children came to know about it and that, somehow, Bon's reputation and social position within the peer group had benefited from this.

Children's Discomfort? The Emotional Manipulation of Humanitarian Operators

My readiness in reacting to the children's expression of apparent emotional distress led me to face a complicated and emotionally challenging situation. Not only Bon, but also the other Saint Joseph's guests started to ask for my mediation so that they could be allowed to circumvent some of the rules prescribed by the Catholic NGO. They sought my intercession for them to get an extra hour to play football on Sundays, watch more movies, return to the slums more often to visit their relatives (if any), and the like. Rather than directly advancing their requests to Thai educators—the *phu-yai* (big people)—children had identified me as a *farang* sensitive to their needs, emotionally maneuverable, with the ability to influence the Thai staff. I eventually became a point of contention: children seemed to compete for an exclusive relationship with me. The latter was used to achieve various kinds of social, economic, and affective objectives.

One afternoon, while playing football in the yard, Bon spectacularly fell to the ground, scraping his knee. I waited in vain for any educator to intervene. Except in the case of real emergencies, indeed, male children over 8 years of age were encouraged to be autonomous, including self-medication. The infirmary was open and freely accessible. Instinctively, however, I rushed to his aid. I picked him up and got him to the infirmary, where I took gauze, ice, and disinfectant to dress the wound. The other children—who, in the meantime, had also come—were looking at us, incredulous that a *phu-yai* could take care of them that way.

Over the next few days, on their way back from school, the Saint Joseph's *dek salam* began to show me the wounds, sometimes very minor, they got due to their reckless, daring, and fun adventures: “Look *phi* (elder brother), I got hurt!” They

knew that I would immediately go to the infirmary, medicate them, and that I would devote all my time to them. They quickly turned me into a kind of nurse. They intercepted my apprehension and began not only to ridicule my clumsy actions but also to manipulate my emotional responsiveness as a “symbolic capital” (Bourdieu 1986). This progressively applied to other behavioral categories that the children understood would elicit my emotional response. For example, while I was present and Thai educators were not, some children began to simulate fights. Thai educators normally left the kids to resolve their conflicts by themselves. Not me, still affected by an unwitting interventionist instance. Ton, a 16-year-old boy, began to stage the following script: he called me while laughing at me, and then he pounced with kicks and punches on a younger child. Both knew that I would get upset and stop them. As soon as this happened, the boy and his alleged victims, cracked up in seeing how the maneuver had succeeded in dragging me into their field of action.

While children relate to Thai *phu-yai*, the expression of negative emotions and content is normally considered unbecoming. In today’s militarized Thailand, demonstrations of emotional vulnerability are also discouraged by a dominant construction of male identity that promotes in boys the assumption of warrior-like and soldierly gendered traits (Bolotta 2016). In spite of this, still convinced that it was essential to enable traumatized children the opportunity to express negative emotions in the context of a positive relationship with an adult, I was treating every sign of discomfort with the utmost seriousness. Especially at the beginning of my experience at the Saint Joseph Center, I used to call aside the seemingly sad children, trying to get them to talk. In response to this, the children were often simulating crying, reminding me of their status as poor *dek salam* in need of help.

Also in terms of body language, while in the relationship between children and Thai educators, respect and mutual affection were expressed through relational emotional codes that did not involve physical contact (at least at the Saint Joseph Center, where the boys’ starting age is 8 years), western volunteers used to fondly hug and touch *dek salam*, even though they were 17-year-old boys. Bon and the others seemed to understand how effective it was to accompany various requests with emotional attitudes of this type in their interactions with me and the other *farang*. Aware of my influence on the directors of the NGO, the children turned me into their mediator, a spokesperson of their concerns and desires.

Although in the slums—as I had the opportunity to learn later, when I carried out my fieldwork in the shantytown from which the majority of Saint Joseph’s guests were coming—the children appeared to be everything but “children” or “victims” (in the standard sense of the terms), in relationships with the representatives of humanitarian reasoning, the *dek salam* seemed to articulate their agency through the strategic adherence to the emotional characterization of themselves as “vulnerable children” or “victims.”⁹ The adoption of these images was realized through the

⁹The political value of western discourses on ‘victimized’ children have been recently documented by several anthropologists in different contexts of the Global South: see, for example, the works by Vignato (2012) in Indonesia, Cheney (2013) in Uganda, and myself (2017a, b) in Thailand. These studies show how such discourses, and the correlated image of children as “victims,” could be strategically used and appropriated by the subjects of humanitarian policies.

deployment of a “language of suffering” which was made up of emotionally connoted body and narrative performances and aimed at the emotional manipulation of *farang* humanitarian operators.

Beyond Moral and Emotional Economies of Victimized Children

Emotional narratives do not simply reveal an alleged sentiment but have a pragmatic effect: they produce a reaction (Beatty 2005, p. 25, as cited in Stodulka 2015, p. 87). This is exactly what happened during my ethnographic encounters with the *dek salam* of Bangkok.

I would like to highlight a few important points in this regard. In order to produce specific emotional experiences in humanitarian operators, the Saint Joseph’s children must have preliminarily identified a number of things: first, the NGOs social workers’ emotional categories (which refer to different cultural norms than those prevailing locally); second, the inter-subjective, situational, and pragmatic character of these emotions (they must, for example, have realized that if Italians meet crying kids, they will be normally induced to offer some kind of solidarity); and third, the relationship between the humanitarian conceptions of *dek salam*, victim, and the *farang*’s care-giving models. This is an emotional and cultural learning process children go through due to the continuous interactions with the many social workers who frequent the slums as actors on the humanitarian cosmopolitan stage. The complexity of these children’s emotional understanding evidently destabilizes NGOs workers’ normative representation of “the child” as a vulnerable, passive, and socially low-skilled subject.

Children must also have embodied these culturally shaped inter-affective schemas in order to be able to “performatively” (Butler 1990) reproduce a certain emotional experience, say sadness, in a culturally convincing way. In other words, if they want to provoke the empathic response a desperate kid is able to stimulate in *farang*, they must be able to mimic the bodily and verbal expression of childhood desperation *farang* are averagely used to in their own cultural contexts. Very often, the children’s performance is intentional and designed to produce the appropriate and complementary emotional response: NGOs social workers’ compassion and willingness to intercede on specific requests.

Thomas Stodulka (2015) analyzed researcher–children inter-affective interactions similar to those just described. In relationships with the actors of Java’s humanitarian landscape (activists, researchers, NGOs workers, etc.), “street children” used to tell morally and emotionally overloaded dramatic stories like those related, for example, to a parent’s death or serious illness. Making strategic use of these accounts, the youth of his study hopelessly begged for money only to then prove to be anything but desperate in vernacular social contexts. Stodulka described his research’s protagonists’ ability to turn their marginality into emotional and

economic ties with humanitarian operators as an “emotional economy” (Stodulka 2014, 2015). Indeed, by linking their emotional expressions to the ideological context of the meeting, the young men were able to expand their social networks and convert them into “social and economic capital” (Bourdieu 1986). This is similar to what I observed in the slums of Bangkok where, in many cases, *dek salam* could access NGOs’ resources, also thanks to their ability to produce emotionally well-orchestrated performances. As Stodulka explained, the young men’s strategies to present themselves according to the image of victim, and to focus on the expression of emotions related to suffering, did not merely represent the outcome of individual subjective choices. On the contrary, the fact that many *dek salam* selectively exhibited certain emotional patterns in their interactions with *farang* shows that such strategies constituted a collectively learned survival strategy and embodied socio-cultural fact. At the same time, if for children, *farang* working in aid institutions such as the Saint Joseph Center represent an affective and relational reservoir that can be turned into social and economic capital, *dek salam* constitute a humanitarian capital for *farang*, too. In this regard, I believe it is important to complement Stodulka’s fine analysis of marginalized children’s and NGOs workers’ interrelated emotional economies, making clear that these are both connected and informed by a political transnational superstructure, namely the humanitarian moral economy of childhood.

Humanitarian discourses and practices provide a propitious framework with which to examine how the entire transnational “industrial chains of charity” list categories of “disadvantaged children” to direct global resource flows. NGOs, in particular, continue to reify and commodify images of “children in need” in order to stage “a spectacle of suffering” (Boltansky 1993) that is essential for raising funds and donations. The depiction of children as victims to be saved is thus at the center of a “moral economy” (Fassin 2013) which is one of the main contemporary vectors for post-colonial compassionate governmentality to penetrate the cracks of the world’s internal and external peripheries. By sticking to the humanitarian categories they are exposed to, victimized children, for their part, unconsciously perpetuate the need for humanitarian interventions, thus revealing the complementary nature of the relationship between local emotional economies and transnational moral economies.

In many cases, while aid workers experience their compassionate emotions as “internal” and “natural” subjective facts, there is no awareness of the historical, cultural, political, and economic characterization of the humanitarian scaffolding on which these emotions are structurally embedded. An ethnographic approach, which seriously considers the researcher’s affects as epistemic data, is the prerequisite for the emergence of such an awareness and for avoiding ethnocentric distortions of subjects categorized as victims in humanitarian contexts.

Conclusions

In this chapter, I discussed the role of emotions as a valuable source of insight during ethnography. I argued that an ethnographical approach grounded on the anthropologist's "emotional reflexivity" (Davies 2010; Stodulka 2015) is especially necessary in contexts of humanitarian action, where the languages of suffering and compassion constitute a dominant ethos, emotions are public vectors of post-colonial geo-politics, and where victimization processes produce psychologized and depoliticized portraits of local social actors.

In my research with children living in the slums of Bangkok, I demonstrated the role of "humanitarian emotions" in shaping affective interactions between compassionate social workers and victimized slum children. I showed that this emotional exchange responds to a broader set of cultural values, political-economic practices, and scientific discourses, including psychology. Using emotions as a lens to reflect upon my positionality, I gradually realized that my behavior towards the children, like that of the other volunteers, was similarly molded by the humanitarian compassionate ethos. This emotional self-understanding lays at the origin of my decision to remove the vestments of an NGO psychologist and to wear those of an anthropologist no longer interested in saving slum children but strongly motivated to share their socio-cultural experiences.

One final point should be made about my affective relationship with the children. After 8 years of knowing each other, our emotional interactions are not interpretable anymore only as the top-down effects of humanitarianism. Over the years, the children and I co-created idiosyncratic relational patterns of interaction. Indeed, emotions are central in determining an intrinsic (but often neglected) dimension of the ethnographical experience: the construction of intimate, close, sometimes even life-long relationships with local social actors, too often erroneously intellectually packed into emotionally neutral classical constructs (informants, natives, etc.). Today, the children do not relate to me in the same manner as the first time we met. They now know their emotional strategies will not work on me. Of course, I have not achieved the anthropological utopia of becoming "one of them" but, certainly, I am no longer a *farang* psychologist looking at *dek salam* as though they were traumatized Italian kids. Conversely, the children do not see me as a Thai adult, nor do they think of me anymore as a *farang* of the humanitarian arena. Our relationship today is a singularity, a breakthrough, a discontinuity, something that was previously missing and is hardly definable. Our affective bond might be seen as an "event," in the Foucauldian meaning of the term, namely "the collapse of a specific power (...) the refunctionalization of a language and its use against previous speakers" (Caruso 1969). The power of the compassionate ethos and the language of suffering have lost their strength, and are not anymore the main codes of organization of our affective interactions.

The collapse of a specific power, at the same time, inevitably favors the emergence of new power configurations. The ethnographer and local social actors continue to struggle to impose social roles and cultural categories on each other. As

Jackson (1998, p. 8) reminds us, “intersubjectivity is stepped in paradox and ambiguity (...) a site of constructive, destructive and reconstructive interaction.” “Compassion and conflict are complementary poles of intersubjectivity, the first affirming identity, the second confirming difference” (ibid., p. 4). I am not anymore the “average *farang*,” but—in the eyes of local social actors—I am still a white European man, representative of a richer world, holder of vastly greater cultural, economic, and political resources and capital compared to poor children living in a Thai shantytown. Yet, the prolonged sharing of experience, thoughts, and emotions with children created slowly, but progressively, a strange and intense relationship which transcended the initial reciprocal enactment of “humanitarian emotional behaviors,” and turned into a new emotional pattern, a “third inter-subjective space,” from which both myself and the children have the opportunity to observe comparatively, and reformulate critically, the cultural specificity of ourselves.

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Emotional Vulnerability and Ethnographic Understanding: A Collaborative Research Project in a Women's Shelter



Marina Della Rocca

Fieldwork remains at the heart of anthropological research. (Spencer 2010, p. 1)

Introduction

In 2014, I commenced an ethnographic research project as part of the PhD program in Social Pedagogy at the Free University of Bozen-Bolzano, Italy. The research concerns the analysis of advocacy practices employed by a shelter for women who have suffered domestic violence, a shelter where I worked from 2010 to 2014, and where I am still an active member and volunteer. The shelter, operated by a women's association, is located in northern Italy, and offers counseling, legal advocacy and a secure safe house for victims and their children. The shelter collaborates with the local social services, the police and the court to ensure the protection of women clients and their children.

During an early phase of the research process, I was overwhelmed by a range of emotions, and soon came to recognize “how certain emotions evoked during fieldwork can be used to inform how we understand the situations, people, communities, and interactions comprising the life-worlds we enter” (Davies 2010, p. 9). In questioning these personal emotional experiences, I will focus on the shift in my role as a women's shelter operator to that of an anthropologist researcher, and thereby reveal two aspects of the research process. The first concerns my relationship with my former colleagues, an aspect dealt with primarily in the first part of the research process. The second concerns my relationship with the eight migrant women who I interviewed, all of whom had previously sought support from the women's shelter. With this objective in mind, I will try to answer the five questions formulated by anthropologists Dimitrina Spencer, who aims to highlight the relevance of emotions

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as a way of knowing: “How do our emotional experiences, attachments and detachments affect the anthropologist as both person and researcher?—How do we cope with them, integrate them, and employ them as methods for deeper understanding?—How do emotions influence our participant observation, and our wider interpretative and explanatory enterprise?—How do we reconcile the emotional and the subjective with our scientific goals, and what are the consequences of such integrations of anthropology?—Are all of the above issues relevant to the training of anthropologists?” (Spencer 2010, p. 2–3).

Research Topic and Methodology

During my work experience as a women’s shelter operator, I had observed a set of structural obstacles hindering the empowerment of women with a migration background. Many studies reveal the specific vulnerability of migrant women suffering domestic violence, referring specifically to the intersection between gender, race, and class (Crenshaw 1991; Farmer 2009). Referring to the analysis of the Italian situation, where migrant women frequently face a set of linguistic, legal, institutional, and economic barriers (Creazzo et al. 2011), I was able to identify a range of specific structural difficulties. Firstly, the impact of Italian migration laws on family reunification and residence permits, whereby women who have obtained a brief-term permit for family reunification risk being classified as illegal immigrants while seeking legal separation from their husbands, a situation often leading them to return to their violent partners due to a fear of deportation. Other serious and frequent problems relate to the economic marginality of migrant women, due in part to their isolation (caused by their domestic violence situations), the linguistic barriers they experience when separated from their families and communities, and the inevitable and often unrelenting racism they confront when searching for a job or an apartment. My research critically investigated the shelter’s advocacy work in order to identify the extent to which the shelter operators’ practices reproduced these examples of structural violence. The research developed within three specific ways. The first concerned the involvement of five shelter operators who participated in a series of meetings where they reflected on support practices. The second focused on my immersion in the field as a volunteer in the safe house, where I undertook my participant observations. The final phase of research concerned the collection of 14 interviews undertaken with eight migrant women who lived in the safe house, and which gathered their points of view about the support they received. The ethnographic analysis of my research was based on the triangulation of these different perspectives. In order to ensure that the research process was collaborative, the shelter operators were involved at each stage of the project. The inclusion of the operators provided an opportunity to obtain a shared perspective on the nature of the power dynamics inherent in the shelter’s advocacy practices, and then to review both the power dynamics and the shelter’s political agenda in the context of structural violence affecting migrant women. This transformative goal responds to the

feminist activist approach of the research project in its aim to analyze power dynamics within social structures and to foster social justice (Kirby et al. 2006). The inclusion of the shelter operators also addressed my commitment to them by ensuring they were provided with opportunities to review the findings, give feedback (Snyder 2002, p. 78), and discuss the potential risks related to the dissemination of my critical analysis (Madison 2012, p. 132).¹

Shifting Positionalities: Emotions that Affect the Anthropologist

As mentioned above, the coexistence of my different roles as a former operator and as a current researcher contributed to a plethora of emotional experiences, beginning at the time I returned as a volunteer to the shelter (and its annexed safe house) to commence my participant observations.

From the time I asked my former colleagues if it was possible for me to return as a volunteer in the night-service, it was apparent that my positionality in that space had changed, a space that must be understood in both physical and relational terms. As a shelter operator, I had previously moved within the safe house with an air of familiarity, and had shared common practices and struggles with the operators who became my research partners. Having failed to reflect in advance on the consequences of this role shift in my relationships with former colleagues, I took for granted the fact that they would agree to my returning as a volunteer. In my field diary, I noted my disappointment that the opposite had in fact happened, and that they had been required to evaluate my request according to formal shelter procedures.

She [an operator] told me that she had to talk with those responsible for planning the volunteer night-service. Firstly, she would report my request at the operators' meeting, and then, I would be informed of their response. After our phone conversation, I felt a certain degree of distance between us, and a sense of being excluded. (Field notes, 20 January 2015)

At that moment I realized, with astonishment, that I was being addressed not as a shelter colleague, but as an ordinary member of the public. I was extremely upset, and shocked. The following day, one of the operators called me to respond to my request. I recognized her awkwardness, and asked myself whether my former colleagues had perceived me as a threat. I was immediately aware that my role had changed. Part of my identity had been compromised, and with that came feelings of doubt and uncertainty. I felt "I was moving within myself (...) from a known

¹Other ethical aspects concerned the ways in which the research could affect the women interviewees. Through a process of informed consent, I guaranteed anonymity and confidentiality, and thanks to my professional expertise as a shelter operator, I paid particular attention to the emotional risks to women who had lived through a traumatic experience such as domestic violence (Ellsberg and Heise, 2005, p. 38–40).

cultural space (home) into this unfamiliar terrain” (Davies 2010, p. 74), and I no longer knew how to interact within that space, a space that had previously shaped my professional identity and a space that was now redefining it.

The operators finally accepted my request, but a number of issues were of outstanding concern to them. How would I deal with my role as researcher while obtaining data during my volunteer service, and in which ways would my research role potentially compromise my role as a volunteer? How would I manage delicate information concerning the safety of the women and the operators? Should formal procedures be followed as to how specific information would be managed, given the potential conflict between my roles as researcher and as a shelter member? In my field diary, I reported my surprise at the operators’ concerns. I thought it was obvious that, as a former operator, I would be capable of managing those issues properly while being “inside” as a researcher. Nevertheless, what did my personal understanding of “properly” actually mean to my former colleagues?

Emotions as a Method for Deeper Ethnographic Understanding

Recognizing that I had been disorientated by their response, one of the operators pointed out that their questioning was not a personal matter. When I subsequently reflected on her statement, I realized that my feelings of exclusion were naive. By re-entering the research field as an anthropologist, and not as a shelter colleague, the dynamics of our interactions would necessarily change. The shelter operators were obliged to consider that in accepting me into the safe house as a researcher, they were responsible for the women they were supporting, as well as guaranteeing trust and transparency about their supportive roles. At the same time, it was essential to me that I continued to highlight critical concerns about the shelter’s practices without betraying the trust of my former colleagues. These reflections led me to understand why it was essential that my position as researcher be clarified in ways that emphasized my ethical commitment to the shelter, the operators and the women seeking their support. I then proposed that we should jointly define both the terms of my access to the shelter, and possible ways to manage the ethical issues that might emerge due to the coexistence of my different roles as former operator, researcher, and volunteer. We then committed to being transparent with each other about any other unforeseen issues that might be associated with my ethnographic investigation. This specific field experience made clear to me the extent to which this immersion concerned my pre-existent professional relationships, and how “the field comprises past and future affective relations which become alive and coalesce in the present” (Spencer 2010, p. 28). Negotiating our roles and tasks within the research field, and engaging ourselves in building a trusting relationship between each other, all of us (the operators and I) had to deal with the difficult task of rendering *unfamiliar* that which in our former professional relationship had been familiar

(Crapanzano 2010, p. 65). This awareness made it essential for us to engage in a collaborative research process, and to recognize the genuine commitment required in this process.

Taking seriously the human relationships that give rise to collaborative processes means that we also take seriously the ethical and moral commitments we make to ourselves and others as our ethnographic projects unfold. (Campbell and Lassiter 2015, p. 5–6)

Positionality Attunements, and Analytical Awareness

Taking seriously the uncertainty and disorientation that had resulted from my interactions and engagement in the field, I was able to comprehend the methodological and ethical dimensions required to deal with the multiple positionalities at stake in that research. The operators' response to my request for access to the shelter demonstrated the extent of the professional and political commitment of its operators to ensure safety and anonymity for the women they support, and thereby upholding one of the most important political principles underpinning their advocacy work. When a woman turns to the women's shelter, the operators immediately attempt to build a trusting relationship with her to ensure that the woman feels that she is in a safe space, can be understood without feeling threatened, and where she can finally give voice to the oppression she experienced at the hand of her abuser. This represents the basis of the advocacy work of the shelter and is where I had grounded the expertise to enter my specific ethnographic field. Nevertheless, the unexpected nature of the *relational process* (mediated by emotions) of re-entering the field (Spencer 2010, p. 2) had forced me to expose the implicit assumption of the women shelter's support activities, and accentuate the *taken for granted* in my embedded practices. Referring to the first question raised by Spencer, my emotional experiences in the field affected me as both person and anthropologist, because those experiences were the result of the indissoluble tie between me as person and me as researcher. By engaging myself in the process of *re-entering* the field, trying to define my own roles and unveiling my embedded practices, it was clear that I had experienced what anthropologist Mascarenhas-Keyes defines as "schizophrenia between the 'native self' and the 'professional self'" (Mascarenhas-Keyes 1987, p. 180).

Although I am researching at home and not in an-*other* space, in an-*other* country, I feel that my immersion in the field became invasive. I am *elsewhere* at my home, experiencing the tension between being *inside* and *outside* together. But, being at home, this *elsewhere* doesn't allow me to define its boundaries, and requires time and energy from my daily life, making me feel in a condition of apnea, which challenges me personally, the ways I perceive myself and in my everyday life. (Field notes, 24. March 2016)

In terms of a response to the second question proposed by Spencer and Davies concerning the ways that emotional experiences can lead to a deeper field understanding, I am convinced that my re-entry into the field as an anthropological researcher—and the immediate impact of this on the perspectives of both the

operators and myself—provided fresh insights and new motivation to challenge the taken for granted. This included a deeper understanding of the collaborative method and the relational engagement that is required to enable people not only to work together, but to generate a process that enables different points of view to emerge, and in doing so, “strengthen[s] the project rather than weaken[ing] it” (Campbell and Lassiter 2015, p. 21–23). This awareness was confirmed during another point of collaboration with the operators. After 12 months of research, I shared with them the contents of an article I had intended to submit to an academic journal. The article focused on specific and critical aspects of the shelter’s practices. The operators immediately expressed fears about its potential publication, pointing out:

(...) Reading what you wrote, these few lines where you put the focus on the critical aspects... (...) I personally think that I was aware about them, and I tried to act against those aspects, but then you can’t do it, (...) You know, to read these few phrases on the critiques, without speaking about all the discomforts that I felt... (Meeting of 03 February 2016).

The operators claimed that I did not adequately describe the complexity of the situational context, nor the social and institutional dynamics in which they are embedded. This, in their view, would have led to misinterpretations about their advocacy practices. After a challenging discussion, I decided not to submit the paper. Some operators were surprised by this decision. They were sorry, and feared that I had taken this decision in order to protect my personal relationship with them. They were afraid that this decision would damage my research outcomes, and myself, and that this would also compromise our friendship. This, in effect, had forced them to reflect on their own power in influencing my activities as a researcher, and as one of them had pointed out:

Who of us [the operators] has the right to say which critiques will be acceptable, and which not? Who decides it? Isn’t she, as researcher, the one who should have this authority? (Meeting of 01 March 2016).

These episodes made us all aware of the difficulties related to the overlap of different kinds of relationships, many of which had been mirrored by emotions surfacing during my research process. This led us to engage into constant dialogue, with the operators realizing subsequently that exposure of the critical analysis to the public was not a disadvantage, but instead a potential stimulus for other women shelters to reflect on their own practices.

Affective Encounters with Migrant Women

During the development of my process to collaborate with the shelter operators, I contacted a number of migrant women who had lived in the safe house for three to 9 months and asked them to participate in this research project. Eight of the women

accepted. With the exception of one woman,² I decided to involve only those who I had not personally supported in my previous role as a shelter operator so as to make it easier for them to express their criticisms about shelter practices.

The trusting relationship built between shelter operators and women suffering domestic violence is based on specific feminist principles. Of most importance is the principle relationship among women, which focuses on gender solidarity and is similar to that found within a “mother/daughter” relationship where “one woman gives her trust or entrusts herself symbolically to another woman, who thus becomes her guide, mentor or point of reference” (Plesset 2006, p. 100). This concept implies that the operator comes to understand the woman’s point of view in order to support her according to the woman’s own wishes and needs, a process that requires active listening and reciprocity within the relationship. I had started working at the shelter after I had completed my Master’s degree in anthropology, and I soon became aware that my previous fieldwork experiences had instilled in me an ability to listen empathically, and to understand women’s perspectives on their own experiences. However, the relationship between an operator and a woman assumes its sense within the framed space of the shelter, which in itself provides specific professional coordinates through the support provided, eventually leading to various interventions in a woman’s situation. Supporting women who have experienced violence produces a high degree of emotional distress. However, shelter operators have the opportunity to share their distress with their colleagues, and to draw on specific professional instruments, such as psychological supervision, to help manage that pressure. When I decided to *re*-contact some migrant women, I relied on the *know how* formed during my previous work experience, which allowed me to talk openly about the women’s experiences of violence, and which included specific knowledge about the dynamics of domestic violence, the shelter’s practices, and the difficulties that migrant women frequently face when they escape from their abusers. In my meetings with them, I became conscious of their ongoing legal, linguistic and economic difficulties. The fear of being deported from Italy and the difficulty in obtaining work remained the greatest concern for many of them. In particular, the women facing legal barriers constantly expressed feelings of dire uncertainty about their situation. They claimed that they did not receive clear responses concerning these issues, and felt that they were living in a legal limbo, making it impossible for them to plan for their lives and those of their children.

I feel this situation like a torture...the situation is not clear. The last time I was in the police station, in the city where I lived before, they [the officers] told me that I would not obtain a residence permit—There are a lot of problems—“You don’t have work, you won’t pay the taxes, so it will be very hard for you”—After that I thought I go back to my land, or to my husband (...) Here, they told me that they will find any solution. But, I don’t know if I will obtain a permit for 6 months, for 1 year (...) (Interview with a shelter tenant, 2015).

When I asked one of the women why she did not explain to the social assistant the legal problems she was encountering with her residence permit, she replied that she

²I supported this woman during my work experience at the women shelter and afterwards she expressed her willingness to participate in the research.

was afraid that her legal difficulties would compromise her right to retain the financial support that she was already receiving from the social services.

Another issue affecting the women is the precariousness of their economic situations. Some of them are unemployed, and others must rely on limited incomes that are insufficient for them and their children.

I didn't find, really, I didn't find (...) because I don't know people, and places, to which I can ask for work (...) I had a lot of difficulties. A lot! A lot! Furthermore, to get work I need experience, but how can I get experience if no one gives me a work? (Interview with a shelter tenant, 2015).

In my field diary, I reported the emotional distress I had experienced when witnessing the woman's desperation:

I feel sad witnessing how much most of the women who I have contacted for the interview are upset, tired, overcome, lonely, exhausted. (...) How much force must they still demonstrate? How much force will they need again? Am I angry? Yes, I am! How could I not become angry against the world and the reality that oppresses them? They can never count on a life full of positive things! Haven't they suffered too much pain? (Field notes, 15 June 2016)

All the women interviewed reported that in the shelter they had received specific and relevant resources for their empowerment. They were often led to believe that their lives might finally change for the better, and were hopeful that they would be able to provide futures for themselves and their children. However, the structural violence that had made them vulnerable to domestic violence in the first place still existed, and sometimes those barriers became even harder to negotiate and manage.

(An Attempt at) Making Sense of Affective Encounters

As well as my anger about the consequences of structural violence for these women, my engagement in an *informal* relationship with them (in the sense of not being shaped by the shelter's practices framework) generated many doubts as to how, in this new role as anthropologist, I should manage and process the information gathered from them. As a researcher, I needed to obtain ethnographic data, but at the same time, I needed to respect the confidentiality of that information. As a former operator, I recognized the risks that some of these women were still facing, including, in some cases, the continuing abuse and threats of the abuser, or, as mentioned above, the ongoing difficulties associated with the consequences of domestic and structural violence. I then asked myself how I should deal with this information in an ethical sense. What do these women now expect from me? How should I deal with my role as researcher, which requires me to *not* intervene in these situations, or my embedded experience as a women's shelter operator where, on the contrary, I would usually be prompted to intervene on their behalf? This set of questions led me to an emotional crisis:

Last night I did not sleep well. I was nervous. I feel overwhelmed by the problems of the women I interviewed, because I would like to do something for each of them. However, I ask myself if I am managing the task of building *reciprocity* between us. Not only with respect to my ethical principles as a researcher, but also with respect to my principles as an activist, which don't let me stay still and just listen to them, but, on the contrary, push me to consider any possible solutions to their difficulties. Sometimes, to manage the extent of the problems which oppress these women (and these seem to be a huge tangle of difficulties and problems), and the obligation which lead me to give them at least a possible response (even a little one to each of them) are impeding my ability to research in a calm way, and with peace of mind. Furthermore, I cannot talk with anybody about that. Much less with my former colleagues, because of the anonymity of the women. However, they are actually the ones who could better understand my worries. Am I able to manage all this? Am I exceeding my limits, risking giving too much and finally dropping my commitment to them, disappointing the women and leaving them alone again? Is it ethical, this approach? (Field notes, 21 July 2016).

In my previous work as a shelter operator, I had been immersed in the traumatic experiences of women victims on a daily basis, but was also supported professionally and emotionally by my colleagues and supervisors. The shift in my role from the formal and professional context to a more *informal* one, where as an anthropologist I attempted to be as close as possible to the interlocutors' world, was associated with new feelings of vulnerability and uncertainty. In this new role, I was exposed directly to the women's suffering and trauma without the professional and emotional support that had previously been available, and I felt overwhelmed. I asked myself how I should deal with this, and after analyzing my field notes and intersecting those with narratives of the women who had been interviewed, I recognized the ethnographic content of these emotions. This content related to women's need to talk to someone who would actively listen to them, and be capable of understanding their troubles and needs. During this research process, it became increasingly evident that the women were not able to rely on the social and institutional network that was meant to support them, and oftentimes this network only added to their feelings of loneliness and disorientation. "I'm sorry, I know that I am stressing you, but I vent my problems with you because I need someone that can understand me and who I can trust", became a constant outpouring from one of the women interviewed. Notwithstanding the variety of strategies adopted by migrant women to overcome the violence they have experienced, it is their "immigration status, or lack of citizenship, (that) continue(s) to be a major cause of unequal access to justice and protection" (Humphreys et al. 2006 as cited in Thiara et al. 2011, p. 21):

I was at the office where legal information is provided to migrants, but I didn't get any response, and I said to the social assistant that I don't know where I should turn. And she told me I have to go to the Chief of Police Office and that she would call me back in 2 days. But now a month has passed, and she still hasn't called. (Interview with a shelter tenant, 2016).

Yes, I know, you can get a little support here, but for me the situation is not good, because nobody helps me. If my friend hadn't helped me with my children, what would I have done? Would I have worked? No! It wouldn't have been possible! (Interview with a shelter tenant, 2016).

Analyzing his emotional experiences in the field following his interactions with street-related adolescents in Indonesia, anthropologist Thomas Stodulka underlines how his “unprofessional, emotional dilettantism in the field brought [him] to unexpected knowledge” (Stodulka 2014, p. 198), and made him reflect on his positionality. Through the analysis of his affective relationships with interlocutors, he recognized the opportunistic stance in the ways in which they related themselves to him, realizing that, in fact, this was unavoidably linked to his own activist commitment in the field (*ibid.*). As evidenced by the experience of Stodulka, my feeling of being overwhelmed by the women’s narratives and their articulated needs was strictly related to my positionality. It was related to the shift in my role as a shelter operator to that of an anthropologist, and also to my stance as an activist working to remove the structural barriers that affect women. This is a personal commitment that I made clear to the women from the outset of the research. As Spencer highlights, referring to anthropologist Svašek’s reflections about fieldwork:

(...) seeing emotions as discourses, practices and embodied experiences (...) uncovers the intersubjectivity of field relations as a complex intermingling of past, present and future desires, memories, imagination and expectations.(...) our filed emotions may be the result of clinging to particular ideas, habitus, or preferences predating fieldwork, but stirred by it. (Spencer 2010, p. 28)

In addition, the ethnographic relevance of this emotional experience lies in the similarity of my feelings with those of the shelter’s operators. When I was working at the shelter, my colleagues and I frequently experienced the ambiguities and contradictions of the local supportive systems. This experience produced feelings of frustration and helplessness for the workers, and for me often resulted in anger, as documented in my field notes concerning the specific situation of a woman whom I had supported in the last months I worked at the shelter:

I feel repeatedly frustrated. My feelings (...) come from my anger towards a racist, unjust, sexist world. (...) And you are looking at all these, and you have to do something, and you do it, but sometimes you fail, because you feel helpless and you feel that there is nothing to do. (...) As if the problems concerning the women’s residency permit, language difficulties and unemployment were not enough! (Field notes, 01 April 2015)

In fact, frustration, fear, impotence, indignation, tiredness, anxiety, and pain comprise the set of feelings that the operators expressed during a joint supervision when they reflected on my research issue. The shift in my positionality had brought me closer to daily life of these women, and the ongoing structural oppression that they must deal with daily. It also highlighted the loneliness and isolation experienced by women victims of violence, all of which highlight the contradictions of the support system’s goals.

It became clear to me that the analysis of this specific experience highlights that emotions cannot be considered as a mere subjective experience but must be treated as social facts (Stodulka 2014, p. 199) that speak about the interlocutors’ reality.

Reflecting systematically on the lived experience of fieldwork relations may play an important role in understanding the dynamics of power in the field and,

in particular, the living embodiment of local and global power hierarchies (Spencer 2010, p. 17).

The ethnographic understanding that I obtained from this specific experience responds to Spencer's third question concerning the influence of emotions on participant observation, and the wider interpretative enterprise of the fieldwork. Firstly, my emotional experiences highlight how positionality represents a red thread between the implications of my research encounters with the women and with those of my former colleagues. Secondly, by treating these experiences as *social facts*, I was able to obtain a wider understanding of the structural difficulties affecting the migrant women whom I interviewed, even after they had left the safe house. I have now recognized that my multiple identities and related affective dispositions and involvements in the field were simultaneously subjective and scientifically relevant (Stodulka 2014).

Emotion, Self-Reflexivity, and Theory

The experiences described above address the issue posed by Spencer and Davies concerning the relationship between emotions, subjectivity and scientific goals. The subjective dimension of the research process emerged from my relationships in the field, and constantly molded, reinforced and subverted my emotional experiences that in turn became scientifically relevant in shaping the methodology of my ethnographic inquiry. These experiences led me to commit myself to an ongoing reflective process concerning my relationships with the operators who made me challenge my *taken for granted*, engaging in a collaborative process, and with the interviewed women, who made me aware of their ongoing structural difficulties.

Emotions, (...) are ways in which we engage actively and even construct the world. They have both "mental" and "physical" aspects, each of which conditions the other; in some respects they are chosen but in others they are involuntary; they presuppose language and a social order. Thus, they can be attributed only to what are sometimes called "whole persons", engaged in the on-going activity of social life. (Jaggar 1989, p. 159)

During her research project in Vietnam, the Vietnamese anthropologist Nguyen (2007) analyzed her relationship with Phurong, a Vietnamese female interlocutor. Starting from her position as a native anthropologist, Nguyen Thu Huong pointed out that while conducting research in domestic settings can provide a sense of familiarity and comfort, it is also imbued with its own set of challenging experiences, which, in her case, was a range of unexpected emotions generated through her relationship with Phurong. Analyzing those feelings, Nguyen Thu Huong became aware of the ambiguity of her positionality, whereby the cultural proximity with her interlocutor had led her to develop personal prejudices concerning Phurong's attitudes, and on the opposite spectrum had fuelled a sense of responsibility aligned with the feeling that "there was more than just an encounter between a researcher and her informant. It seemed as if there was a sisterly bond between Phurong and me" (*ibid.*, p. 34). This highlighted the relevance of reflecting on the anthropologist's own

biography and subjectivity, and as an ethical act generated by a sense of responsibility for the other's life because "it is important to explore how this subjectivity comes into play in the dialogical relationship between the researcher and the people being studied" (ibid., p. 31). Nguyen Thu Huong's experience leads again to Stodulka's perspective, as mentioned above, which concerns the scientific relevance of the emotions in the field as social facts. This is a perspective that mirrors my own experiences with interlocutors, and also answers Spencer's fifth question by confirming that the ethnographic analysis of emotions is in fact relevant to the training of an anthropologist. And this is due precisely to the ethnographer's emotional vulnerability, which enables "compelling analyses of particular local worlds" (Smith and Kleinman 2010, p. 185).

Conclusion

In the evolution of my research, the consideration of emotional experiences as being ethnographically relevant does, as Spencer and Davies suggest, respond to *empirical affect montage* generated by the practice-oriented self-reflexive approach (Stodulka et al. 2019) adopted in my research. This perspective is mirrored by the ongoing documentation of my early interactions in the field, where my feelings and reactions became a relevant part of my field notes. This ongoing documentation and analysis allowed me to reflect systematically about my emotional experiences, lending them empirical robustness while at the same time generating a complementary set of ethnographic data (ibid, p. 10). Furthermore, focusing part of my reflections on my emotions highlighted my ethical commitment as a researcher, and provided greater transparency about my research intentions in view of my research partners. In reflecting on my positionality, I analyzed my interactions in the field, unveiling the *taken for granted* of embedded practices and making explicit my perspective, which subsequently became one of the perspectives that the research process had aimed to recognize; that is, the point of view of all the research partners. This process also mirrors the reflective stance of the shelter's practices that are based on feminist principles, and most importantly the principle to "begin from oneself". This is itself rooted in the Italian feminist principle that promotes engagement in rethinking woman's subjectivity, taking herself (a woman and her gendered experiences) as a starting point for building an empathic and trusting relationship with other women (Plesset 2006, p. 60). This specific background has shaped the habitus of my embedded practices as a shelter operator. As a researcher, I shifted this background to the research process itself, not only in imbuing it with a specific feminist perspective for social justice, but also in shaping the approach, which allowed me to "begin from myself," and to reflect on my own positionality in respect to my relationships in the field and the ethnographic data resulting from it.

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Conflicted Emotions: Learning About *Uchawi*



Gerda Kuiper

Introduction

This contribution discusses my conflicted feelings as an early-stage researcher when I discovered that the emotional process through which I learned about *uchawi* (usually translated as “witchcraft”) did not resonate with mainstream anthropological understandings of this phenomenon. The context was as follows: in January 2010, I travelled to Lindi, a town on the South Coast of Tanzania, with the aim of doing research on land tenure relations. I flew to the country together with a fellow student who set out to do research on traditional healing (*uganga*) in another region. I did not suspect that I, too, would learn a lot about these practices and about their relations to suspicions of *uchawi*. Only later did I read the work of authors who noted that *uchawi*¹ is a part of everyday life on the Tanzanian coast and in Northern Mozambique. It is considered a type of knowledge or a skill that is used to inflict harm on others. It is usually attributed to greed or jealousy on the part of the *mchawi* (“witch”), or the person who enlisted his or her help (Becker 2008, p. 164; Green 2015, p. 327; Larsen 2015, p. 223; Mesaki 2009, p. 132; West 2005, p. 238). Importantly, Becker (2008, p. 160), in her study of the spread of Islam in Lindi and its rural environs, mentioned that “human agency [is] intrinsic to the notion of witchcraft.”

Although I had not expected it, I also encountered *uchawi* in Lindi. I learned about it unintentionally, through my experiences of living with a host family in Lindi. Not only did I, over time, come to see the everyday importance of suspicions

¹ *Uchawi* is a Swahili term. In the Mozambican context described by West (2005), the Makonde term used is *uwavi*. Many residents of the present-day Lindi have ancestors who originated from this Mozambican region. There is thus a close connection, despite the national boundary dividing the two areas.

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of *uchawi* and of the search for healing and protection, but it also became a lived reality for me. It was a factor with which I had to reckon in my relationships in Lindi, but which at the same time I understood little about. It therefore triggered emotions such as fear, irritation, and doubt. Reflecting on these emotional experiences enhanced my understanding of the role of suspicions and acts of *uchawi* in land tenure and other relations in Lindi. Furthermore, this reflection not only helped me in thinking about “the field,” but also about anthropology as a discipline. I will elaborate on this learning process in this chapter. After a description of the research context, I will briefly introduce scholarly discussions surrounding the topics of witchcraft and “emotional learning.” A discussion of several of my own emotions and affects in relation to *uchawi* forms the core of the chapter. I will conclude with some methodological, epistemological, and ethical considerations.

Arriving in Lindi: What Did I Know?

Although Lindi’s sisal plantations in former times attracted labor migrants from as far as present-day Northern Mozambique, the region has been economically marginalized since the time of British rule (Becker 2008, p. 95; West 2005, p. 104). The peripheral status of the region was for a long time epitomized by the deplorable condition of the 500-kilometer road from Lindi to the economic center of Tanzania, Dar es Salaam. Plans to tarmac this road never seemed to materialize (Seppälä 1998). However, when I first arrived in 2010, most of the road had been tarred and it was possible to travel to Lindi by bus within 1 day. The last stretch of the road was only finished in 2015, just shortly after the discovery of gas in the region. Despite this recent opening up of the area, feelings of isolation remain. Inhabitants of Lindi complained that the new job opportunities that arose with the discovery of gas were filled by people from other parts of Tanzania. I also experienced an enduring prejudice against the region when talking to people in Dar es Salaam, who pitied me for staying in such a “backward” area. Inhabitants of Lindi are perceived, both by themselves and others, as ignorant, “unmodern,” and captured in poverty.

I set out to study land tenure relations in this rural region for a period of 3 months, as part of my master’s degree in cultural anthropology. I stayed in Lindi and conducted unstructured interviews there; I also visited surrounding villages, and I interviewed a handful of government and NGO officials. I already had a good proficiency in Swahili, and I therefore was able to follow everyday conversations and execute my interviews in this language. I stayed with a host family, at that time consisting of a widow in her late 40s, one of her adult daughters and two of her grandchildren. I soon was considered, and considered myself, to be part of their family. This not only was based on feelings of mutual care, but also was a consequence of, unarticulated, strategic considerations: I felt safer staying with a family, and my host family could make more financial demands on me as a family member than if I had remained a guest. This was significant, as they had no permanent source of income

and relied on family members and temporary jobs for their everyday subsistence. My contributions to the family as a “working child” were thus welcomed. Taking part in the daily life of this family turned out to be overwhelming to my senses as well as to my intellect. After returning home, I realized I had learned more through this “participant observation” than through methods which were more conscientiously applied. I decided to write my thesis about the one plot of land that I had learned about most: the plot I had stayed on with my host family. Through describing this case and contextualizing it with information gathered during interviews, I showed some of the complexities of land tenure practices vis-à-vis legal regulations in Tanzania.

But not only did my host family play a crucial role in my thesis, my involvement with this family also turned out to be more enduring than I had initially foreseen. I came back to Lindi several times in subsequent years to work as a volunteer and I stayed with the same family. Although I had finished my research on land tenure relations, my learning process continued. Specifically, I learned much more about *uchawi*, although I had not prepared myself for this. Witchcraft is a classic topic in anthropology, and I had some familiarity with this literature before embarking on fieldwork, but this did not prepare me for the possibility that I would encounter this phenomenon myself. For one, anthropologists working on witchcraft or “evil” in other regions have mostly focused on *reactions* to evil, implicitly and sometimes explicitly denying that witchcraft itself is real (Van Beek and Olsen 2015).² Second, most of the authors, as reviewed in Rutherford (1999), followed the functionalist approach of Evans-Pritchard (1976) in his classic monograph on the Azande. These authors provided an etic explanation of the internal rationality behind accusations of witchcraft. More recently, anthropologists have aimed to show that the occult can be an integral, constitutive part of “modernity.” These authors focused on narratives and stories and thus drew attention to the *discursive* (not the *immediate*) power of witchcraft. These authors, in their writing, did not take into account that the research subjects themselves do not understand witchcraft as an idiom but as something very real and immediate (Ashforth 2005, p. xiv; Rutherford 1999, p. 97). Significantly, the anthropologist in these cases mostly remained an outsider, not directly implicated in relations which included witchcraft and threats thereof. More personal implications and experiences were merely mentioned in a footnote or in the form of an anecdote.³ The only exception I was familiar with at the time was Ashforth’s book on his bewitched friend *Madumo* (2000). He vividly described not only the experiences

²Van Beek and Olsen stated that the real problem “for anthropologists” is not evil itself but individual suffering because of *reactions* to evil (2015, p. 10). Ferguson (1999, p. 120) explained that this has been a common approach among anthropologists, partly because accusations of witchcraft in themselves can be violent acts. Nevertheless, I feel that such an approach sidelines the suffering of those who feel they fell victim to evil *acts*.

³Only long after my initial fieldwork did I read ethnographies which included the anthropologists’ own experiences in the analysis of witchcraft. I will refer to these studies in the discussion.

of his friend, but also his own involvement—for instance, through financing his friend’s treatment—and his own doubts, anger, and concerns. Impressive as this book is, however, it is an anonymized and even partly fictionalized account which makes no reference to academic theory. I thus did not relate Ashforth’s experiences to my own position as a “researcher.” At this point in time, what I gathered from these texts was that one could choose to study reactions to and narratives on witchcraft, and one could likewise choose not to. As I simply was not interested in the topic, I (perhaps naively) did not consider the possibility beforehand that I would encounter these discourses frequently in my own field site. I was even less prepared for the possibility that I, myself, would become implicated in relations in which (suspicions of) witchcraft played a role. But through the formation of intimate and long-lasting relationships, I became a “participating observer” (Hume and Mulcock 2004, p. xii) in everyday life in Lindi, including in matters of *uchawi*. This was an informative as well as an emotional process.

Emotions and Empiricism: How Did I Learn?

In conventional empirical approaches, it is assumed that emotions distort scientific understanding. This approach forces the anthropologist, who is supposed to gain inside knowledge while remaining a detached outsider, to perform a complicated balancing act (Davies 2010; Hume and Mulcock 2004). Okely (2012, p. 9) pointed out that the “total” experience of anthropological fieldwork makes it virtually impossible for the researcher to separate the emotional self from the intellectual self. Moreover, perhaps this separation is also not desirable. Davies (2010, p. 1), following a “radical empirical” approach, stated that much can be learned from a vigorous analysis of emotions evoked during research. Green’s work (1999) on widows in Guatemala living under violent conditions exemplifies well how reflecting on their own emotions can help ethnographers gain a better understanding of the object of their research. Green’s own fear helped her to understand how fear suffused the daily life of these women. She came to understand how ambiguous acts of intimidation became effective in creating terror. Green at the same time acknowledged that, even though she also experienced fear, her position was different from the position of the widows; she had the option of leaving. This inequality created feelings of discomfort. “Fear joined me to the people and yet separated me from them as well” (ibid., p. 20). The need to balance intimacy and distance, and the impossibility of overcoming differences in power, can put the researcher in emotional and “awkward” social spaces. According to Hume and Mulcock, these spaces themselves can be productive:

By rigorously analyzing our own emotional responses to particular field encounters (...) we can usually learn something about the values of those around us and the social processes we have become part of during the research process. (2004, p. xxv)

This process is complicated further, and therefore also becomes more enriching, by the fact that the anthropologist does not only position him- or herself, but is also ascribed a certain position by the people he or she works with (see, for an example, Hume and Mulcock 2004, p. xvi). Merely by staying in my host family's home, I obtained a certain position within existing family and neighbor relations. I soon perceived there were certain tensions in these relations, but it took me longer to realize that *uchawi*, and suspicions thereof, formed an integral part of these relations. Unlike most authors writing on the topic of witchcraft, I did not primarily learn about the phenomenon through interviews and conversations or even through gossip (Ashforth 2005; Ferguson 1999; West 2005), neither did I learn about the topic intentionally, as for instance Stoller (1984) did by becoming a healer's apprentice. I learned about *uchawi* inadvertently, even unwillingly by times, through becoming part of existing relationships surrounding my host family in Lindi. This learning was not mediated through any kind of formal method but rather through my cognitive as well as emotional struggle to cope with unplanned and by times overwhelming experiences. The "awkward" spaces I found myself in were initially confusing and frightening, and made me feel I had failed as a researcher. Yet, they were eventually also enlightening, and shaped my interpretation of both everyday life in Lindi and existing anthropological theory on the topic of witchcraft (Hume and Mulcock 2004, p. xviii).

Fascination

This learning process already started on my second day in Lindi. A relation from Dar es Salaam had accompanied me to Lindi.⁴ After arriving, she got a terrible headache and decided to visit an *mganga*, a healer, to find a cure. He told her the illness was caused by a man she knew, who had put something in her body. I observed some of the treatments. For instance, the *mganga* wrote her name and the name of her father on a plate with a special type of red ink and prayed over it. He then washed off the text with water, and the patient drank the mixture of water and ink.⁵ While I was observing this procedure, I wondered whether I should not advise her to go to the hospital, but I felt it was not my place. I considered myself to be a mere observer, not implicated in the illness or the treatment. I simply took the practices of healing, and the suspicions of malevolent intent behind the illness, as some of the many interesting aspects of this new, fascinating environment that I was being exposed to.

⁴In order to protect the anonymity of the persons mentioned, I do not use names and have omitted certain personal details.

⁵This ritual was also mentioned by Becker (2008, p. 302) as *kunywa kombe*, although she stated they are verses of the Quran which are written with this ink.

Feeling Irritated

I started to hear frequent stories about *uchawi*. Witchcraft featured prominently in the Nigerian and Tanzanian movies aired on television. Furthermore, I heard a lot of local gossip about it. These stories sometimes told of violent excesses, similar to those that I was familiar with from global media reports, for instance, the killing of albinos. However, I more frequently encountered less dramatic, everyday, but pervasive experiences of the occult in Lindi. One night, I was kept awake because of the drums of a spirit exorcism ritual at a neighboring house. Another day, while on my way to Dar es Salaam, my bus passed by streams of people who were walking to the next village, where a travelling *mganga* was practicing that day.⁶ The omnipresence of the occult—about which I understood so little—started to irritate me. Although witchcraft can be a topic surrounded by secrecy or silence,⁷ suspicions of *uchawi* were quite openly discussed in Lindi, at least when the cases did not in any immediate way involve the person who was talking. And so I listened to numerous stories about politicians, Europeans, landowners, and business people, who were suspected of having grown successful or rich through certain magical practices, usually at the cost of others. These stories seemed irrational to me. I interpreted them in a functionalist manner: I saw witchcraft accusations as a way of preventing people from prospering. West called this the “anti-developmental dynamics of sorcery” (2005, p. 192). Nevertheless, on some occasions I was unable to find “rational” explanations for what was going on. One morning I was woken up by the excited voices of the children in our house, who had just heard that there was a “witch”—an unknown naked old woman—who had gotten stuck to a neighboring house due to protective *dawa* (medicine) used by the owner of the house. The woman had to be rescued by the police from the gaze of the crowd.⁸ I was annoyed by the whole situation and the excitement around it. It simply did not make sense to me. I grew weary of hearing about *uchawi* and everything related to it. But the frequency with which the topic came up made me realize that it was not possible to consider *uchawi* or accusations of it unimportant or irrelevant, an awareness which irritated me even more.

⁶This *mganga*, originating from another region in Tanzania, was touring Lindi and the surrounding villages and moved from house to house in an attempt to “cleanse” the area of witches. His practice attracted many spectators. It showed similarities to the collective cleansing rituals, sometimes turning into popular movements, as described in the historical work of Becker (2008) and Green (2015). But unlike in the rituals described by these authors, this *mganga* exposed those who he suspected of *uchawi*. I disapproved of this, and I never went to witness his practice. The authorities in Lindi eventually prohibited him from continuing with his tour.

⁷Especially in cases where words themselves are considered to have power (Favret-Saada 1979; Larsen 2015).

⁸A South African friend of Ashforth described something similar: “There was a woman who was found naked early in the morning. She was just standing there in somebody’s yard. You know we have this thing here that if someone has protected their house with strong *muti*, and then a witch comes in the night to do whatever it is she wants to do, she will be trapped” (Ashforth 2000, p. 81).

Shock

However, despite this irritation, I did yet not feel that these suspicions or practices affected me personally in any way. Fears of *uchawi* and of its impact on my relations in Lindi only got me in their grip 2 weeks after returning home. I was informed that my host mother, with whom I had established close bonds in the 3 months that I had stayed with her, had suffered from a stroke. She was severely ill and it was feared that she would die. The doctors told her family that they could do nothing for her. The family then decided to bring her to an *mganga*, an old lady living in a village some distance from Lindi, under whose care my host mother slowly started to recover. When I found out about her illness over the phone, I was shocked and concerned not only about her health, but also about my relation with her and her family. I feared that perhaps her illness would be indirectly blamed on me, that is, on jealousy because of her relationship with me, but no one said a word about this.

Confusion, Doubt, and Feelings of Guilt

A long period of rehabilitation began for my host mother. I was in doubt: Where should she look for a cure? Other authors have described how people they studied had their doubts about, for example, the trustworthiness of healers (see, for instance, Evans-Pritchard 1976, p. 107). But I had these doubts myself: Should I advise my host mother to return to the hospital? Or could she really be treated more adequately by an *mganga*? I also was unsure about my own role: I knew my host family did not have the means to finance extensive treatment, but was I willing to pay for it? And how would my decision to either pay or not impact on our relationship? Finally, I decided to send part of the money needed—partly not only out of feelings of care but also out of a feeling of guilt. I felt that this misfortune had happened because of my stay with my host family, though I did not have any logical reason to assume so.

My host mother stayed with the *mganga* for several months. Part of the treatment consisted of praying and other ritual acts, but the most extensive part was simply physical exercise. After several weeks, my host mother started to be able to talk again, and after several months, she got back up on her feet. I only later realized how fortunate she had been to find someone who was able to treat her. It is very difficult to assess what one should do in case of illness. A patient can go from hospital to healer and back without finding any relief. We ourselves also had an encounter with a dubious *mganga* during my next stay in Lindi, 6 months later. My host mother called this man to provide *kinga* (protection) for the house we lived in and for its residents. The *mganga*—who did not take off his tinted glasses during the entire visit—put together some *dawa* for the high blood pressure of my host mother. He also put other *dawa* in a bottle, which was buried in the mud floor behind the front door, so that only the opening of the bottle was still visible. Most of the other acts he performed, for example, sitting down at one corner of the house and rubbing over

the soil with his bottom, made less sense to me. I became especially suspicious about his sincerity when he raised his price considerably after seeing me. I later found out that my host family also had their doubts about his qualities. We even laughed together about some of his practices. Ensuing discussions about how one could find a trustworthy *mganga* showed once more how difficult it is to assess what one needs to do when looking for treatment and protection.

Fear

Only years later was I told explicitly that the family, while acknowledging that my host mother had suffered from a stroke, indeed suspected that this stroke had been caused by someone who was envious of her, partly (but not only) because of my presence. My host mother claimed she had found a small marble under her tongue when she woke up on the day she got the stroke, which she took as a sign of bewitchment. But my fears of being cast away by the family because of this had proved to be unfounded, although I am not sure what would have happened had I declined to contribute to the payment of the treatment. However, not only did I at some point experience fear for the consequences of *suspicious* of witchcraft, at a later stage I also started to become more worried about *practices* of witchcraft. This fear was triggered by an event which took place just a few days after the visit of the dubious *mganga*. When I woke up that morning and opened our front door, I saw a small hole in the ground with a diameter of approximately half a centimeter. I called my host sister, who looked at it briefly, took a small stick, and started to lay bare the small tunnel. It ran up to the mud wall, passed a small hole, which had not been there previously, in the wall next to the front door, and then ended exactly where the bottle with *kinga* was positioned. I was shocked because I could not think of a “rational” explanation of how anyone could have made such a perfect little tunnel, leading precisely to the bottle. I took it as a sign of someone trying to harm us, even if, only by scaring us and nothing else. We did not discuss the little tunnel further, although it was clear we all had the same—uncomfortable—thoughts. The next day I told my host sister that I had not slept well that night. She admitted she was also worried, though, even then, we did not explicitly discuss our fears. Likewise, Ashforth (2000, p. 140) described how one morning something occurred for which he had no logical explanation (in that case finding a strange brown smear on the wall of the house). Some of his host family members suspected an attempt to bewitch, but Ashforth was reluctant to follow that line of thought: “Knowing not to believe in witches, and preferring not to believe in malice, I knew not how to read the signs” (ibid.). During the day, I was as reluctant as Ashforth to believe that witchcraft actually existed. But during the night I was not so sure anymore. The idea that an inexplicable someone or something had been, and perhaps still was, wandering around our house at night terrified me.

Feelings of Estrangement

My fears were a strong emotion which I could not share with my family and friends “back home.” I tried on a few occasions, but they simply would start to look for “rational” explanations. I also was reluctant to discuss it because I did not want to reinforce the picture of Lindi as a backward region or invoke Orientalizing images of Africa, in general. This inability to share, which ran deeper than merely my experiences with *uchawi*, created feelings of alienation even after returning home. I felt awkward (see also Hume and Mulcock 2004), especially as I also always, to a certain degree, have remained an outsider in Lindi, not least because I always had the option to leave (as also pointed out by Green 1999, p. 20). At the same time, I realized these feelings of estrangement were productive: they helped to unpack what I had learned. However, I also started to feel estranged from my discipline, which was a less productive emotion. My inability to make sense of my experiences of witchcraft in a way that was acceptable for an anthropologist made me feel at a loss. The “untroubled authority” (Rutherford 1999, p. 93) which other authors (who had come to very different conclusions than I) had assumed on this topic made me feel like a failure. I felt I had crossed a line and feared I had “gone native.” I had perhaps even done something immoral by accepting the possibility that acts of witchcraft might actually take place. Only after rereading some of the earlier works on witchcraft, and ones I had not been familiar with before, I realized that many anthropologists must have gone through similar experiences. They just, mostly, did not write about it.

Abating Fears

After this frightening episode, I continued to spend longer periods of time in Lindi and my fears abated. Not only because nothing “remarkable” happened for some time afterward, but also because I saw the reaction of my host family to events like this and to cases of illness. They look for protection and cures, which shows their concerns. At the same time, they are determined not to let *uchawi* determine their lives. After all, the possibility that someone falls ill or suffers a misfortune because of the ill will of another person can be very disturbing—but it is also part of everyday life in Lindi, and one can anticipate it. It had initially been a shock to me when I discovered that my host family was implicated in *uchawi*. But this gradually became a fact of everyday life to me—just as it was to my host family. Once that happened, I was able to reflect on how *uchawi* had shaped my, as well as others’, emotions, relations, and decisions in Lindi.

Discussion and Conclusion: What Did I Learn?

My first aim in this chapter has been to show that a reflection on one's emotions can provide insights into "the field," in my case into everyday life in Lindi. For me, *uchawi* was not the topic of my research, nor has it been subsequently. It was also not of particular interest to me, and I sometimes felt uncomfortable with the subject. Despite my own reluctance, I gained intimate, though partial, knowledge of the topic through my engagement with people for whom the possibility of bewitchment is a reality. I would not be able to give a taxonomy or classification of witchcraft and magic, such as Evans-Pritchard (1976) did. I would also be unable to provide a detailed analysis of how "modernization" in the form of new economic opportunities through the discovery of gas has impacted occult relations and practices. But through my engagement with the social environment I encountered, and my emotions throughout this process, I gained insights into the emotional, psychological, and social impacts of *uchawi*, and on its influence on individual decision making in Lindi. Anthropologists often have edited themselves out of their ethnographies, especially when working on the topic of witchcraft. However, as shown by Favret-Saada (1979, p. 17), in her book on the topic of witchcraft and the power of words in France, it is in certain contexts not possible to be a mere observer without becoming a participant. Perhaps it is also not desirable. Tourigny stated:

When, and to the extent that we silence our receptivity out of fear (...), we filter our perceptions and therefore mute our understanding. Imposing a distance between our participants and ourselves, and again between what we see and what we choose to feel, may reduce fear () we filter our perceptions and therefore mute our understanding. (2004, p. 124)

Admitting to the fear I experienced in Lindi allowed me to arrive at very different conclusions than Seppälä did. He stated that witchcraft in Lindi was a "myth" (1998, pp. 30–31) created by people from other regions to reinforce the peripheral status of the region. Although it is possible that references to witchcraft have been used in portraying the region as backward, my emotional experiences showed me that *uchawi* in Lindi was much more than a discourse. I learned that the possibility of falling ill or suffering misfortune due to the malevolent acts of other human beings is omnipresent. On the other hand, I also learned that there is a lot of ambiguity and uncertainty attached to the process of looking for causes, culprits, and cures in case of illness or misfortune. Furthermore, I eventually came to realize that this threat is nothing extraordinary but a part of everyday life. At the time I wrote my thesis, I had already come to realize that suspicions of witchcraft had repercussions on relations in Lindi, including relations of land tenure. I mentioned in my thesis that my host mother had once decided not to go to court when someone tried to occupy her land, partly because she was afraid that the thief would retaliate with the help of an *mganga*. I therefore made the general statement that witchcraft was a pervasive factor in power struggles. Yet, as many authors did before me, I focused on witchcraft accusations and on fears of witchcraft, and not on acts of *uchawi* and their effects. I was too confused about my host mother's illness to include it in my analysis and writing. I had not yet started to grasp how the land my host mother lived on was

involved, or was suspected to be involved, in her illness, and thus did not reflect the full complexity of the case in my thesis. Only a reflection on my emotional experiences during prolonged “participant observation” provided me with more coherent insights into the place of *uchawi* in this specific case and in everyday life in Lindi, in general.

A second aim of the chapter has been to show how my feelings of having failed as a researcher shaped my interpretation of many of the influential ethnographies on the topic of witchcraft. Most authors have put much effort in making the belief in witchcraft appear less “exotic.” Evans-Pritchard (1976) pointed out time and again that the Azande were not unaware of empirical causes of death or illness, and that there was a certain rationality behind their beliefs. Other writers followed this approach of explaining the occult and making it understandable. However, as pointed out by Rutherford (1999, p. 98), such an approach reasserted the colonial distinction between Africans and Europeans. More recent works have tried to overcome this by showing that witchcraft can also be “modern,” yet in this pursuit the authors still attempted to apply a Western rationality and did not reflect on their own position in wider political projects. Despite their efforts to take the “other” seriously, most authors have been reluctant to accept the possibility that actual acts of witchcraft take place.⁹ They sometimes described how they themselves started to use the idiom of witchcraft, and even acted accordingly, but they usually emphasized that they did not “really” believe in it. “I, too, used to react to misfortunes in the idiom of witchcraft, and it was often an effort to check this lapse into unreason” (Evans-Pritchard 1976, p. 45). Ferguson (1999, pp. 118–122) proposed understanding fears of witchcraft not as “unreason” but as fear of immediate acts of violence. However, he attempted to make this “reasonable,” for example, by including acts of poisoning in his definition of witchcraft. I agree with his understanding of witchcraft as a form of violence, yet I found that poisoning in Lindi does not count as *uchawi*. Although bewitchment can cause death, the ultimate goal is not to kill but to make someone suffer. I feel that such “rational” explanations of witchcraft might make sense to a scientific audience but do not reach the essence of the matter. They seem to be the cases of the “domestication” of occult practices “to the analysts’ own sensibilities” (Kapferer, 2002, p. 20). Furthermore, apart from only allowing for limited understanding, these authors also neglected, “their own positioning within the anthropological project of proving the ultimate rationality of non-Western practices and beliefs” (Rutherford 1999, p. 92). By providing an explanation from their own point of view, which established their authority as anthropologists, the authors still distanced themselves from “the other” and did not question their own position in wider “webs of power” (ibid., p. 93).

⁹Anthropologists shared this reluctance with colonial and postcolonial legislators. They, too, have struggled to define witchcraft, and struggled with the question what are acts of witchcraft, or rather accusations of witchcraft which should be punished (Rutherford 1999, pp. 98–100). Mesaki (2009) described how Tanzanian legislators have followed an eighteenth century English law which sanctioned “pretended” acts of witchcraft.

To conclude, I feel that anthropologists need to have the audacity to be vulnerable, in the field as well as in writing (Behar 1996), especially with regard to the topic of witchcraft. I was thankful to, at a later stage, read the more distinctive and critical works of, for instance, Favret-Saada (1979), Stoller (1984), Rutherford (1999), and Kapferer (2002), which helped me to come to terms with my “awkward” feelings. Had I read more of these writings early on, this could have better prepared me, insofar as such preparation is even possible, for encountering *uchawi* in the field. At least it would have made me feel less of a failure as an anthropologist. Moreover, I argue that a more vulnerable and self-reflexive approach can do more justice to the fears and suffering of those who consider themselves to be victims of witchcraft, and can bring their epistemologies to the foreground instead of obscuring them.

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Part II
Reciprocity in Research Relationships

Reciprocity in Research Relationships: Introduction



Mechthild von Vacano

For fieldwork relationships to be reciprocal is an ideal most anthropologists would likely subscribe to. But beyond the rather general notion of mutual exchange, the conceptual as well as practical details of reciprocity in research relationships remain mostly vague. It seems to be as much a political and ethical as an epistemological issue, which somehow addresses the question of im/balance between researcher and interlocutor. As such, most ethnographers experience reciprocity in fieldwork relationships as a challenging and emotionally charged topic. One of the most prominent feelings in this context is certainly guilt (see Gable 2014). But beyond that, it remains a topic with which ethnographers associate a whole range of contradictory feelings. During the workshop that preceded this book, a working group on “taking and giving” in research relationships compiled the following list of such feelings:

Exhaustion, anxiety, frustration, disappointment, sadness, inaptitude, inner conflict, insecurity, pressure, constraint, inadequacy, shame, discomfort, guilt, compassion, loyalty, solidarity, mutuality, reciprocity, dependency, responsibility, obligation, friendship, belonging, acceptance, affection, comfort, connectedness, joy, appreciation, gratitude.¹

Oscillating between gratitude and guilt, these feelings indicate a fundamental tension characteristic of anthropological research relationships.² This tension arises from the conflict of (anthropological) research ideals with multifarious—and to some degree constitutive—inequalities in field relationships. Since reciprocity

¹This list was presented at the workshop “The Researchers’ Affects,” December 3–4, 2015, in Berlin. As former moderator of the working group, I owe thanks to the participants Patrick Keilbart, Mirjam Lücking, and Veronika Siegl for our joint discussions, which inspired the editing of this section and the writing of this introduction.

²By research relationship, I refer to relationships between the researcher and other people, while disregarding other forms of relating that constitute the anthropological field (Spencer 2010).

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stands for the effort to overcome or mend these inequalities, I suggest taking them as an analytical starting point to identify different strategies of creating reciprocal research relationships.

For one thing, research relationships are constituted by an inequality of roles and motives: one party engages in fieldwork with the intent of obtaining data from the other. While the first is dubbed “researcher,” the terminology for the latter has changed over time and still varies according to different epistemological–methodological premises. While some ethnographers choose to stick with the term “informant,” others prefer to use terms like “interlocutor,” “protagonist,” “participant,” or “collaborator.” These terminological shifts and turns indicate how uneasy anthropologists feel in describing their positionalities vis-a-vis the people they study (with). This discomfort stems from the fact that the inherent inequality of research relationships is at odds with a certain power-critical commitment and the value of mutuality (Sanjek 2015) broadly shared among anthropological scholars.

Inherent to the design of the ethnographic method is a blurring of the lines between personal and professional communication: as ethnographers, we adapt our personalities to create rapport, mobilize our social skills to gain trust, and use our empathic competencies as epistemological tools (e.g., Spencer 2010; Svasek 2010), while shifting between immersion and detachment (Davies 2010). Even if personal relationships, like friendship or kinship relations, may develop over the course of fieldwork, the actual research relationship is constituted by the transaction of information in which the researcher is on the receiving end of insights provided by the research participant. Even if anthropologists engage in open and personal forms of dialogue, ultimately, they engage in these relationships with the specific purpose of knowledge construction. Their motives lay beyond the social relationship as an end in itself; instead, the relationship primarily serves as a means of attaining others’ self-reflective accounts of their lifeworld (van der Geest 2015, p. 4). Ethnographers’ motives are guided by a scholarly interest in the research issue, perhaps accompanied by an agenda to foster some form of socio-political change. Whatever ethnographic style or format anthropologists may choose, with its publication (at the latest), the ethnographic material they gathered will be transformed into academic currency and will serve to advance their professional careers (Stodulka 2014, pp. 123–124).

In addition to this inherent inequality, other power asymmetries shape research relationships, depending on the respective positionalities of the individuals and collectives involved. Owing to the historic genesis of anthropology as a scientific discipline and the continuing hegemony of “Western” academia, much of the anthropological knowledge perceived is still produced by members of former colonizing societies conducting fieldwork in formerly colonized settings—even though the exclusivity of this model has been dismantled and its dominance continues to be challenged. Driven by a certain disciplinary ethos, anthropologists continue to display a preference for studying marginalized groups or communities. Postcolonial power constellations and the general inclination to “study down,” often correlate with significant prosperity gaps, in which even student ethnographers with limited financial resources appear as “representatives of relative wealth” (Kingston et al. 1997, p. 27). Yet even anthro-

pologists conducting their fieldwork at their “doorsteps” often find themselves in a position of relative privilege with regard to class and educational background, or as urbanities studying rural conditions.

Anthropology, just as other social sciences, owes much of its power-critical self-awareness to the strenuous criticism of postcolonial and feminist scholars, who have raised awareness of structural power relations and their multifarious effects on research.³ As a political-ethical, and often epistemological, consequence of these criticisms, it has become a regular demand to call for reciprocal research relationships. The notion of reciprocity, however, can refer to a whole range of approaches, which I suggest categorizing into three basic strategies. The first set of approaches addresses the inherent inequality of the research relationship itself by redefining the researcher–researched relation in an effort to reduce (or overcome) its inherent power gap: the paradigm of intersubjectivity or radical empiricism (Jackson 1989), for instance, challenges the opposition of the knower and the known, by its epistemological premise of mutual discovery. While participatory or collaborative research methods open up the position of the researcher to include (a least some) research counterparts as research partners or co-researchers (e.g., Fluehr-Lobban 2008), other approaches focus on representation and question the researcher’s exclusive position of authorship (e.g., Clifford and Marcus 1986; Tedlock and Mannheim 1995). The second strategy addresses those other power asymmetries which potentially add to the inherent inequality of fieldwork relationships. These approaches challenge the positional constellations of who conducts research about/ among/with whom. On an individual level, this strategy can lead to the decision to “study up” instead of “studying down” (see Gable 2014), to conduct anthropology “at home,” or to base one’s study on shared positionalities, such as women studying women. On a structural level, such changes require diversifying the positionalities of anthropologists and decentering the role of “Western” academia (e.g., Escobar and Ribeiro 2006). As yet a third strategy, anthropologists adopt compensatory approaches to balance the inequality between researcher and researched. This strategy corresponds most closely with the economic anthropological notion of reciprocity in its narrow sense, because it is based on the principle of giving, receiving, and giving back (Mauss 1990). Ways of “giving back” can vary from small personal gestures to advocacy work committed to macro-political change. Apart from the scope and scale of giving, researchers also must determine to whom exactly they want to “give back,” on the basis of what principles, within what time frame, and in what form (Gupta and Kelly 2014, p. 8). Should they compensate individuals or whole communities? Should they allocate resources according to the level of contribution to the research, based on the relative effort undertaken by the respective interlocutor, or simply according to need? Researchers can opt for instantaneous ways of “giving back,” or reciprocate at a certain temporal distance that might extend beyond the fieldwork period and may even lead to the developing of

³Asad (1973) and Said (1978) have provided seminal contributions to the postcolonial critiques of the anthropological discipline; on decolonizing methodologies, see Smith (1999); for discussions on feminist ethnography, see Visweswaran (1994) or Skeggs (2001).

long-term exchanges. “Giving back” can manifest itself in material and non-material form, as emotional or practical support, as recognition, information, intercession, advocacy, labor, money, or goods.

The authors in this section were invited to take their emotional experience as an analytical starting point for reflecting on the practical implementation of different forms of reciprocity in research relationships. The examples presented in the following contributions range from stationary community-based fieldwork (von Vacano) to transnational multi-sited ethnography (Siegl). They further include approaches of “studying up” (Perujo) and collaborative research (Lücking).

The chapters highlight different forms of “giving back.” Veronica Siegl and Emilia Perujo reflect on various immaterial ways to reciprocate the time, effort, and openness of their interlocutors. Both argue that ethnographic conversation itself can be mutually beneficial, because interview partners can find comfort in sharing their feelings and intimate experience with a patient, non-judgmental, and empathic listener. Ethnographers can further provide interlocutors with valuable technical information or convey the experiences of other interlocutors facing similar problems. On a representational level, research can raise public awareness and (de)legitimate certain issues and positions, because ethnographic writing can make the voices of those affected heard and communicate their experience in relatable terms. Mirjam Lücking highlights that research relationships can also include mutually beneficial forms of intellectual exchange, especially if collaborating with local research partners equally trained in anthropology. Complementing these immaterial forms, Mirjam Lücking and Mechthild von Vacano discuss tangible, material ways of “giving back.” Their contributions include a re-evaluation of their live-in arrangements during fieldwork, yet exemplify two fundamentally different fieldwork trajectories. While Lücking moved multiple times, staying at each household for shorter periods of time, her live-in arrangements were mostly based on a non-monetary principle of hospitality. Von Vacano, on the other hand, details the explicit financial agreements she had reached with the family that hosted her for the entire year of her fieldwork. Having conducted their fieldwork in a research constellation of (stark) economic disparity, both underscore the importance of the material dimension of reciprocity. Instead of suggesting one particular formula, they encourage researchers to be active and creative, when searching for context-sensitive material ways to give back or to share resources. This implies that it is not the monetary form per se that renders a gift impersonal or inappropriate; much depends on the content, form, and timing of these contributions, in terms of whether they can be accepted without straining the relationship.

Reassessing distinct features of their fieldwork experience, each author scrutinizes specific facets of establishing reciprocal research relationships: Veronica Siegl’s contribution focuses on mismatched expectations. She reconstructs the dynamics of a research relationship that culminated in an emotional accusation of betrayal, and retraces how her well-intended actions could have evoked such a deep feeling of disappointment in one of her research participants. Based on her multi-sited ethnography on surrogacy between Switzerland and Russia, Siegl’s contribution alludes to the challenge of equally including multiple stakeholders with

diverging interests into an ethnographic study. While trying to establish rapport with all sides involved, researchers walk the fine line between empathic listening (sometimes performed), sympathy, and (un)intended gestures of partiality. Siegl concludes that some conflicts might be inevitable, because researchers can neither fully control the course of fieldwork, nor regulate the expectations interlocutors project into them. Researchers can, however, minimize the risk of mismatched expectations by being clear and transparent about their own motives. This may require the ethnographer to accept that his/her own partiality might enhance the potential for rapport with some interlocutors, while limiting it with others.

Looking retrospectively at four different, but thematically close research projects, Emilia Perujo pursues the question of why any stranger would agree to participate in ethnographic research on taboo topics like (male) infertility, sperm donations, or custody for divorced fathers. What did her interview partners gain from the research relationship in return for the time and effort they took to meet, share intimate narratives, and expose their emotional vulnerability? Perujo conducted her studies in Mexico City as “anthropology at home,” where she found herself socio-economically in a research situation of “studying up,” leading her to preclude material forms of compensation and focus on the exchange of intangible gifts instead. Her contribution highlights the paradoxical effects taboo-afflicted research topics may have on research relationships. Once the researcher has succeeded in gaining access, conversations on taboo topics can create particular social and affective bonds. Once her interview partners had overcome internalized feeling of shame and taken the emotional risk of sharing, they felt relieved to finally have someone to talk to and who would listen with empathy and free of judgment, even if—or because—this person was a random outsider. Perujo herself became an important confidant for many of her interlocutors and began carrying the weight of their silent suffering.

Mirjam Lücking’s chapter reflects on her multi-sited ethnographic research, a study of the images of the “Arab World” as perceived by Indonesian pilgrims and labor migrants. Distinguishing between the material, emotional, and intellectual dimensions of reciprocity, she discusses the emotional quality of material gifts. Monetary gifts can either appear as a pay-off, or a symbol of sympathy for the interlocutor’s specific living conditions. She illustrates that they can either close off or enhance the personal character of ethnographic research relationship. Like Siegl, Lücking raises the issue of rather unpleasant interaction partners. She describes how some encounters with ultra-conservative Muslim leaders left her with a complicated mixture of feelings including insult, annoyance, and gratitude. Such unpleasant interactions indicate the limits of reciprocity, while obstructing the potential for emotional and intellectual exchange. Concluding her paper, Lücking advocates for intellectual exchange with “local” academic peers as an epistemological strategy to diversify perspectives and decolonize processes of knowledge production, while simultaneously acknowledging these relationships for their practical and emotional support.

Mechthild von Vacano’s contribution reflects on her fieldwork in a lower to lower-middle class neighborhood in Jakarta, Indonesia, for a study on the subjective experience of work. Noting the fact that most anthropologists work in research

constellations of significant economic inequality, she criticizes the lack of ethical and methodological frameworks to account for this inequality, and for the economic dimension of fieldwork more broadly. By revisiting the economic anthropological discussions of reciprocity, von Vacano shows how ethical notions of reciprocity still carry an idealistic, anti-market bias which feeds into the de-economization of research relationships. As a principle of exchange, reciprocity further presupposes equality by implying a logic of equivalence and balance, while failing to address the uneven distribution of resources. As an alternative, von Vacano develops a research ethic of “economic participation” which is based on the active and conscious integration of the fieldworker into the social context she studies. Underscoring the embedded nature of fieldwork, this framework requires anthropologists to acknowledge economic inequality and challenges them to negotiate the different abilities and needs among everyone involved. The chapter concludes with several practical suggestions on how such a process of negotiation could be realized, ranging from budgeting to expectation management and an anthropological perspective on soliciting strategies.

The contributions in this section emphasize the social, economic, and affective dynamics of research relationships, and how they are negotiated—despite and under the condition of the inherent researcher–researched inequality and other macro-structural power asymmetries. As with reflection on structural privilege in general, well-intended self-criticism can, however, run the danger of absolutizing the very power relation it intends to challenge. Reducing research identities to one dominant dimension (e.g., the researcher as a “Westerner”) is one common pitfall. In contrast to such an oversimplification of positionalities, an intersectional perspective is required to account for multiple and contradictory power positions (see chapter “[Uneasy Thankfulness and the Dilemma of Balancing Partiality in Surrogacy Research](#)”). Furthermore, moralized feelings of guilt often find their expression in fieldworkers’ ambitions to performatively eradicate structural inequalities on an individual level instead of negotiating them self-consciously. When ethnographers absolutize the inherent inequality of research relationships, this can result in an overemphasis of their actual power, agency, and significance to host communities. After all, ethnographers depend on their counterparts in the research relationship to grant them access to their social worlds, and to share their insight and experience; if the researcher’s power position is misconstrued as absolute, their interlocutors are denied the agency to shape their interaction with the researcher. Only because ethnographers engage in research relationships with a particular intention and the overarching objective to pursue the “academic-career values” (Sanjek 2015), this does not preclude their interlocutors from pursuing their own agendas—potentially instrumentalizing the researcher for their own purposes. Negotiating research relationships includes the acknowledgment of interlocutors’ expectations. Some interlocutors may utter specific requests, while others might only hint implicitly at their expectations. Sometimes these expectations exceed the researcher’s possibilities. To handle such instances responsibly, ethnographers must be self-aware and transparent regarding their own limits, just as they should about their intentions. By being transparent, anticipating, and accepting potential sources of disappointment,

researchers can prevent some conflicts from emerging. At the same time, it is helpful to regard conflicts as a “normal” feature of research relationships, just as they are of any other dynamic relationship. According to this understanding, interlocutors voicing their disagreement or discomfort indicate that the research relationship is being negotiated on open terms. Instead of obviating any sign of disharmony, ethnographers might give their interlocutors space to—implicitly or explicitly—express unease or disappointment. With regard to managing their own expectations, ethnographers might also find it helpful to accept temporary avoidance or refusal as potential trajectories in any research relationship. For ethical responsibility can be measured not only by the outcome, but also by the process of negotiating reciprocity in research relationships.

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Uneasy Thankfulness and the Dilemma of Balancing Partiality in Surrogacy Research



Veronika Siegl

Veronika, I have been reading what you wrote about surrogacy. I must say I am really disappointed. Your interpretation of the facts is very very very biased and it does not correspond to reality. I wasted my time with you. You were very unfair in your comments, and I am really hurt. I hope you never have fertility issues, so you don't need to use commoditized eggs, sperms, embryos or anything.

I was sitting in my Moscow apartment, writing down field notes late in the evening, when these words suddenly appeared in the Skype window of my computer. It was Alex, one of the intended fathers I had interviewed in the course of my research on transnational surrogacy. My heart began to race. I stared at the screen. What had I done? “You don't understand why I am mad? Are you F*** serious?” were the next lines on my computer screen. No, I did not understand, at least not fully.

Alex and I had met on an internet platform for intended parents and surrogates, where he had stated that he was in a gay relationship, looking for a woman in Europe who would carry his children. He immediately agreed to share his experiences of surrogacy with me. But it was not until a few months after our interview that Alex must have Googled me and consequently came across a description of my PhD project. The short abstract stated that the “Intimate encounters of prospective parents and donors/surrogates are not only marked by unequal power structures but also by a state of precariousness. Both parties walk a fine line between coercion and free choice in following their desires and needs. (...) The frequency of the gift-metaphor—often contrasted with issues of commercialization and commodification—and the humanitarian call for (global) female solidarity hint at the necessity

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of moral justification for engaging in assisted reproductive technologies.” It was this text that made Alex contact me again that night.

With our interview in mind, I realize that terms such as “commercialization” and “commodification” could be provocative to someone who strongly wishes to find an altruistic surrogate and make the process of surrogacy as “beautiful” as possible, as Alex had told me. And yet I was struggling to comprehend how the wording of my abstract could lead to such an aggressive outburst. I had chosen my words carefully, so that the text would reflect my research interest: to include perspectives from different actors and to complicate the dualistic picture of rich, consumerist intended parents and the poor, defenseless surrogates, by taking vulnerabilities on both sides as a starting point. When I explained this aim to Alex in my response, he answered that my endeavor had failed. Rather, my text was “simply insulting” and made him feel “terribly sad.” In addition to the short project description, he had also found the abstract to my first and, at the time, only article on surrogacy (Siegl 2015). Alex copy-pasted one of its sentences into the chat window: “Surrogacy has often been discussed as the ultimate form of commodification processes, that position surrogate mothers as the weakest links in global reproduction chains.” Followed by his words: “Do you know who feels like the weakest link? (...) I feel like trash (...) I feel like nobody fucking cares about us [i.e., gay men who want to become fathers] (...) And I feel like I wasted my time and efforts.”

Alex’s words hit me hard, for it felt like I had disappointed someone who had put trust in me. I could not give back what he had obviously been anticipating in return for sharing his story with me. In this chapter, I want to explore the affective dimensions of fieldwork by reflecting on the conflict between us. I will engage with the question of how we, as researchers, can attend to expectations of reciprocity, some of which might be unspoken and possibly unconscious, and some of which might not be in line with our own plans, wishes, or convictions. Furthermore, I seek to understand how these expectations are nurtured and shaped by the complex power relations inherent in our fields of study and in what ways this influences how we approach or present ourselves to (potential) research participants.

Being Biased

Gay men encounter a wide range of legal, social, and biological restrictions when trying to achieve parenthood. Alex had experienced these restrictions when confronted with the difficulties of adoption for homosexual couples and the prohibition on surrogacy in his home country. “If you have a uterus, you can do whatever you want,” he observed gloomily. And while any heterosexual man could be “a father for free” (i.e., without having to pay for reproductive procedures), he was denied the right to fatherhood. “We do not have access to the same reproductive rights as the rest of the citizens,” he wrote to me that evening. “We are constantly subject to discrimination. Write something about that.”

Up to now, I have not written anything about “that.” Alex’s perspectives will form part of my thesis but they will not—as he had hoped—become the central

strand of concern, and I will not become a mouthpiece for his agenda. This is for a number of reasons, connected to issues of situatedness and partiality as well as simply to the (pragmatic) prioritizing of some aspects over others. Even though it is my aim to capture diverse positions on surrogacy and to understand how the different people involved made sense of their thinking and acting, I was always aware that I could not play the “God-trick” (Haraway 1988): neither could I capture *all* aspects or represent *all* perspectives, nor could I pretend to look at the world from an elevated perspective in order to produce what some would call “objective” knowledge. Within many of the social sciences and humanities, recent decades have witnessed the deconstruction of the “objective researcher” and the notion of an objective and singular truth. Feminist and postcolonial interventions, post-structural theories, the crisis of representation and many other critiques have contributed to an understanding of knowledge as “situated,” to use another of Donna Haraway’s (ibid.) well-known expressions. The “situatedness” of knowledge is related to the situatedness—or “positionality”—of the researcher. It shapes her understanding of particular events and phenomena as well as the aspects that are important to her within a specific research area. The fact that I was a female, young, white, upper-middle-class PhD student from Austria, participating in a well-funded Swiss research project, shaped my research in more ways than I could possibly list here, let alone fully grasp myself. And so did the circumstance that I have no children and that I have not (yet?) been affected by fertility problems. In addition, my politicization and involvement in (queer-)feminist contexts directed the choices I made along the way—choices about methods, approaches, and theories, but also, most importantly, about whose perspectives I was most interested in in my research. It is here that situatedness and partiality meet. While the former is seldom something we can choose, the latter refers to a stand we actively take. Nevertheless both are tightly interwoven, for our situatedness unarguably influences our partiality. While there is a tension between partiality and the aim of taking seriously and giving space to different perspectives on a specific topic, there is no contradiction. For, as Armbruster (2008) points out, our knowledge is always partial in a double sense—in that, it can never be complete and we can never be equally balanced on all sides.

I felt more partial toward the women who worked as surrogates, in the sense that it was primarily an interest in their experiences and lifeworlds that had initially drawn me to the topic of surrogacy. Consequently, my first article—the one Alex referred to—was mainly concerned with their perspectives on the surrogacy process. The choice of my research locations further led to unintended consequences concerning the diversity of my interview partners. Due to the legal frameworks in Russia and Ukraine¹ (the second “country of destination” I researched), almost all

¹In Russia, only heterosexual couples (regardless of marital status) and single women have the right to surrogacy, and in Ukraine only married heterosexual couples. Additionally, a so-called medical indication is required, meaning that women need to provide proof of their infertility or of prior miscarriages. As such, it is argued that men have no right to surrogacy because they suffer from so-called social, not physical, infertility. Nevertheless, there are cases of single men and gay couples using surrogacy in Russia, but the great majority of cases involve heterosexual couples.

of the intended parents I spoke with were in heterosexual relationships. Not surprisingly, Alex had “searched the entire website” of the research project, without seeing anything written about “the discrimination we [i.e., gay men] are suffering because we are denied access to public health.” If this is what Alex meant by saying I was “biased,” then, yes, he was right. But what exactly was it that had made Alex assume I would prioritize his suffering?

The Benefits and Burdens of Reciprocity

As researchers—but also as human beings per se—we are not only situated but also relational. We are never just ourselves—we come into being through our interactions with others and these others perceive us from their own positions and belongings, projecting their wishes, desires, and expectations onto us, each having their own (sometimes hidden) agendas. In ethnographic research, the dynamics between researcher and researched are particularly fragile because we seek an understanding that can only be achieved by forging bonds and building relationships of trust. A process that takes not only time and patience but that might also entail having to leave the position of a researcher and to share information about our own lives. And yet, at some point, we have to withdraw, regain distance, and critically evaluate what we have experienced—leading to an analysis that might not be in line with what our research participants hoped for or expected us to conclude. Contact with Alex ended before it had the chance to develop into such a long-term relationship, but similar dynamics were nonetheless at stake, for the field of surrogacy is an especially delicate research site. The particular entanglement of intimacy, secrecy, and power that it entails triggers heated debates in society about commercialization, exploitation, and moral decay. These factors make questions of access and power challenging, demanding a constant balancing of interests and expectations.

Accounts of fieldwork and self-reflective writing in the field of qualitative research are full of discussions surrounding the problematic of power and the connected issue of reciprocity (Duncombe and Jessop 2012; Luff 1999). As anthropologists, we collect local and personal knowledge in order to then take it back into academia, build our careers, and become experts in these fields of knowledge. Many have criticized the often “extractive” nature of doing research (Smith 2012) and have called for practices of reciprocity (Sudbury and Okazawa-Rey 2009). However, it is never just the power axis of researcher–researched that is relevant. Being situated means that power relations are not static. Considering the intersection of different positions and belongings in a specific context (gender, class, race, nationality, age, dis/ability, and many others) reveals that there are always aspects and experiences that connect us to and others that differentiate us from our research participants—we are insiders and outsiders at the same time (Hsiung 1996; Narayan 1993; Riley et al. 2003; Thapar-Björkert and Henry 2004; Wolf 1993). As such, it might not always be helpful to seek out “hierarchies of oppression.” What makes the

power situation even messier is that, as researchers, we are highly dependent on our research participants. We have to appear likable and trustworthy, in order to get others interested in what we are doing and to convey what they can gain through participation in our study. After all, most people do not take part in our research merely out of benevolence or friendliness. They are not necessarily strategically calculating individuals, but their stories and their time are nonetheless “gifts” that come with expectations of reciprocity (Mauss 1966). Some of these are explicit, others subtle; some we will perceive as alleviating and enabling, while others might feel cumbersome and restraining. At least these were my experiences during fieldwork.

I found myself in many situations in which I latched onto any possibility of “giving back.” This was often the case in my interactions with surrogates. Many were interested in receiving information from my side about the surrogacy process or about different clinics and agencies. For other surrogates, it was more the psychological need of having someone listen to their daily struggles, while yet others were merely curious about talking to a foreigner and learning about my personal life. I felt more at ease with my role as researcher when being able to give something back. There were other situations, however, in which I perceived expectations of reciprocity as a burden or obstacle. This feeling was particularly prominent in my interactions with staff in private fertility clinics and agencies. The question “What’s in it [*sic*] for us?” often turned out to be a central condition for participating in my research, as these actors were interested in building up a reputation and making Russia and Ukraine a better-known destination for surrogacy. Some of the intended parents, again, were hoping that my work would contribute to the social acceptance of surrogacy.

When I reread the initial e-mail I had sent to Alex and other intended parents I was hoping to interview, I realized the misunderstandings my words could and obviously did entail. Given the sensitivity of the topic, I had worried about not finding enough interview partners. My e-mails therefore left out certain pieces of information, while remaining vague about others. I was reluctant to openly position my thesis at the intersection of social anthropology *and* gender/feminist studies; I feared that this intersection could automatically be read as an anti-surrogacy stance and, therefore, as a threat. I was also hesitant to mention my interest in morality and ethics; I was afraid that interlocutors could ask me about my own moral stance toward surrogacy, expecting me to have a clear opinion and write my thesis around it. But I did not have a clear standpoint and I was not interested in writing a manifesto, neither for nor against surrogacy. For these reasons I decided to keep my questions as open as possible, particularly at the beginning of my fieldwork. This practice was also a matter of methodology, for while it is important to have a theoretically informed research question, I wanted to create space for my interlocutors to formulate aspects that were relevant to them. In my e-mails to potential interview partners I merely stated that “I would like to know more about how people [in this case intended parents] make their decisions in an area that is marked by seemingly endless options and possibilities on the one hand, and constraints and boundaries on the other.” I also stressed their potential benefit from my research, as I was hoping to “contribute to a better understanding” of surrogacy by “capturing diverse experi-

ences and perspectives on the topic,” hopefully making the debate less heated and more informed. Both these things were true, but the way I formulated my phrases might have suggested that I was interested in actively supporting surrogacy and its legalization. I suppose it is here that I have to seek the roots of Alex’s pain and disappointment.

Rapport and the Drawbacks of Empathic Listening

The simultaneousness of power and powerlessness we experience as researchers is unsettling and confusing and can make us act in ways we find problematic and unethical. Fieldwork accounts are full of confessions about not being “yourself.” Diane Wolf (1993), who composed one of the early anthologies on feminist fieldwork, wrote about her inner struggles when doing research on the intimate lives of women, while feeling compelled to lie about her religious orientation and marital status in order to gain access to the research site: “I lied about the same topics about which I hoped for honesty from my respondents. This particular representation of myself made me feel dishonest and uncomfortable, but I did not see another way out” (ibid., p. 2). Psychologist Sarah Riley (Riley et al. 2003) experienced similar discomfort when working on gender relations from the perspective of “professional men.” She recounts interview situations in which she was confronted with opinions that she not only *not* shared but that also went against her own. Riley silenced herself, as she puts it, because she knew she needed these interviews but simultaneously she experienced an internal struggle because “In not arguing back, I was producing a false self. (...) In being false, I was now also manipulative” (ibid.).

My own experiences resonated with these accounts. I often felt the need to display a “me” that was dressed in a fairly discreet and unobtrusive way—sometimes in a more “adult” way, in order to be taken seriously, at other times in a “youthful” way, in order to appear less threatening, depending on the kind of agency people attributed to me. Similarly, I would sometimes display a “me” that was knowing and informed, while at other times I was a naïve “me” (see also Thapar-Björkert and Henry 2004). And frequently, I was a “me” that did not challenge an opinion but merely nodded and listened. After all, as social scientists, we are often taught to influence the interview situation as little as possible with our own way of being or our own opinions. Adopting this role troubled me less when doing interviews with professionals (doctors, agents, lawyers, etc.), but it became more problematic when I engaged with my research participants in more intimate ways, asking them to share their personal stories with me. I wanted to provide a safe space in the interview, a space in which they did not experience judgment. Given the sensitivity of the topic, I would have felt like I was betraying their trust had I interfered or commented on their stories and decisions in a critical manner. I saw my role as one of an empathic listener, putting aside my own viewpoints and feelings, in order to see and feel from the perspective of my interlocutors. These are ways of “doing rapport,” as anthropologists often phrase it, ways of establishing relationships. Some of these

are conscious, deliberate, and strategic; others are unconscious and unintentional. Through doing rapport, we engage in what Hochschild (2012; see also Duncombe and Jessop 2012) has termed “emotion work” and “emotional labor.” The former refers to how we manage emotions within ourselves in order to align them with the “feeling rules” of doing research, of conducting interviews. The latter concerns the way we do or do not display our emotions and how this might affect our research.

It is exactly this kind of emotion work and emotional labor I experienced in the interview with Alex, during which issues of power and empathy merged into a feeling of being trapped. While there were several factors that made me, at least from his perspective, more “powerful” (me being the researcher, having a uterus, not having fertility problems, not being gay), Alex was clearly addressing me from a superior place. He adopted an educative tone with me, the young woman who wanted to learn from him. I felt cornered by the way he positioned me as an ally and by the way he was constantly seeking my approval by putting a “Right?” or a “You know what I mean?” at the end of a sentence. I got angry about his derisive remarks about “the feminists”—meaning those who were not on his side, who “still didn’t understand” that surrogacy was something “very beautiful.” I often felt the urge to interfere, to complicate the picture of *the* feminist. But in the way Alex was speaking there seemed to be no space for ambivalence and contradiction. Being an active supporter of the legalization of surrogacy, he seemed to perceive others as either for or against his cause. Some of his arguments were convincing, but I certainly did not agree with all of his opinions, let alone the way he phrased them. I felt the urge to free myself from this verbal corset as quickly as possible, and yet I could not help but stay in the role of the empathic listener, encouraging him to keep talking, and laughing at his sarcasm. This was not because I saw this perseverance as my professional obligation but rather because—despite my unease—I liked him and I appreciated his making time for me, even answering further questions via e-mail in the days following our interview. Did my way of interacting foster his assumption that I would be on his side? There is a fine line between empathy and sympathy or support. Can we and others always tell the difference? I never got to ask Alex these questions. As he did not want to give me any further explanations that night on Skype, I suggested we talk again at another time. “Now I feel like saying no but I know myself and I know I will,” he answered, but neither of us made the first step for over a year. It was only when I came across the story of a woman who had faked an entire pregnancy to trick the intended parents that I wrote to Alex. The personal background information of the surrogate matched with what he had told me about a woman he had been discussing an arrangement with. I was ambivalent about what to do. I was worried about reigniting our dispute while simultaneously feeling obliged to pass this information on to him. I followed my sense of responsibility and, as it turned out, it was indeed the very same woman. However, Alex and his partner had themselves stumbled upon reports of the scam on the internet and had broken off relations with the potential surrogate. On receiving this answer, I could not help noticing that I felt a bit disappointed. I must have hoped that I could bring peace to our relationship by “rescuing” him from a potentially traumatic experience. But even though he did not need rescuing, contacting him again alleviated the

paralyzing feeling of uneasy thankfulness I had felt for so long. The situation made me realize that, in addition to reflecting on the topic of reciprocity as an issue of ethical and political importance, it would be fruitful to include in the analysis the psychological or emotional importance reciprocity can have for the researcher. Reciprocating, for the researcher, can often function to relieve the researcher—in positive as well as in potentially problematic ways—with regard to the extractive nature of research, on the one hand, and of thankfulness or indebtedness on the other.

A Call for Transparency and Confrontation

Reading and listening to post-fieldwork accounts as those produced by Diane Wolf (Wolf 1993) or Sarah Riley (Riley et al. 2003), I wonder whether the people confessing their “mistakes” would really act differently if they could relive the particular situations they describe. It is so much easier to *say* what we would change in our next project than to actually *do* it. Many dilemmas result from the special kinds of relationships ethnographic research enables and are to a certain degree insoluble (Stacey 1988). Fieldwork is such a challenging endeavor precisely because it has so much to do with who we are and who we become in the very specific interactions with our research participants. What are the chances that I might once more find myself in a similar conflict as the one with Alex? I do not know, and it would be presumptuous to deny the possibility of such a situation repeating itself. Nevertheless, my experiences with Alex certainly left their mark and have affected the way I have approached research participants ever since. Realizing that my partiality could lead to unintended problems, my reaction has been to be much more transparent about my research goals and to refrain from seeking close relations with intended parents. I gave up on my wish to accompany them in their transnational endeavors and decided to stick to interviews instead. This change of mind was challenged when I was contacted by a couple that wanted to start a surrogacy program in Ukraine. I was hesitant about getting involved with them, so I was happy for our interaction to have a contractual character. We had an unspoken and yet fairly clear deal that left little room for further expectations or claims. I provided them with information, and they provided me with their story. I explained to them in detail how I approach anthropological research and writing and which research questions guided my study. Over the course of our many months of intensive communication, more personal bonds developed, and a year after we met, I accompanied them to Ukraine to pick up their new-born twins. Despite our close relationship, they never seem to forget that I am a researcher, and I consciously remind them of this now and again.

There are obviously no universal rules for “correct” conduct during fieldwork, but nevertheless I would like to make an argument for paying closer attention to our own feelings as researchers. If we feel uneasy, there is a reason for this, and rather than ignoring or suppressing the feeling, we should confront it. We must seek out the root cause of our unease and query why it appears at certain moments and not

others. Being attentive to the intersection of different experiences and belongings can help us understand how these factors can lead to “misunderstanding, disappointment, and broken trust” (Kirsch 2005, p. 2170) on both sides. Such attentiveness includes being self-reflexive and self-critical as a researcher, while also acknowledging that this stance should not end in a diligent search of who did what wrong. Conflicts are part of every ethnographic undertaking. They arise from an interaction between two or more people, who perceive each other from their own perspectives, shaped by situatedness and partiality, be it as researcher or researched. All of these aspects could help form a solid basis for making deliberate choices about the relationship between the duty of transparency and the need for material—in the interests of our research partners but also in our own interests, because fieldwork encounters can be intense and last for a long time. We must not grab hold of any opportunity that arises but should select who we want to work with and, hence, toward whom we want to feel indebted and thankful. An important aspect of my contact with the couple mentioned above is the fact that they are open to critical questions and comments from my side. In many regards their worldview is different from mine, but there is a mutual space of encounter that Alex never allowed for in our interaction. Alex was merely concerned with getting his message across. I sensed that there might be a misunderstanding between us and yet I did not address this issue during our interview. However, rather than shying away from potential conflict we must risk confrontation. This can entail being as transparent as possible about our research interest and our way of working, or even asking directly about the expectations our interlocutors have when they agree to an interview. We need to be careful with the pressures of academic knowledge production and maintain a dialog with our own feelings. They can tell us much about our own and other people’s unspoken assumptions and implicit promises, and they can help us avoid at least some of the pitfalls of ethnographic research.

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Exchange of Intangible Gifts? Reflections on Research Relationships When “Studying Up”



Emilia Perujo

A PhD dissertation on acrimonious divorces was just one of several studies I conducted around the topic of adverse kinship situations in Mexico City. Doing research on such intimate issues challenged me to thoroughly reflect on research ethics: on issues of legitimacy, anonymity, and responsibility. Therefore, I thought I had already learned a lot about research interactions. But just a few months after my PhD defense I had a unique chance to switch places and become an object of inquiry myself. This experience made me think about the feelings, possible motives, and social position of subjects as they engage in the research process.

At that time, I had volunteered to participate in a documentary film, which was inspired by a reality TV format: For two days, a camera and two set directors followed three people in their daily routines. The team filmed me brushing my teeth, getting ready, typing, making music, meeting with my friends, and having formal conversations on the phone. Intermittently, during the shooting, the director would insert a few interview questions about my day-to-day activities, my favorite music, and my friendships. To me as a researcher, this was an eye-opening experience: I found it extremely uncomfortable to be watched by strangers while performing my routine activities, and I did not like to be interviewed about any aspect of my life, even the most superficial. After years of studying other people’s lives, their intimate kinship practices and relations, I found myself to be on the other side of interviews and the observing gaze. This gave me an insight into an issue I had not explored earlier, but which altered my future anthropological inquiries.

I found it difficult to understand why anyone would agree to participate in an anthropological research project. I became uncomfortable with the idea that those participating in my research were not being explicitly compensated for their time and the life stories they shared, for being observed and questioned, and for the

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intrusion into their private spheres. I decided to take a step back and analyze why—in my previous research projects—people had volunteered to share their personal accounts. If not for financial compensation, why would anyone want to talk to me about their menstrual cycles, feelings of loneliness and sadness, or their hopes, fears, and plans for the future? Why would they let me into their lives, even if I did not contact them to offer help—regardless of whether I would turn out to be helpful? What do research participants who engage in our studies possibly gain from our interactions?

In order to examine forms of mutual exchange in research relationships, it might be helpful to refer to one of anthropology's key concepts: reciprocity, that is, the principle of mutual exchange, an act of giving, receiving, and again giving back the same amount of something received. Reciprocity is involved in almost every social interaction. Anthropologists have observed and discussed reciprocity as a core principle of the communities and social relations we study (Malinowski 1922/1978; Mauss 1925/2002), but the question of reciprocity in research relations, the social relations between the researcher and her interlocutors, has long been overlooked or ignored (Spencer 2010). This void is remarkable since, after all, anthropology is a deeply relational science: We study relations—between people and things, people and institutions, ideas or places, and people and other people. And we study these by relating: We immerse ourselves in various social contexts by relating to things, institutions, ideas, rituals, places, and, above all, people.

I understand research relationships as a particular type of social relation based on exchange. The act of giving establishes a social relationship, because it requires that the gift (what is given) be received and reciprocated. Marcel Mauss (1925/2002) highlighted the fact that the gift is always more than an object being transacted; rather, the act of giving establishes “complex social relations” (Tober 2001). For an exchange relation to be reciprocal, both participants have to give and receive something of equal value. However, the gifts exchanged do not have to be material objects: rituals, festivals, or respect, can all be considered gifts (Mauss 1925/2002). The value of the gift has no “objective” measure; it is the imagined value (Tober 2001) that those involved in the exchange have to consider equal. Based on this evaluation, they decide to give a gift or accept it, to enter into a complex social relationship, to remain party to it, or to end it.

But how does this principle apply to research relationships? Researchers pursue a clear goal when entering a research relationship. We know why we want to contact certain people, and we seek to benefit from their knowledge, their experience, opinions, and beliefs. We garner information or data, but this presupposes that our research counterparts would spend or invest their time to speak to us, share their contacts, or mobilize the energy to talk about potentially difficult topics. Our potential gains and motives are clear, but what about the other side of this research relationship? Usually, we do not pay for our interviews, and the research relationship seldom involves any other material form of exchange, for example, useful or informative material from our side, or our help in a concrete task. Usually, our research relationships are based in immaterial forms of exchange, and are therefore difficult to grasp; the gifts we give and receive are intangible. For reciprocity to exist, both

sides of the interaction consensually enter into and remain within a complex relationship, one based on mutual obligation. To be able to create reciprocal research relationships, it is important to understand why people agree to participate in our research, and what kind of gifts we can offer in return. This chapter is the result of my reflexive process of reevaluating the (possible) motives research participants have, and the transactional dynamics between the researcher and her counterparts/partners/interlocutors. It is based not only on rereading and reanalyzing my own field notes and interview transcripts, but also on the direct answers provided by participants in my most recent fieldwork.

In the following sections, I will begin by briefly characterizing the four fieldwork experiences on which my reflections here are based. I will then trace the different phases of fieldwork and illustrate how research relationships begin, unfold, and end, while questions of giving and receiving are being constantly negotiated. I will show how the content and context of these gifts affect field interactions, and how they relate to our emotional understandings of topics and situations.

Introduction to the Fieldwork Contexts

Between 2008 and 2016, my research interests have moved around contemporary kinship situations in my hometown, Mexico City—infertile couples availing themselves of assisted reproduction technologies (ART) to become parents; male infertility and its treatment; fatherhood after divorce; sperm donations mediated by Internet platforms. Each of these research topics were motivated by futuristic ideas and questions about kinship; actors immersed on these specific topics were key to understanding legal reforms, reproduction without sex, advanced technology, the existence or impossibilities of “new” masculinities, and choice. People who decided to take part in research were people with access to innovative technologies or legal mechanisms surrounding kinship relations.

With the exception of my last research project on sperm donation, all research was conducted among people of a higher class, age, and in most cases, with participants of the opposite gender (see Arendell 1997). All fieldwork experiences began with me meeting people who were older, well educated, wealthy, and had successful careers; most of the men and working women I met were directors, heads of departments or business owners. Almost every aspect of social privilege that could impact research regarding power imbalances was in their favor. Compared to my own positioning, these research participants were situated on the favored side of the power balance, a rather atypical constellation in anthropological research for many decades (see Nader 1972). My research projects entailed the challenge of “studying up” and therefore provided me with a particular setting which enabled me to reflect on research ethics and reciprocity in research relationships (see Cassells and Jacobs 1987). But while my research counterparts were “up” in socioeconomic terms and had access to the technology or lawyers, only a small minority of the general Mexican population could even imagine that their situation was characterized by

some sort of suffering and experience of failure. They were people who used ART and who had lost pregnancies, babies, fortunes, and marital statuses trying to reproduce; they were divorced fathers impeded from seeing or talking to their children, who had lost all the custody battles; they were donors unable to find receivers they would like to give their semen to. These research topics were characterized by sadness, anger, secrecy, and personal tragedy.

Entering the Field: Initiating Contacts

During the study on infertility and ART, one particular medical doctor served as gatekeeper and was determinant for the success of my research. A gynecologist, he put me in touch with different couples facing difficulties conceiving. Only later, when I discussed the patient–doctor relationship with my interviewees, did I realize how much our initial contact had depended on the close relationship they had with their doctor and their high degree of satisfaction with his work. Thankfully praising her doctor, M. described her trust in him and its importance in achieving her life project: “I only feel comfortable with him, I think I never would have become pregnant in Puerto Rico, because I didn’t like my gynecologist there, we were not close.” Because his patients trusted him, the doctor’s recommendation helped me establish research relationships (see Hammersley and Atkinson 1993). He introduced me as a friend doing academic research and asked his patients if I could contact them. After this introduction, my research faced no major obstacles in gaining access to information or interview partners. I was soon visiting mothers, pregnant women, or women receiving fertility treatment at their homes. I entered a field where a trustful relationship was already established between the medical doctor and his patients, and through this recommendation this rapport extended to my fieldwork relationships.

The initial situation for my second research, which focused exclusively on male infertility, was similar. I never could have established such close contact to male infertility patients and the topic per se if not for the help of the same medical gatekeeper. This time, I was searching for couples who were facing a male infertility diagnosis. I wanted to interview both partners together and separately, but as it turned out I could only interview men after I had sat down with their wives several times, and at the end I could only interview one man alone. In all these conversations, my interview partners never themselves mentioned male infertility as a reason for their difficulties in conceiving (see Becker 1994). This led me to reflect on matters of secrecy and on the limits of access to certain firsthand experiences. Measured by my initial intent—to meet these couples and talk about their experience of coping with a male infertility diagnosis—these meetings did not provide me with the information I was looking for. In this instance, the transference of trust did not suffice to create a situation in which the taboo topic of male infertility could be discussed. But there was also an important external factor that impeded the development of continuous and close research relationships: because of academic time constraints,

the duration of this research project was rather short, much more so than other field experiences.

Matters of secrecy and the ways men relate to reproduction grew on me, and I decided to dedicate my doctoral research to the topic of conscious and committed fatherhood. This meant investigating divorced fathers who were impeded from seeing their children after divorce and who were legally battling for their right to do so. As in my prior research projects, my field access was established by a professional, a family lawyer in this case. Again, I was introduced to these fathers by the one person who was helping them solve their cases: a lawyer they trusted with their futures, just as the couples accessing ARTs had trusted their doctor. Because of this trust they agreed to participate in my research. In this case, it was the beginning of a 1-year period of fieldwork and interview sessions.

Right after defending my PhD thesis, I was invited to join a research project where I could either continue and further the topic of fatherhood in the absence of children or elaborate a new proposal. My immediate response was a long academic justification for my refusal to continue working on the same issue. In reality, I wanted to take some time off, time to sleep, regain weight, stop problematizing gender, relationships, marriage, birth, friendships, and anthropology. But I did not want to forgo the opportunity, and therefore, decided to rediscover my curiosity for sperm donation. So I designed concrete, delineated “happy fieldwork.” This new research project focused on the transactions between sperm donors and receivers who were setup without the mediation of a fertility clinic. However, from the onset, my research simultaneously—and at the same level of importance—focused on fieldwork transactions as well. For the first time, I did not know anyone who could introduce me to the new field, any gatekeeper, or key informant to rely upon. I began researching online and found specific discussion forums on the topic. I registered with a transparent profile introducing myself as a researcher and openly searched for people willing to participate in interviews. Upon their declaration of interest, I sent all the potential participants a detailed description of the project and informed consent forms. This experience proved to me that people were willing to participate in my research project as long as I could explain everything beforehand. Interestingly, the people who agreed to take part did research on me first. Just as the Internet gives us a chance to find research subjects (actors), it gives them a chance to “research you back.” Depending on the personal Internet presence of the researcher, this can entail a lot. One potential participant even called the anthropology department and asked me to e-mail a copy of my degree so he could check my credentials; he further requested a personal confidentiality promise before telling me anything. I found this a fair way to start a research relationship. Especially after my own experience with the documentary film project had led me to doubt our disciplinary research techniques as a whole, I felt relieved that they could ask something from me since the beginning.

In many ways, direct online recruiting is different from being introduced by a gatekeeper. Perhaps the most significant difference lies in the establishment of trust. Meeting participants via a third person is very helpful, because we can build upon a preexisting sense of trust and commitment, which we then have to extend and

transform into our own. When we approach research participants online, on the other hand, they either trust us blindly or we slowly have to develop a relationship of trust. The information we retrieve in our first conversation may be less dense, but once trust is established, the relationship may even be considered more honest, since it actually evolved between the researcher and the research participant and was not transferred from another social relationship. Trust and reciprocity are built as fieldwork develops.

Starting a Research Relationship: First Personal Encounters

In my opinion, the most exciting moment of fieldwork is meeting potential research participants for the first time. As researchers, we prepare for these moments: we adjust our outer appearance and think about how to word our first questions. These initial encounters have a great impact on the dynamic of a research relationship and can be surprisingly overwhelming. I experienced such a memorable and defining moment when I first met A.C., a successful doctor and recently divorced father. I documented this meeting in my field notes as follows:

He told me he and his ex-wife gave birth to their first son, who died of cancer a few months later. Then he stared at the wall, remained silent for a couple of minutes and cried. He had not even begun to tell me the story about the children he could not see since August and I did not know what to do. Should I hand him a *Kleenex*, leave our table at the crowded cafeteria, say something, change the topic, finish the interview? (personal field notes, October 2012, translation E.P.).

We are never fully prepared to handle the emotions of research participants, even if we were used to people tearing up and crying in an interview situation. People cry for different reasons, in different settings and ways. Never had I expected a man like A.C. to cry in front of me, especially within the first few minutes of our conversation. I was unsure how to respond, nor did I feel worthy as someone to whom he could express his emotions in this manner or of the trust he seemed to place in me. After all, I did not say anything or try to finish our conversation. I just let him speak. As I learnt over the course of my fieldwork with divorced fathers, remaining silent while people cried turned out to be the most helpful and appreciative response I could give. For my silence allowed the participants to express their feelings in non-verbal terms, and my witnessing their expression legitimated their sadness without questioning it. Silence was my way to respect their pain. And looking back, this was something I was able to “give back” as a researcher: to lend the research participants my undivided attention, to sit down with them, and take the time to listen and wait.

In anthropological research, intimacy is a crucial currency, because it determines the degree of access we obtain to the experiences of the people we study. The ways to establish such intimacy depend greatly on the specific context, namely the specific topic of the inquiry as well as the personal situation of our collaborators. My first anthropological research relationship began as another extreme encounter. As a principle, I always let interviewees choose the time and place for our meetings. Ines

had chosen the hospital. At that time, Ines was about to give birth, but she was considered at risk for preeclampsia, a dangerous rise in blood pressure. She wanted me to visit her before her delivery. At first, I was doubtful about the proper way to approach the situation, but when I met Ines, she seemed scared and strangely lonely in this moment on the cusp of becoming a mother. So my presence, even as a stranger and a researcher, provided her with some calming company.

As I experienced with A.H., another divorced father, as researchers we might not be the only ones asking for something. A.H. had agreed to a first meeting, for which he suggested that we meet before a TV show on which he was going to appear. Outside the studio he told me that he had arranged for both of us to speak in front of the camera as experts on problems divorced parents face when introducing new partners to children. It was our first meeting ever, and I was quite surprised by this setup. Without much time, I decided to refuse his plan, but we still entered the studio together and he arranged for me to sit in the audience. His contribution during the TV show was short, but emotive, and we discussed it afterward in the parking lot.

All of these situations are examples of rather intense first encounters with research participants. In their intensity, their dynamics might be considered specific to a research situation, where the contact with these research participants was established through a highly trusted mediating person—a doctor or a lawyer, as explained above. But these examples show how the question of mutual benefit, of reciprocity, is negotiated from the onset of a research relationship.

Developing the Research Relationship: Going Deep

If the initial encounter between the researcher and the potential research participant succeeds, their interactions will become more frequent over the course of the fieldwork. By agreeing to meet, both partners agree to develop a research relationship. As fieldwork progresses, the researcher will seek to explore deeper concerns than those explored in the initial acclimation phase, and she might apply methodological tools that suit a more advanced stage in the relationship. In my experience, it is helpful to start with noninvasive and informal research methods before progressing to techniques like life-history interviews or in-depth questions.

By the time interactions have gained a certain level of depth, when questions become more profound and intimate, the researcher already knows a lot about her research counterpart. However, at this stage, the research participant has also gotten to know the researcher. They know us through the research project, by the way we handle certain issues and respond to particular topics; they can assess our personalities by the way we conduct the conversation itself; and they know about any personal details the researcher had decided to share at a particular point in the interaction. Because of the highly intersubjective nature of their fieldwork, anthropologists “know with the price of being known” (Cornejo et al. 2008, p. 31, translation E.P.).

At this advanced stage of the research relationship, I experienced how the “gifts” I reciprocated became tangible in a strange physical way. In return for the wealth of data my informants provided, I offered them supportive listening, empathy, and comforting company. Sharing in their often painful experiences provided crucial insight into the research topic for me, and at the same time, provided a valuable opportunity for them to release these negative emotions and rely on my supportive presence. I was there for them and I felt with them. Bound to keep my professional promise of anonymity and confidentiality, I became a container for all the negative emotions and the general atmosphere of the field. I was constantly immersed in issues of absence, anger, clinical depression, failure, and loneliness. My intense emotional involvement had real physical effects on me. I began to somatize the emotional burden as back pain and headaches; I developed insomnia and lost focus in other spheres on my life—outside of fieldwork, my social skills seemed to devolve. I found it hard to find suitable mechanisms to release this emotional burden without failing the trust of my research participants.

Another challenge I faced at this stage was that my research counterparts by this point had become aware of the existence of other research participants, and they began inquiring about the knowledge and experience I had gained from them. Such a cross transfer of experience can generate tension. I felt an obligation to share the experience I had gathered through the other participants’ accounts, but while leaving out all details and guaranteeing anonymity. However, even if I granted their explicit wish in telling them about other research participant’s experiences, I could not ignore the potentially negative effect this information had: It can be devastating for someone struggling to achieve a pregnancy, or be granted the right of access to their children, to hear other people’s stories who were sharing the same fate, who were also failing and suffering.

Even if the experience you share does not represent bad news, it may still cause distress, as the example of R. shows. R. was a young married woman who, when I met her, was trying to get pregnant with donor semen in a clinic. At one of our later meetings she asked me if I knew of any case in which a woman had become pregnant by having sexual intercourse with a donor. I honestly told her that I did, because I could not see any harm this information or my sharing it could cause. But later she declared to me via a text message that she was going to have sex with a donor, too, “because I told her it worked.” The seemingly innocent information I provided put me in an awkward position of authority, given responsibilities I never intended to carry.

This leads to another example, which shows the boundaries of what we can “give back,” even if our research counterparts explicitly request us to. During my research with divorced fathers, one night an enthusiastic G.V. called me asking if I could be his legal witness. As it turned out he was about to meet his ex-wife and daughter after one year of searching and trying to establish contact. The meeting was to take place in a public space, and G.V. wanted to have someone accompanying him, who later could testify to the fact that his ex-wife impeded him from being a father by “poisoning” his daughter’s feelings toward him. I refused, because neither did I feel prepared to observe children in such a conflict situation nor did I think it was an

ethical role to take for me as a researcher. After this call, G.V. did not answer my calls for two months; later he asked me to meet, but did not show up. I felt disappointed, but somehow relieved at the same time, because I remembered how angry he had been on the phone. With some distance, I interpreted G.V.’s behavior as a passive form of revenge, and a way to express his disappointment in my contribution to our research relationship. As such I could empathize and understand that his reaction was necessary for him to deal with the situation. I therefore never asked him about the incident, even after we later reestablished contact. Refusal and avoidance belong to the repertoire of research relationship dynamics; they are important features of how these relationships are negotiated by both sides involved.

Ending the Research Relationship: Leaving the Field

Ending fieldwork involves much more than simply no longer meeting those with whom we have established research relationships. The way to end the research relationship may be negotiated between the researcher and the research participant. Once an intimate/close research relationship has been established, and research partners have gotten used to talking to and sharing with the researcher, ending the relationship may be problematic. Ines, for example, the same woman that had asked me to meet her at the hospital, found it hard to terminate our relationship. Even after we had had our closing session, she contacted me and requested to meet again, because she just wanted to talk. I agreed, feeling somehow satisfied that I could be there for her and return the gift she had given me as a researcher. So I gave her my time and attention. A.H., the father I had accompanied into the TV studio, also wanted to have one more meeting, so we had breakfast. In our previous meetings he had refused to speak about his personal experience as a divorced father but only wanted to address the topic at a general level. I respected this decision. But for this last breakfast meeting, he had asked me to bring my voice recorder along. At the meeting, he told me his personal biography in a 2-hour interview. He apparently felt comfortable enough to share his story and did not want our research relationship to end before he could. Unexpectedly, A.H. gave me an “extra gift” in terms of data, while I lent him my ear, willing to represent his experience to my best capabilities.

Another example was M.R., who made it difficult for me to terminate our research relationship. For the last meeting, M.R. had decided to invite me to his home for dinner. He had been living with his 8-year-old son for a few months at that time. In addition to a sophisticated meal, this invitation entailed the special opportunity to meet his son—and being able to observe father and son interact. To me this invitation was a generous gift which I felt unable to return: I had come empty handed, with no more time or information to share. Looking back, I interpret M.R.’s generous invitation as motivated by his wish to proudly show off his newly established family life, to present the relationship he had finally been able to build with his son, and the home he had made. Through my research, he wanted his happy ending to become public. But maybe it was an incitement for me to continue my

research—and the research relationship with him. That evening, our conversation went as if we were going to see each other again. I actually found it hard to end my inquiry here, when he had just opened the next door to his personal experience of divorced fatherhood. But sometimes—or probably quite often—fieldwork has to end, even before all data are saturated and before the research participants are ready to let us go.

One of the most obvious ways to give back is sharing our results with the former research participants. Since this form of sharing presupposes some degree of data processing, it usually happens at a later stage in the research process, after we have left the field. In the aftermath of my first fieldwork, I edited a brief informal report anonymously summarizing the accounts of ART patients' experiences at different points of their fertility treatment. When I handed each a copy, many expressed their gratitude. They found it consoling to have their collective experience documented; I felt relieved that I was finally able to reciprocate.

Over the course of time, I learned that my presence in these women's lives had made a difference. My conversations with L., for example, had supported her decision to begin psychological therapy; other interlocutors highlighted the emotional relief they gained by having someone to talk to during this emotionally challenging period. Most of my research topics were burdensome, taboo topics, which made it hard to find people who were willing and able to open up about their experiences and share them with me. But at the same time, research conversations became a rare opportunity for the research participants to talk about these issues (Stodulka 2015). What I offered in return was empathic listening and a nonjudgmental ear; sometimes giving and taking is not so much about materiality but more a critical reflection of one's own and others' compassion.

After Fieldwork and Final Remarks

As mentioned in the opening of this chapter, my last fieldwork encounter began with the question, "Why did you agree to take part on the study?" Answers circled around the interlocutors' felt need to open up about a taboo, and the aspiration to contribute to a more adequate public understanding of what it means to donate sperm as an altruistic activity. Some of the gifts studying up can offer to social actors is to make their knowledge available for a broader audience (Nader 1972). After my research on divorce was published on a radio university station, many other divorcing men contacted me asking for "expert" information.

Fieldwork—within a broad range of forms, degrees of involvement, and immersion—means interaction, and produces emotions that are present throughout the whole process of knowledge construction. Even when emotions and affects are foregrounded, they arise from the earliest stages of our research processes—in the way we choose our topics, how we approach them, and more intensely, as we relate to others during and after fieldwork. They shape our findings, especially in the case of sentimentally charged topics and interviews. They have an impact on our field

relationships and on how researched phenomena can be approached, studied, and reported. Many of the ethical decisions I have made during interviews and other fieldwork encounters have depended on my own emotions and feelings toward a topic or situation, and to reciprocate does not always come without a risk. Emotions can also set limits or alter the dynamic of give and take between researcher and informant. If not reflected upon, emotions can stand in the way of research and our relationships with informants and interlocutors. As Spencer (2010) argues, the emotional dimension could be integrated into to research, “liberating [it] from the extraordinary, the feminine and the apolitical” as a vehicle of knowing, opening new areas for research, and establishing dialogues between anthropology and other academic disciplines.

Over the years, I have encountered many colleagues and fellow students who had a difficult time achieving what they wanted in research terms, or who were constantly feeling that they had failed and suffered during fieldwork when studying up. Yet sometimes working their way through conceit, skepticism, and suspicion, socially and economically better-off research participants who are caught in dilemmas arising from cultural taboos, mostly accept anthropologists’ implicit contributions: participating in the research gives them the chance to be listened to, to feel legitimate, to learn about the experiences of people in similar situations, and to talk about their conflicts with someone who shares their vocabulary. It also gives them space to cry, regardless of their status, or to terminate the relationship with no social consequences. Through empathetically listening and providing a discursive arena of mutual exchange, the researcher can become a “familiar stranger” (Simmel 1921). Reflections of ethics in anthropological fieldwork often refer to research constellations where the power asymmetries between researcher and researched lie clearly in favor of the anthropologist; the literature therefore focuses on research constellations where the researcher finds herself in a privileged position and has to reflect upon ethical ways of dealing with this position of being “powerful.” But both different and similar predicaments can manifest themselves when the people we interview and study (with) are more resourceful and privileged than the anthropologist.

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Reciprocity in Research Relationships: Learning from Imbalances



Mirjam Lücking

Introduction: Dimensions of Reciprocity

“It’s sad that a nice person like you will burn in hell.” This is what one of my research participants from Madura Island, Indonesia, said when I told him that I am not a Muslim. My research concerned Muslim lifestyles in Indonesia, and most of my research participants were open and welcoming toward other religions and cultures. However, in this case, as in some others, I was confronted with harsh attempts to convert me to Islam. When this particular interlocutor, a traditionalist religious leader in rural Madura, expressed his discontent with “Western infidelity,” it was the climax of a research situation in which I mostly played the role of the empathetic listener, while some of my interlocutors showed little or no acceptance of my cultural background. Nevertheless, I also felt indebted to many of my interlocutors and informants because they were making time to talk to me and showed great hospitality—despite our disparate worldviews. The combination of feelings of annoyance and indebtedness ultimately culminated in a sense of guilt that gave rise to my awareness of persistent imbalances and inequalities, which in some regards made the establishment of reciprocity a challenging and limited undertaking.

“Giving and taking” is deeply ingrained in ethnographic research life—practices that bring about emotional experiences and intercultural learning in our host communities. Conducting in-depth, qualitative ethnographic research, anthropologists

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engage in peoples' everyday lifeworlds, often living among our research participants and taking part in their daily activities. Mutuality is a natural element of any relationship. It is expected and it makes personal encounters meaningful (see Sanjek 2015). As a research methodology, participant observation requires that we reflect on the mechanisms of mutuality. Concerning the example of research participants' attempts to convert me to Islam, I found it crucial to establish an emotional distance in order to analyze their reactions to me—a non-Muslim, unmarried, female, white, Western researcher—as interesting data rather than personal attacks. Handling distance and proximity with research participants can be a contradictory endeavor. For me, the exchange of material, emotional, and intellectual values established understanding, but it was also characterized by imbalances and inequality. Thus, the reflection in this chapter focuses on imbalanced reciprocity in researchers' affects.

When I speak of “imbalanced reciprocity,” I refer (1) to one-sidedness in the dynamics of giving and taking in emotional, intellectual, and material terms and (2) to structural inequalities. Marshall Sahlins differentiates between “generalized reciprocity,” “balanced reciprocity,” and “negative reciprocity” (Sahlins 1972, p. 193–194). My use of the term “imbalanced reciprocity” is not meant as the opposite of Sahlins' concept of “balanced reciprocity,” which he sees as the direct exchange of material goods or money. The notion of “imbalance” to which I refer occurs in settings that—according to Sahlins' typology—are characterized by “generalized reciprocity,” in which there is no expectation of a direct return but an indirect commitment to reciprocate gifts.

In order to unravel these dynamics, I provide reflexive descriptions of dilemmas that I encountered during research stays in Indonesia.¹ My analysis of selected cases (1) highlights the interrelatedness of the material, emotional, and intellectual dimensions of reciprocity in fieldwork relationships, outlining different modes of “giving and taking,” and (2) provides practical suggestions of how to deal with the ambivalent emotions that result from imbalances in this “giving and taking.” As a strategy for dealing with researchers' affects in situations of imbalanced reciprocity, I introduce collaborative tandem- and team-research models that have been established by German and Indonesian anthropologists, as an example of how to create epistemological surplus from the juxtaposition of different positionalities. I argue that a self-reflective and systematic focus on exchange of material, emotional, and intellectual values between researchers and their informants enhances intersubjective understanding and helps us come to terms with pleasant and unpleasant affective experiences in fieldwork encounters even if the reciprocity remains imbalanced.

¹Throughout this chapter, I refer to experiences from a research on “Indonesia and the Arab World,” that I conducted in Madura and Central Java in 2013 and 2014 (see Lücking 2014, 2016, 2017). The project was funded by the German Ministry of Education and Research through the program “Grounding Area Studies in Social Practice” at the University of Freiburg with the grant no. 01UC1307.

Material Reciprocity

Much of our everyday morality is concerned with the question of obligation and spontaneity in the gift. It is our good fortune that all is not couched in terms of purchase and sale. Things have values which are emotional as well as material; in deed in some cases the values are entirely emotional. (Mauss 1966, p. 63)

With reference to Mauss' idea of the gift, I discuss instances of material exchange that did not occur as official transactions but as "gifts," including monetary gifts, in a context of "generalized reciprocity" (Sahlins 1972, p. 193). As the statement by Mauss suggests, material gifts become meaningful when they are related to exchanging parties' emotions. Thus, differentiating between materiality and emotionality is somewhat artificial, as the two dimensions are significantly intertwined. Yet, this differentiation also helps reveal where imbalances and inequality may complicate research relationships.

The very fact that material and emotional values are intertwined in "gift exchanges" highlights that culturally specific peculiarities are at stake. Engaging in "giving and taking" demands an understanding of the relevant "culture of reciprocity." Establishing material reciprocity is part of the famous "second socialization" of an anthropologist (see Spittler 2001, p. 12; Förster 2001, p. 461). If we neglect or misunderstand the effects of well-intentioned attempts of establishing reciprocity, research relationships can go awry.

A culture of great hospitality, for example, does not mean that there are no expectations of material returns. Accepting hospitality without attempting to give something in return means living at the expenses of people who often have very little to give. While living with research participants in Madura, I realized that because of my presence my hosts cooked special and potentially more expensive food. I calculated that, relative to the average income in the region, the act of showing great hospitality to me as their guest meant an economic burden for some of my hosts. Yet the possibility of offering direct payment—as a form of "balanced reciprocity" (Sahlins 1972, p. 193)—risked depersonalizing the relationship and reinforcing power relations. However, to accept this hospitality without appropriately reciprocating would also risk manifesting unequal power relations. Moreover, there are culturally specific ways to give money. In Indonesia, this would rarely happen openly. Rupiah notes are hidden in envelopes and exchanged via a handshake, known as *salam tempel*, literally meaning a "pasted greeting."

Similarly, the geographer Farhana Sultana shows, with reference to her fieldwork experiences in Bangladesh, that to accept certain offers and gestures of hospitality puts the receiver of these "gifts" in a superior position. When she realized that the acceptance of generous hospitality could reinforce hierarchies between her as a superior researcher and her hosts, she carefully negotiated her positionality, not by bluntly refusing hospitality nor by paying for it, but through conscious choices regarding what to eat, where and how to sit, how to dress, and how to address people (Sultana 2007, p. 379). Finding the right ways to receive and give back is part of the process of establishing rapport. In many contexts, accepting hospitality without

giving anything in return or, conversely, paying for accommodation, food, time, and information can manifest and entrench the power relations between the researcher and the researched and influence the ways in which stories are told and information is revealed. Direct payment brings a market logic into the research and induces what Graeber (2001, p. 221) calls “closed reciprocity.” In “open reciprocity,” by contrast, counterparts establish emotional and social bonds of continuous commitment, while “closed reciprocity” can cause the termination of a social relationship through meticulously calculated monetary forms of compensation (*ibid.*, p. 220). Graeber’s argument scrutinizes the challenge of “giving and taking.” How can we establish research relationships that neither involve the material exploitation of our hosts and research participants, nor create “a kind of fragile, competitive equality” within the hierarchies of the host society (*ibid.*, p. 221)? This means that a researcher’s engagement impacts on local hierarchies and can add value to, devalue, or complicate peoples’ social status.

Western ethical standards are rarely helpful in resolving the challenge of finding ways to “give back” (Dubinsky 2016; Sultana 2007). Ultimately, the negotiation of “giving and taking” remains a continuous process that constantly needs to be adjusted (Sultana 2007, p. 379). Based on his research with children in football academies in Ghana, Itamar Dubinsky states:

My experiences taught me that an unyielding embrace of Western perspectives on ethics can blind us to the local research participants’ views regarding reciprocity. Rather than coming to the field with a fixed agenda on reciprocity, it is essential to allow our engagement with the field to reshape our ethical and methodological positions. (Dubinsky 2016, p. 393)

In Madura, the situation was resolved by observing local conventions and consulting friends on how to “give back.” I understood how relatives exchange material goods and money and realized that wealthier family members are expected to show their care for disadvantaged relatives by supporting them financially. Monetary gifts are often given for a clear purpose, like renovating the kitchen or buying land. This means that those who give are aware of the receivers’ needs, sincerely caring for them. Giving adequately in material terms demands personal involvement with others’ living situations. I began to join my hosts as they shopped for groceries; we discussed monthly expenses and school fees and I eventually found ways to contribute to meeting these costs. Providing financial support for children’s education or buying a new dress for a friend on a special occasion communicated compassion for others, and hence was appreciated. Moreover, I was reminded of the Indonesian concept of *rejeki*, which, literally translated, means “fortune” and refers to sudden material gain that does not have a clear source, often considered a “blessing” or a “gift from heaven,” and which does not demand reciprocation. Labeling monetary gifts *rejeki* provides a safe space for receiving financial support. Apart from *rejeki* gifts and contributions to groceries, I made small presents to express my gratitude, which did not have a material value as much as a symbolic one. I brought souvenirs (*oleh-oleh*) from Germany, including, among other things, collectible cards autographed by the German-Turkish and Muslim soccer player Mesut Özil, which evoked considerable excitement among my research participants and often prompted

conversations about Muslim cultures and identities in Indonesia and Germany. Thus, if considered carefully, gifts can build bridges between different cultural and religious positionalities.

Obviously, material gifts are often linked to emotional relatedness. In the semi-structured emotion diaries designed by *The Researchers' Affects* project, one question that triggered my reflection in this regard was "Who are you in the field?"² While living with my research participants, I became a friend, a sister, and a daughter. However, although I supported my status as an adopted family member by finding ways to contribute to living expenses, in many cases, the temporality of my presence was clear. Other possible answers to the question "Who are you in the field?" included guest, Westerner, and outsider. Moreover, sharing in living expenses did not involve the same financial burden for me as it did for local people. Compared to most of my research participants, I was rich. The imbalances between my material contributions and those of my research participants, and my corresponding feelings of indebtedness, indicate structural socioeconomic inequalities and asymmetrical power relations on a broader scale. Learning about the conventions of "giving and returning" cannot deconstruct differences in access to privileges. These structural inequalities and the awareness of persistent imbalances evoked feelings of guilt and frustration, which were part of the process of learning and caring about the living situation of my research participants.

Emotional Reciprocity

Looking more closely at the emotional dimension of reciprocity, we find that emotionality is not simply a matter of establishing rapport. Participant observation is a method that involves all the senses, and any related emotions are not only personal byproducts but connect anthropologists to the lifeworlds they study and can hence be understood as ethnographic data. In his plea for "thick participation" as a radical form of participant observation, Gerd Spittler (2001) argues that participant observation is especially effective in gaining access to the field and a more general understanding of a society or topic. Similarly, Förster argues that it is misleading to consider language as the essence of culture, and therefore, participation and everyday seeing/observing are crucial elements in ethnographic fieldwork (2001, p. 474). Researchers tend to prioritize verbal data, which can be misleading, as there is often a discrepancy between words and deeds (*ibid.*, p. 474), and situations often arise in which the actual meanings of words are not as important as the emotional subtexts of conversations (Wikan 1992, p. 470). Wikan argues that empathy or "resonance" is a form of communication and understanding that is not language based (*ibid.*, p. 466, 470). Comprehending a person's situation entails emotional reactions, grasping their fears, hopes, needs, and desires. Emotions are both method and data.

²See Stodulka et al. (2019) and Appendix.

With many of my research participants, the emotional relatedness felt like a natural process and research relationships became friendships. In these friendly research relationships, I found it easy to reciprocate emotionally and to understand subtle notions and emotional communication. Asking out of sincere curiosity and reciprocating because of sympathy and care made fieldwork life very enjoyable. Here, research was not a one-way activity. I took time to answer my research participants' enquiries into my personal and cultural background, becoming a "resource person" for some questions, for example, concerning information about scholarships and higher education abroad.

Apart from gaining an understanding of the research context, growing accustomed to social conventions and emotions was especially crucial when it came to the question of consent. In Indonesia, it is considered impolite to openly reject a request or to bluntly say no. In order to determine research participants' feelings about their willingness to take part in my research, it was necessary to recognize indirect messages and emotional subtexts. This also meant that I would give interlocutors the opportunity to reject my enquiries by presenting excuses to them like, "These are busy times; maybe if you have time to talk to me, give me a call." By verbally postponing potential involvement in the research or suggesting that I could be contacted at another time, I avoided putting my research participants in a situation where they had to make a statement of consent on the spot. Moreover, I sought reconfirmation of their consent in the course of the research.

While this strategy mostly worked well, establishing resonance and rapport with research participants whose perspectives I found at times hard to comprehend, who were openly hostile, or whom I simply didn't like was difficult and sometimes impossible. I found myself very consciously in this mode when I talked to persons who denounced worldviews other than their own as sinful mistakes. I became impatient when I had the feeling that there was no acceptance on their part of, or interest in, my lifeworld and cultural background. Research can be an emotional and intellectual one-way effort in such circumstances. The researcher is the one who learns; makes an effort to master a foreign language; studies historical, religious, cultural, and political contexts different from her own; and undergoes sometimes physically and emotionally arduous fieldwork. This one-sidedness can be frustrating. For example, the religious leader whom I quoted in the beginning of this chapter further questioned my legitimacy as a non-Muslim in studying a topic concerning Islam, suggesting that I wanted to do "harm" to Islam. I felt attacked, rejected, and misunderstood. Had I not gone to every length to conduct sensible research, to understand emic interpretations and expectations, and to foster mutual understanding, while opposing Islamophobia? "And all that I get in return is rejection," I wrote in my field notes. At the same time, I reflected that it had been my decision to do this research. Why did I expect to be welcomed, as I was by the vast majority of research participants? In order to understand where my interlocutors' reactions came from, it was helpful to distance myself emotionally and to understand that this was not a personal attack against me but against the things that I represent in the eyes of my interlocutors.

The traditionalist Muslim leader in Madura did not want to hear what I had to say, he did not want to listen or to establish rapport. He wanted to express his sentiments, and I obviously represented an enemy to whom he felt he could address his grievances. Ironically, for my interlocutor, I represented the “Western infidel,” the “Orientalist” who studies Islam in order to harm the Muslim world, whereas in Germany and other Western societies, I have been attacked by xenophobic people for “defending” Islam. Thus, despite all efforts at establishing reciprocity, the imbalances and confrontation that arise in research encounters reveal broader frictions within and between societies. Ethnographic research enables researchers to describe these frictions and may contribute to challenging or even resolving them.

To treat hostile reactions as research data was a learning process for me. I felt personally insulted and was struggling to find a professional stance as a researcher. Here, instead of empathy, emotional detachment helped me deal with frustration and feelings of deficiency. I found this particularly challenging when the people who I felt insulted me also offered me material resources like food and transportation or even accommodation. Could I accept sleeping in the Qur’anic school of the Muslim leader? Could I stand dealing with his grudge every day? Could I still treat this situation as an interesting research opportunity, taking the chance to get behind the scenes at his Qur’anic school? Would I end up in a situation where I would have to thank him for letting me stay and explore, a situation which would establish a hierarchical imbalance between an indebted researcher and a seemingly generous religious leader, who seeks to buy my loyalty? In the end, I didn’t stay at his house/school and did not follow up his story. I felt that the material imbalances would complicate the distance that I needed to maintain in order to analyze the situation. Consequently, I cannot report much about the inner workings of his school. My own limitations mark the limits of my research capacities in this case.

In most other cases, differences in worldviews, values, and convictions were more subtle. Even though some other research participants might have similarly disapproved of what they perceived to be my “irreligiousness,” the encounters did not have the same harsh aspect. Moreover, I had found ways to reciprocate in material terms and did not feel that I would be indebted if I accepted the material offers of accommodation, food, and transportation made by my research participants. Nevertheless, underlying material imbalances, in combination with differing values and convictions, evoked ambivalent feelings about the research relationship. This was particularly difficult when I received actual “gifts.” For example, the wealthy leader of another Qur’anic school in Madura gave me perfume and jewelry that she had brought back from her pilgrimage to Mecca. On the one hand, I did not feel comfortable receiving these gifts, while on the other hand, I felt honored that a research participant would give to me something special and dear to them. Now, these gifts remain as physical proof of the ambivalences that exist in research relationships. They represent the imbalances and complexities of engaging in material reciprocal exchange. They are a reminder of the co-existence of mutual sympathy and contrasting values and convictions.

The uneasiness I felt about such acts of receiving, rejecting, reciprocating, or compensating expanded from the field to my desk, where I realized that my interpretation and representation of data was affected. The emotions provoked by the imbalances, inequalities, dissonances, but also the joys of “giving and taking” in the field affect data analysis and the representation of research results. While writing, I began to wonder how I could reciprocate with the results of my research.

Intellectual Reciprocity

The social geographer Tatek Abebe sees material exchange as short-term reciprocity, while he considers the communication of research findings to participants and policy makers as long-term reciprocity that can help improve the lives of research participants (2009, p. 461). In the field, acts of reciprocity—material and nonmaterial ones—are often affective and spontaneous. At the desk, the question of what we can give back in the long run may cause more a serious headache. Moreover, this “long-term reciprocity” may in fact have no effect on research participants’ immediate living situations, whereas a material gift—like supporting education or land ownership—can have a sustainable impact for research participants. Thus, the question of what we can return through research results is a rather hypothetical one, especially in the case of basic research. In the following, I describe “intellectual reciprocity,” as being relevant in (1) the representation of research results and (2) the intellectual exchange with research partners. In line with the examples of my research on Islam in Indonesia, I reflect on how intellectual reciprocity might look in situations that essentially challenge our normative and ethical standpoints.

Ethnographic data are intellectually and affectively precious to the researcher. But to whom does this data belong? Analogous to the copyrights held on photos, the stories told by research participants remain their own. In this regard, “giving back” the documentation of research data is a good way to acknowledge that the data in fact can be a treasure to our research participants.³

Yet, research data are no “raw material.” In the end, we shape and filter research results. What we can offer is an outside viewpoint on the phenomena we investigate: an informed perspective that may not always represent what research participants might hope for. Even though anthropologists use inductive approaches and aim to represent emic perspectives, the theoretical lens through which we look, listen, and feel cannot be underestimated. Henrietta Moore has described this as a “pre-theoretical commitment” of subconscious, theoretical preconceptions, or a *Zeitgeist*

³An example of this is the compilation of historical accounts in local language as it has been achieved by Judith Beyer for her research location in Kyrgyzstan (see Beyer and Inogamova 2010). Another example is the project behind this book: Among other things, Thomas Stodulka shared the transcript of our interview with me, which I highly appreciated and fostered my realization that data are not only precious to the researcher. Furthermore, giving back the data can be a trigger for further inquiries. In ethnographic filming, this method is well known as “elicitation.”

that we do not question but take for granted (2003, p. 18).⁴ In other words, my uneasiness about “taking” also concerned the practice of taking information or “data,” which I would analyze and interpret with the aim of producing authoritative scientific knowledge. This knowledge is represented in the conventions of Western academia. The feeling that I could not adequately return something substantial to my research participants lingered, spread across my writing desk, and permeated my academic presentations at workshops and conferences.

For example, I frequently discussed ways to challenge Western Islamophobia with my research participants in Indonesia. While more theoretical explanations of my academic interest in the intertwining of sociocultural and religious change appeared confusing for research participants, the idea of contradicting Islamophobia was something that they could relate to more easily. Indeed, some research participants linked these explanations to their own interests and soon spread the word that I was conducting research in order to fight Islamophobia. In principle, this was true, as I wanted to show that Islamic traditions were multifarious and that Islamic lifestyles were linked to social structures and traditions, which rendered Islamophobia irrational. Nevertheless, I knew that I could not fulfill some research participants’ expectations that I would become an “ambassador” in the name of Islam, which was often linked to their hopes of seeing me embrace Islam. Yet, as this example shows, discussions about the impact of research in the long run triggered intellectual reciprocity. It also helped me understand the potential harm and beneficial effects of research. After all, our own interpretation of what is “beneficial” might differ from research participants’ ideas in this regard, and in some circumstances, “giving back” intellectually remains an unattainable goal.

In this sense, cooperative research with colleagues in Indonesia essentially contributed to my learning process. Here, my approach was very much inspired by the Freiburg-Yogyakarta model of tandem and team research that was invented by Judith Schlehe and successfully implemented in the framework of a now 16 years partnership between the Department of Social and Cultural Anthropology of the University of Freiburg and the Department for Anthropology of Gadjah Mada University in Yogyakarta (Schlehe and Hidayah 2014). Schlehe (2013) describes the tandem model, which fruitfully combines different perspectives in research practice, as “wechselseitige Übersetzungen” (“mutual translations”). This means that research partners systematically “translate” their different positionalities and use them consciously in research encounters and data analysis.

In Madura, I experienced this kind of cooperation with the anthropologist Khotim Ubaidillah, or Ubed, who significantly supported my research through his expertise on social theory, local language, and culture, and through our joint reflection on our differing perspectives. His reaction to my description of the encounter with the religious leader in Madura illustrates how our cooperation opened up ways for further interpretation, while my personal emotions had limited them. After Ubed read a thesis chapter in which I reflected about being confronted with missionary activities, he commented:

⁴For discussions about the relativity an inductive approach in ethnographic research see: Bernhard (2006), Davies (1999), Förster (2001), Gobo (2008), and Moore (2003).

I found it funny to read this. This represents a funny and ironic reflection about your research participants. (...) I remember the interview with the old Kyai⁵ who spoke harshly and urged you to become Muslim. (...) Didn't this incident become a trigger for our discussion about the [inter-religious] tolerance of Madurese people? About these provocative efforts [to proselytize]—from the softest way to the more straightforward ones—I only keep them as comical stories. (...) I remember an anecdote that reflects the exorbitance of orthodox NU⁶ people in Madura, which says: “My child, you have to marry a fellow Muslim. If you can't, then go and marry an adherent of Muhammadiyah.⁷ (E-mail correspondence, Khotim Ubaidillah 24.11.2016, translation M.L.)

This anecdote refers to Muslims in Madura who belong to the Indonesian Muslim association Nahdlatul Ulama (NU) and who do not approve of any other Muslim branch. By reminding me of the fact that NU Muslims in Madura are intolerant toward any other religious or ideological orientation—including other Muslim orientations—Ubed resolved my feeling of being personally attacked, shifting my interest to tensions between Muslims, the reasons for the exclusivist NU mentality, and the multilayered meanings of this *Kyai's* harsh words. Moreover, he reminded me of the fruitful discussions that ensued from my frustration and the fact that our experiences of the situation were remarkably different. Ubed's ability to look at the situation with a sense of humor was enormously helpful in enabling me to reflect on my emotions and render them productive for further inquiries.

Juxtaposing our different feelings and perspectives during research allowed us to engage in fruitful data analysis. Taking different positionalities into account, identifying different perspectives, contextualizing, and representing them, corresponds with the demands of the *Writing Culture* debate of the 1980s. James Clifford famously argued that truth(s) are only partial (2010, p. 1–26), and different perspectives complement one another. Rabinow (2010) suggests pluralizing and diversifying research approaches to avoid hegemony in knowledge production. In order to use different approaches and engage in different perspectives in dialog, an awareness of one's own perspective is presupposed (ibid., p. 244), but at times only painfully achieved. The cooperative research and friendship with Ubed was essential in allowing me to practice reflexivity and juxtapose different perspectives. My gaze was limited because of the ambivalent feelings that very complex field situations had caused. Ubed's humor and his perspective opened my eyes to the reasons for and the complexities of the *Kyai's* statement. Representing a variety of perspectives and making clear our research positionalities and related ways of feeling, understanding, and writing contributed to a more holistic understanding of research data.

And yet, no collaboration is flawless. Even though this model has achieved a high degree of fruitful research cooperation, it too has its limits; intellectual reciprocity does not flatten power imbalances that have material and structural implications between research partners. Conventions in international academia privilege

⁵Religious leader.

⁶NU is the largest mainstream Muslim association in Indonesia, known for its orthodoxy but also mystic traditions in Islam. NU has its strongholds in rural areas.

⁷Muyammadiyah is the second-largest mainstream Muslim association in Indonesia. It is associated with reformism and urban Muslim lifestyles.

researchers who work at Western research institutions. In international “competition,” the hegemony of Western academic traditions remains decisive. Nevertheless, creating intellectual reciprocity is a first and essential step in decolonizing knowledge production and can contribute to the uncovering of global and local inequalities as well as strategies for overcoming them.

Outlook: Limits of Reciprocity

The examples at hand show that the establishment of reciprocity is in many ways imbalanced and limited. The spheres of materiality, emotionality, and intellectuality are intertwined. Differentiating them analytically allows one to understand the context in which imbalances occur: (1) imbalances in material exchange that occur through the culturally specific meanings of exchanging material/monetary gifts, the structural inequalities prevailing between the researcher and research participants, and hierarchies within the host community. (2) imbalances on the emotional level, within the methodological contradiction of establishing proximity and empathy on the one hand and professional distance on the other. And (3) imbalances in the attempt to establish intellectual reciprocity, when hopes about the possible impact of a research differ and when structural inequalities impact on researchers’ different access to resources and privileges.

As the example of cooperative research indicates, accepting limitations and establishing an awareness of one’s own positionality can expand our interpretative capacities and help researchers come to terms with the affective dimensions of fieldwork. My struggle with affects in the course of imbalanced reciprocity came with the idea that balance, equality, and mutuality constitute the desirable fieldwork mode, while imbalances and one-sidedness are negative and destructive. Even though I maintain that a critical reflection about imbalances is crucial—especially power imbalances and structural inequalities—it turns out that in many cases, there are intractable limits to the establishment of balance, equality, and mutuality. Being aware of the reasons for imbalance can contribute to an understanding of the situation and may enable a contribution to structural changes. After all, to reciprocate does not automatically mean to establish equality. Finding a solution to this is not easy. Considering material reciprocity, the immediate “return” of a material gift, a payment or compensation, can even “close” research relationships (see Graeber 2001, p. 221). This short-term reciprocity (see Abebe 2009, p. 461) may only gloss over structural imbalances and inequalities rather than changing structures. Therefore, attempts at “giving back” are a continuous process that does not end with the conclusion of a research project. The sense of guilt that resulted from my reflection on persistent imbalances was important in order to come to the realization of unattainable reciprocity. Here, the question of what researchers can return expands beyond the field and becomes political.

To come to terms with researchers’ affects does not mean to flatten emotions and gloss over imbalances but to make sense of them. Feelings of guilt, for instance,

although painful, can trigger reflections about research ethics, power relations, and the political implications of research endeavors. In this regard, I consider cooperative research approaches not only a fruitful methodological model but also as an essential step in the decolonization of knowledge production. Even though there are limits to establishing equal conditions for tandem researchers, the continuous striving for material, emotional, and intellectual reciprocity marks the cooperation as a promising long-term commitment.

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Reciprocity Reconsidered: Toward a Research Ethic of Economic Participation



Mechthild von Vacano

Considering how many anthropologists conduct their fieldwork in research constellations of stark economic asymmetry, I find it remarkable how little is written about the ethical and practical implications of this fact. The self-reflexive turn in ethnographic writing has made it rather popular to reflect upon the social and emotional qualities of research relationships, though we usually learn little about the economic dimension of these encounters.¹ Even when addressing reciprocity in field relationships, most anthropologists tend to focus on immaterial aspects of exchange (e.g., Wax 1952) rather than getting into “the nitty-gritty on-the-ground negotiations” (Wesner et al. 2014, p. 8) of material inequality.

The economic dimension of research relationships seems to be treated as a nuisance, rather than an ethical or epistemological issue, while economic inequality is treated as a challenge to be handled in private. By this omission, novices are left without much guidance as to how to negotiate economic expectations in the field or how to act in a position of relative wealth, vis-à-vis interlocutors struggling with poverty. Many struggle with situations and personal encounters in which structural global inequalities become tangible. Because of the lack of conceptual tools and practical advice, (novice) ethnographers are often overwhelmed by these confrontations and are burdened with feelings of guilt as a result (e.g., Baker-Médard 2014, p. 2). In helpless attempts to compensate for this guilt, some overexert themselves and their resources (Rudge 1998, p. 20). Others are weighed down by profound disappointment when close interlocutors repeatedly solicit material assistance from

¹In 1997, the editors of *Anthropological Theory* published a call for contributions on the “gift relationships between ethnographers and their hosts” (Kingston et al. 1997); the response seems illustrative: only two brief commentaries by de Waal (1998) and Rudge (1998) were published.

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them: “I simply felt hurt and exploited over and over again It felt painful to be reduced to a ‘walking cash machine’” (Stodulka 2014, p. 7).

For decades, the anthropological practice of fieldwork has been criticized for its proclivity to reproduce (post)colonial power relations. Yet despite—and potentially even enhanced by—this critique, anthropological research relationships are surrounded by an aura of idealism. It is a common theme for fieldworkers to emphasize the close and equitable relationships they had with their key interlocutors, a relationship often represented in terms of friendship (Driessen 1998, p. 53–54). But as well-intended as this framing may be, these relationships ultimately remain research relationships: they are characterized by multiplex power asymmetries and an inherent instrumentality, at least on the researcher’s part. As an ethical guideline for overcoming these inequalities, ethnographers commonly invoke the principle of reciprocity (see chapter “[Reciprocity in Research Relationships: Introduction](#)”). Like the notion of friendship, however, reciprocity offers a rather idealistic framing for research relationships. This idealism stands in the way of acknowledging the economic dimension of fieldwork.

In this chapter, I argue that the only productive way of dealing with the overwhelming feelings of guilt and disappointment associated with economic expectations and inequality in the field is through systematic reflection of the economic relations which shape our research encounters. I hope to advance this reflection by a critical reevaluation of the—ethical and economic-anthropological—reciprocity paradigm and by developing an alternative ethical-methodological framework of “economic participation.”

Revisiting the Reciprocity Paradigm

The notion of reciprocity is rooted in the foundational debates of economic anthropology and has since gained status as one of its key concepts. Parallel to this development, the idea of reciprocity has also entered anthropological ethics debates, while bearing only traces of its economic-anthropological origin. In the following section, I will revisit the economic concept of reciprocity in order to discuss its implications for the research ethic of reciprocity.

Reciprocity as an Anti-Market Principle

Through Karl Polanyi’s reception of Marcell Mauss, the concept of reciprocity has been closely intertwined with notions of “the gift” (Gregory 1994, p. 922). Thus, reciprocity came to characterize all forms of mutual give-and-take based on the practice of gift exchange. Absorbing Chris Gregory’s distinction between gift exchange and commodity exchange, reciprocity came to be associated with the moneyless and socially meaningful characteristics of gift relations, in contrast with

the monetarized, and hence impersonal, properties of commodity exchange (*ibid.*, p. 911). In continuance of this binary, reciprocity was construed as the principle of nonmarket exchange as opposed to market exchange: reciprocal, that is, nonmarket, relations were supposed to be directed toward fostering social bonds (of obligation) and driven by social motives, whereas market relations were associated with materialistic values and individual motives of profit-making. As these binaries express, reciprocity has come to symbolize a principle “other” to capitalist market economies and has been charged with a moral, anti-market sentiment.

According to yet another binary, the paradigm of reciprocity offers an embedded view on the economy, as opposed to the presumed “disembeddedness” of market economies (Polanyi 1944/2001, further developed by Granovetter 1985). In emphasizing the social qualities of economic relationships—and, conversely, the economic qualities of social relations—the perspective of embeddedness offers a valuable contribution to anthropological ethics debates. It could help to reconcile the social with the economic dimensions of research relationships. Unfortunately, this critical potential seems to be mostly lost. Only the anti-market attributions and their (implicit) moral evaluations seem to have found their way into discussions on ethics. Reciprocity then, paradoxically, serves to de-economize the research relationship, because it prioritizes the “good” social over the “corrupt” economic. In consequence, anthropological fieldwork is idealized as an economically disinterested activity, threatened with corruption if interlocutors should declare material interests.

This seems particularly pertinent if money is involved. Most anthropologists emphatically reject the idea of paying interlocutors,² reserving the option of monetary compensation exclusively for translators and research assistants. Money seems to threaten the trusting relationship between researcher and interlocutor, turning the “friendship” into a business relationship (Driessen 1998). Payment is also suspected to spoil the credibility of an interlocutor’s account (Das and Parry 1983; Moore 1998, p. 57). Data are attributed a higher epistemological status when provided as a gift—as opposed to data sold as a commodity. To economic anthropologists, this image of money as a corruptive and socially disruptive force is as familiar as it is outdated, because the underlying idea of money possessing an “intrinsic power” to transform people—and whole societies—has been thoroughly debunked (Bloch and Parry 1989). If fieldworkers decide to compensate their interlocutors for their contributions, they certainly need to consider the potential social dynamics; but this holds true for any kind of reward, whether made in the form of money, in kind, or as an immaterial return.

Even if it is not about money, most ethnographers react rather reluctantly when interlocutors insert their own material interests into the research relationship. Their reservations mostly resemble those methodological or epistemological concerns associated with monetary compensation. But their skepticism toward material

²According to Driessen (1998, p. 58), this has always been the case for Great Britain, whereas in the United States, the practice of paying informants became only discredited in the second half of the twentieth century.

interests also speaks to a personal fear, widely spread among fieldworkers, to be “taken for a naive fool, whose ignorance could be used to advantage” (Wax 1952, p. 36). For others, it is more of a personal disillusionment, as Rabinow (1977/2007) describes, reflecting on the relationship with his Arabic teacher Ibrahim: “Basically, I had been conceiving of him as a friend because of the seeming personal relationship we had established. But Ibrahim, a lot less confusedly, had basically conceptualized me as a resource” (p. 29). In retrospect, Rabinow concedes that Ibrahim had “not unjustly” situated him “with other Europeans;” at that time, however, it was exactly his being treated as just another European that had devastated Rabinow’s romantic—and culturally biased—imaginary of friendship.

Thinking Beyond the Moral Grounds of Exchange

As an ethical framework for research relationships, the concept of reciprocity has yet another limitation: it is restricted to the moral grounds of exchange. In this respect, as Graeber (2014) argues, reciprocity essentially operates according to the same principle as market exchange: both imply an exchange of equivalent values and a morality of balance. In a market transaction, a good is purchased by paying an equivalent amount of money; in a reciprocal transaction, a gift is received, but the receiver is expected to give something of the approximate same value in return. The timing of the return may be delayed, and equivalence may only be achieved over a continuous process of give and take; but the exchange is expected to be balanced in the end. But balance and equivalence presuppose equality (ibid., p. 72). As a result, reciprocity can only sustain equality among equals, while inevitably reproducing inequality among unequal partners. An ethic of reciprocity, thus, seems inherently inapt for addressing economic or other power asymmetries in research relationships. But on what other moral grounds could we deal with inequality?

Graeber offers two alternative principles: “hierarchy” and “communism.” According to this distinction, a transaction is based on the moral grounds of hierarchy if the transfer between the transactional partners is one-sided, and if the social relationship between these partners does not (significantly) extend beyond the transaction. This principle of hierarchy is most prevalent between people of different classes and social status. Hierarchical transfers can either manifest themselves in one-sided taking or one-sided giving; they can range “from the most exploitative to the most benevolent,” and can include such practices as charity and almsgiving (ibid., p. 73). Hierarchy inevitably reproduces inequality; hence, it seems irreconcilable with the ethical values of anthropology, even in its benevolent form. De facto, however, some of our research engagements might well be governed by a logic of hierarchy, even if it is often wrapped in a “rhetoric of reciprocity” (ibid., p. 74) to gloss over the actual imbalance.

The principle Graeber (2014) provocatively labels “communism” is stated in the simple formula: “From each according to their abilities, to each according to their needs” (p. 67). Instead of equivalence, this principle is oriented toward the relative

possibilities and needs of the transactional partners involved; it accounts for the unequal distribution of wealth, rather than presupposing equality. The moral principle of communism can even operate within commercial transactions. This is the case if sellers adjust their prices according to the (presumed) wealth of a buyer. These adjustments can work both ways, from the economically privileged toward the underprivileged or from the poor toward the rich. As an example for the latter, Graeber recounts the story of a colleague—I presume a white anthropologist from the USA or Europe—who had conducted fieldwork in rural Java. Shopping at the local market, she was determined to improve her bargaining skills. But over the time, the anthropologist became frustrated “that she could never get prices as low as local people” (ibid., p. 70), until a Javanese friend made her aware that it was not a matter of skill, but a matter of economic distribution: market vendors would charge higher prices of anyone whom they categorized as rich. This episode shows how our moral grounds shape our expectations toward economic transaction. While the anthropologist expected to be treated as an equal (once she mastered the cultural forms of bargaining), the market vendors addressed her as economically privileged and felt entitled to benefit from her wealth.

The anti-market sentiments of “reciprocity” as an economic concept feed into a disciplinary skepticism toward material interests, leading to a neglect of the economic dimension of fieldwork. Rooted in the moral grounds of exchange, the ethical principle of reciprocity further implies a relationship among equals, while failing to offer a framework for confronting (economic) inequality.

Moving Toward an Ethic of Economic Participation

As an invitation to think beyond reciprocity, I want to sketch out an alternative ethical framework of economic participation. It is based on years of academic and non-academic engagement in Indonesia³ and a critical review of ethnographic methodology, while the detailed development of the framework reflects my experience of conducting fieldwork in a Jakarta neighborhood. For orientation, let me briefly outline the context of my study.

From October 2014 to September 2015, I conducted classical stationary fieldwork in a neighborhood community in Jakarta, Indonesia. My research project focused on the subjective experience of different forms of work and was driven by an underlying interest in the plurality of economic subjectivities. I had chosen the *kampung*-type neighborhood of Gang Buah as my research site. In the urban context of Indonesia, *kampung* refers to densely populated neighborhoods of a lower to lower-middle class milieu. These are characterized by complex entanglements of social and economic relations among neighbors (Wilson 2015, p. 25) and an ethos of mutual responsibility. Situating my research in such a context allowed me to

³ Since 2004, I have learned from relationships with people in Jakarta and Yogyakarta which shifted between friendship, political solidarity, and academic collaboration.

study a diverse mix of income-generating activities across the spheres of formal and informal economy (see Newberry 2006), while grasping the social embeddedness of work.

To immerse myself into the neighborhood, I had rented a room in the private home of Bu Nani, an elderly widow and mother of four adult children. For Bu Nani, such a commercial live-in arrangement was not a strange thing to do, since she had rented out to students before to earn some additional income. Conveniently for both my research and my sustenance, Bu Nani operated a food stall (*warung nasi*) on the front porch of her house where she served a variety of home-cooked dishes to the neighbors and passersby. On the other side of the front porch, her daughter Dinda offered fresh juice and a changing selection of snacks for sale. I spent long hours on this veranda, talking to neighbors stopping by and learning about Dinda's and Bu Nani's business routines. In the mornings, I watched Bu Nani prepare the meals for the day, while we chatted about her life, family, or the neighborhood. Over the course of time, Bu Nani and Dinda became key protagonists of my fieldwork, just as I became a member of their family. I turned from a boarder—the Indonesian term *anak kos* literally translates as “boarding child”—into one of her children (*anak*), as Bu Nani would proudly introduce me to her guests. I was not the first *anak kos* to become family. Bu Nani loved to tell me stories of other former *anak kos*, who years after leaving Gang Buah still would come back to visit her and to inquire about her health while slipping her some money; what made them family was a mutual relationship of care. By recounting these stories, Bu Nani offered a model for our relationship—and I took the cue regarding how I was expected to express my caring in the future.

Acknowledging the Embeddedness of Economic Transactions

My relationship with Bu Nani is just one brief example of how complex our social and economic entanglements with key interlocutors can be. It further shows that the economic dimension of a research relationship often cannot be separated from any other dimension. An ethic and methodology of “economic participation,” as I propose, would take these complex entanglements as a starting point. By expanding the notion of “embedded economies” to our anthropological research relationships, economic participation rests upon two basic assumptions: that social relationships, including field relationships, are permeated by economic relations; and that economic transactions have the potential to (re)produce sociality. In practice, economic participation can include a wide range of transactions, which I suggest clustering into the following four categories:

1. In classic stationary fieldwork settings, economic participation starts with seemingly mundane matters, like *accommodation arrangements* and *everyday acts of consumption*. The daily interactions of the ethnographer are shaped by simple routines, such as sharing meals, buying refreshments, or dropping off laundry.

These routines can be rather consequential for developing research relationships, because they allow for an ordinary form of contact, the ordinariness of which can foster a distinct quality of rapport and trust. In return, these choices of everyday consumption may cause disappointment with those whose offers of goods or services are foregone.

2. On a collective level, the field might provide opportunities to *engage in communal forms of gift-giving or sharing*, which often function as practices of relating. In my Jakarta fieldwork, for example, I experienced the communal feasting practice of *slametan* as a simple though effective way to perform neighborliness. Not only did I contribute fruit or cake to my next neighbors' *slametan*, but I also came to host my own farewell *slametan*. I could not have done so without the social and culinary skills of Bu Nani, who relieved me of most preparatory work. But in spite of this experienced support, I still was stressed by the delicate diplomacy of hosting such a symbolic event.
3. Economic participation can, furthermore, include *various forms of practical assistance*, from small gestures of attentiveness to substantial inputs of labor power. Some of these practical contributions may require the specific skills or resources of the researcher, while others might be of a rather general character. During the long and regular sessions of participant observation at Bang Akri's screen-printing workshop, I tried to make myself useful whenever possible. I managed to gradually refine my skills in assisting the manual printing process, while Bang Akri and his wife Lela came to play central roles in my ethnography.
4. Last but not least, anthropologists can *engage in (dyadic) practices of giving*. As I have discussed with regard to the reciprocity paradigm, these transfers can take such various forms as remuneration, gifts, donations, or sharing and may involve money or be made in kind.

Within these categories, transactions can operate on one of the three moral grounds identified by Graeber: exchange, hierarchy, and communism. To practice economic participation appropriately, anthropologists need to learn to read the (implicit) moral expectations of their interlocutors.

Negotiating Abilities and Needs

As a holistic approach to fieldwork, economic participation is based on the anthropologist's active and conscious integration into the socioeconomic fabric of the context under study. For this active integration to succeed, anthropologists need to identify or create social roles which allow them to openly conduct their research and, simultaneously, account for their relative positionings within (global) power structures. These roles can be multiple and change over time, depending on personal constellations and situational context. To negotiate these roles in the field, economic participation requires anthropologists to reflect upon their own economic

positionality vis-à-vis their interlocutors and within the broader socioeconomic structures of their fields; it challenges them to acknowledge their tangible privileges and account for the material inequalities that shape their research encounters.

In my example, I was unambiguously rich relative to my social surroundings in Gang Buah. My fieldwork was funded by a doctoral research grant from the German Academic Exchange Service (DAAD) which, while just sufficient by German standards, amounted to six times the local monthly minimum wage. Many residents of Gang Buah struggled to even attain that formal minimum.⁴ But in absolute numbers, my budget was still limited, and I was wary about the sum and scope of spiraling expectations that interlocutors could project into my presence, because as a white anthropologist from Germany I represented a considerable degree of privilege. From the onset of my fieldwork, I was careful to manage my economic appearance. I wanted to avoid raising false hopes, while at the same time protecting myself from the pressure of unrealistic expectations. I tried to walk the fine line between acknowledging my resources and not inviting excessive expectations. In Graeber's terms, I was faced with the challenge of negotiating the different "abilities" and "needs" that existed between me and my interlocutors. Abstracting from my experience, I would break this challenge down into three essential steps: determining my own abilities, managing expectations, and dealing with demands and expectations.

1. To *establish my financial abilities*, I developed a simple budgeting strategy. Guided by the basic principle that I neither wanted to enrich nor indebt myself by doing fieldwork, I decided to divide my monthly scholarship into three budgetary categories: The first covered personal expenses, such as rent, food, and transportation, but also 2 weeks of holiday and ongoing payment obligations from Germany. As a second category, I had allocated a research budget to cover all professional expenses from administrative costs to book purchases and transcription fees. All my remaining funds went into the third budget category, designated for all kinds of social purposes, including individual requests for financial assistance or neighborly gifts and donations. Toward the end of my stay, I used the remaining funds to donate to the administrative unit of the neighborhood and to make monetary gifts to the three families that had become central to my fieldwork.

On an emotional level, this budgeting strategy worked as a coping mechanism, because it depersonalized the negotiation of abilities and needs. I did not have to weigh my interlocutors' interests against my own needs, because they concerned separate budgetary categories. Consciously determining a financial framework for my own needs relieved me of having a bad conscience whenever I treated myself with something beyond minimal needs. Most importantly, knowing that I would donate all remaining funds at the end of my stay gave me some emotional relief whenever I was overwhelmed by feelings of gratitude and guilt in the process of doing fieldwork.

⁴Large parts of the working population are not affected by minimum wage regulations, because they only pertain to workers in formal employment or employed by formal enterprises.

2. *Managing expectations* was as much about managing my economic appearance as it was about being transparent about my abilities. In initial encounters, I tried to downplay my economic options, while staying within the limits of truth. For example, I would highlight my position as a PhD student, while rarely citing my (casual and part-time) employment with a German university which had actually been on hold for the duration of my fieldwork. I furthermore avoided publicly appearing as a donor, because I was determined to avoid unleashing a spiral of expectations. Therefore, I waited until the last months of my fieldwork to make any substantial contribution to the neighborhood association; I then explicitly wanted people to know about my donations, as a gesture of compensation for having supported my fieldwork.

But, on the other hand, I never sought to hide my economic privilege. Within my routine transactions of everyday consumption, I tried to find arrangements which accounted for my economic status. Negotiating the conditions for my live-in arrangement with Bu Nani, for example, I encouraged her to consider my economic background when naming her price—consciously referring to the morality of adapted pricing. I did not want to pretend to be an *anak kos* living off an Indonesian student budget. For this reason, I found it only appropriate to pay Bu Nani a monthly rent of about 30% over the usual fee. In my daily business dealings with Dinda, on the other hand, I insisted on paying the regular (market) price for my daily mango-orange juice, when she wanted to offer me a family discount. I explained to her that I wanted to support her business—despite being family. In these simple moments, I tried to be upfront and proactive about my relative wealth.

When I was approached with expectations that clearly exceeded my abilities, I tried to make clear my real limits, while at the same time underscoring my commitment to assisting in other ways. After I had attended my first neighborhood assembly, one of the representatives, Pak Gunawan, enquired about my ability to finance improvements to the local drainage system. For a moment, I felt put in a spot, because neither did I want to make a rash and polite promise which I later could not keep, nor did I want to reject his request too categorically, especially this early in my fieldwork. We eventually had a long and open conversation about my realistic options and limitations in supporting community projects. In the long run, this talk laid the groundwork for a trusting relationship between me and Pak Gunawan as well as the entire administrative neighborhood leadership. This experience taught me to appreciate any opportunity to explicitly negotiate the expectations toward me, and it encouraged me to communicate my limits with polite clarity.

3. In *dealing with material demands and expectations*, I found it helpful to de-personalize the situation and consider the wider context: from a perspective of structural inequality, it seemed reasonable to me that interlocutors would approach me for my resources. I tried to acknowledge this fact, no matter how I felt in this moment about being called out for my privilege. As a general stance, I did not take the expectation personally. I also took the personal liberty of

detaching myself from the situation and, rather technically, referring such requests to my budget.

Complementarily, I tried to reflect on the situation from an anthropological perspective. I began to interpret my own experience against the ways in which economic inequality was generally negotiated among neighbors; this turned my attention to the cultural repertoire of soliciting strategies. One strategy I experienced resembled what Stodulka (2014) describes as “emotional economy”⁵; it is characterized by a strategic, though highly emotionalized, style of communicating needs, which draw their legitimacy from a dramatic urgency. These kinds of requests were almost impossible to decline. They usually would begin with an elaborate narrative of a heart-breaking story, interjected with (anticipated) praise for my generosity and kindness; toward the end they would culminate in a desperate appeal for “borrowing” money, to cover critical medical costs or avert eviction.

On the other end of the spectrum, I identified the much subtler, but nevertheless effective strategy of casual remarks. On several occasions, I had been explained the moral difference of asking for (*meminta*) or being given (*dikasih*) something. According to this differentiation, it is generally perceived as shameful to ask for any kind of material aid; though it is viewed positively to receive such assistance without having asked for it, because the receiver would feel recognized as a person in need (see also von Vacano 2014, p. 198). But, there are many ways not to ask for something, while still making the other person aware of one’s specific needs. Whenever an interlocutor went into details about a broken fridge, or, without context, started to lament upcoming school fee payments, I could not help but wonder whether this was an indirect pitch for my assistance.

Conclusion

Reviewing the economic-anthropological concept of reciprocity, I have demonstrated the conceptual limitations and ethical implications of a research ethic of reciprocity: imbued with the anti-market sentiments of gift exchange, the ethical framework of reciprocity implicitly prioritizes social over economic motives; constricted to a morality of equivalence and balance, it fails to address economic asymmetry. Both shortcomings perpetuate an idealized view of research relationships and, hence, tend to reinforce fieldworkers’ feelings of disappointment and guilt, rather than offering a practical framework for dealing with economic expectations and inequalities. Economic participation, in contrast, emphasizes the embedded nature of economic relations. Advocating for the active and conscious economic

⁵Writing on street-related young men in Yogyakarta, Stodulka (2014) describes the social technique of “relating to, emotionally bonding with, and ultimately coercing” (p. 8) people like him to provide them with essential items, money, or other forms of assistance.

integration of anthropologists into their field, it draws attention to the multiple practices of economic transaction which shape and produce our research relationships. These micropractices of economic participation can include the whole range of economic transactions that the local context provides; essentially, participation can operate on different moral grounds, from “exchange” to “hierarchy” or “communism,” as long as the moral expectations of researchers and interlocutors align. Yet, as a holistic approach, economic participation requires anthropologists to acknowledge the macro-structural context of their fieldwork. Given that most anthropologists conduct their fieldwork in research constellations of (stark) economic asymmetry, economic participation challenges researchers to deal with economic inequality. Referencing Graeber’s notion of “communism,” I have framed this challenge as a process of negotiating abilities and needs. Translating this ethical perspective into practical advice, I have introduced several strategies for depersonalizing this negotiation, from budgeting to an anthropological perspective on soliciting techniques.

Economic participation is a countermechanism to avoid moralizing when negotiating inequality. It does not necessarily imply personal detachment. As an encouragement to embrace the economic dimension of research relationships, economic participation recognizes the potential of economic transactions to create and sustain social bonds, friendships, and fieldwork rapport.

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Part III
Intimacy and Care in the Field

Intimacy and Care in the Field: Introduction



Leberecht Funk and Ferdiansyah Thajib

Intimate and caring relations are crucial to ethnographic fieldwork. Ethnographers engage in intimate and caring relations with their partners and families either when following them to the field or when left behind “at home.” Those being “researched” also care for their families, neighbors, colleagues, possessions, and landscapes. And more often than not, hosts care for ethnographers, and vice versa. Fieldwork entails the sharing of intimate information, built upon trust and rapport as foundational values of mutual respect. However, ethnographic fieldwork is not limited to positive feelings of intimacy and caring. It is also rife with affective and emotional dilemmas, when feelings of despair, disappointment, and frustration arise between researcher and research participants, as their intimate and caring relationships are intercepted by conflictual dynamics of power and status (Stodulka 2015). This introduction attends to the questions how affects and emotions related to intimacy and care influence ethnographic research, and how they shape ethnographers’ motivations, insights, and analysis.

Before drawing on the significance of intimacy and care through the lens of fieldwork experience, we first want to discuss some commonalities and differences between the two concepts. Both have been taken up by various disciplines, ranging from social anthropology to science and technology studies, as a topic of investigation (Haraway 1991; Probyn and Evers 2010; Roseneil and Budgeon 2004). This focus brings to the table a very broad outlook across different domains of life, especially those that are tightly bound to notions of relationality, family, sexuality, gender, and embodiment. While the concepts of intimacy and care are sometimes interchangeably used, in other contexts, there are small but significant differences in their respective reference. During the last two decades, care has become a key concept that draws attention to many fields of activity, among them issues of class, race,

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the life course, aging, work, kinship, education, and medication. The same holds true for the concept of intimacy, which operates in complex ways through ethnicity, identity, age, life course, spatial environments, and technological devices, as anthropologists and other social researchers (Giddens 1992; Jankowiak 2008; Stoler 2002) have been able to demonstrate.

The profusion of contexts concerning care and intimacy continues to pose analytical challenges: there are no overall definitions or ways of understanding these complex and fuzzy terms. Drothbohm and Alber (2015), for example, try to limit the scope of care by examining its interrelations with work, kinship, and the life course, while Nguyen et al. (2017, p. 202) define care as “processes of creating, sustaining and reproducing bodies, selves and social relationships.” Neither of these formulations is fit for our purposes, as we want to integrate “doing fieldwork” and “emotions” into our specific understanding of care. While we are not able to come up with more precise definitions of care and intimacy (nor are we convinced of the advantages of having them), we believe that a crucial aspect of both concepts consists in their reference to certain feelings of emotional attachment between the researcher and significant others within the affective settings of fieldwork.

To paraphrase Besnier (2015), who argues that the multiple meanings of intimacy signify both a conceptual weakness and strength due to the lack of agreement in the ways this category is used, we concur that the fuzziness of intimacy as a concept is a strength rather than weakness: it opens up opportunities for further methodological reflections on emotional aspects of fieldwork. This creative ambivalence is also a quality of the care concept that refrains from predefining the exact status of caregiver and care receiver. Within this process, it remains open whether the caring relationship is between parents and children, partners, lovers, friends, siblings, or co-workers. Indeed, one could even have intimate and caring relationships with animals, plants, things, and landscapes (Puig de la Bellacasa 2017). It thus seems apt to view intimate and caring relationships as affective manifestations of relatedness. People we feel related to comprise (biological) kin as well as nonkin with whom we have entered into intimate and caring relationships through fieldwork as an ongoing process of exchanging emotional warmth and supporting each other.

As a source of well-being, care is emotionally scripted by a feeling of responsibility performed by the caregiver toward those being cared for. Caring implies a relationship of hierarchy. This is especially true when the interactions between caregiver and care receiver are compounded with relations of power and domination. In parallel fashion, while intimacy is commonly understood as emerging from a more equal standing, it is important to note that this is by no means an absolute given. There may be intimacy between a caring parent and his/her child even though the generational difference between them points toward a hierarchical relationship. Since intimacy and care are embedded in larger social, political, and economic structures, they cannot be solely reflected on the level of interpersonal relationships. Normative and hegemonic disciplinary discourses about cultural ideas of intimacy and care, often camouflaged as ethical guidelines, strongly influence their meaning and related affective practices. When doing fieldwork and establishing rapport it is

thus necessary to reflect on research ethics as well as to analyze the power structures within intimate and caring relations. These political aspects call for a broader understanding on the ethical in research, especially where relationships between the researcher and research participants are constructed as mutually entangled vulnerabilities (Behar 1996; Detamore 2010).

Such notions bear particular methodological implications if we take them seriously as constitutive aspects of ethnographic fieldwork. In anthropological tradition, scholars have highlighted ethnographers' engagements through the establishment of intimacy with research interlocutors and informants (Smith and Kleiman 2010). The developments of close connections continue to be laudable goals for anthropological fieldworkers to such an extent that some anthropologists have coined the term "intimate ethnography" (Waterston and Rylko-Bauer 2006) to stress their methodological importance. Framing intimacy as both an intra-psychic and intersubjective process (Sehlikoglu and Zengin 2015), this perspective is inherently linked with debates on methods and knowledge in feminist and queer scholarships (Browne and Nash 2010; Moss and Donovan 2017).

As a focal point for discussion, intimacy in the field has stimulated a spectrum of ideas. Sexual relationships and erotic experiences in the field remain salient reference points of debate due to the systematic erasure of these topics in academic writing (Kulick 1995; Lewin and Leap 1996). Various accounts also look at how emotional bonds of friendship are negotiated during fieldwork and stretch beyond this spatiotemporal setting (Pitt-Rivers 2016; Taylor 2011; van der Geest 2015). Establishing intimate connections in fieldwork does not only mean gathering personal stories from research participants but also paying attention to the relational unfolding of affective atmospheres in research encounters and how fieldworkers make sense of them. Building upon the argument that intimacy is a relational process that unfolds across a range of emotions and spatiotemporal contexts, we see the potential to broaden the production of intimate knowledge. The analytical scope of these insights not only pertains to an experiential understanding of how the "self" manifests through one's intersubjective relationships with others in the field, but may also extend to the very structures that influence these interactions (Davies and Spencer 2010).

Talking about caring relationships while doing fieldwork puts forward a moral and sociopolitical dilemma that is widely ignored by academia. According to political theorist Tronto (1993), the fact that caring relationships are deemed to be a solely *private matter* can be questioned by an ethics of care approach. In her analysis, she casts a critical light on a complex value system that shapes public imaginings in many Western societies. She demonstrates that, within contemporary US society, care is mostly associated with *women*, whose place is supposed to be the *private domain* and who are thought to be more *emotional* than men (and thus closer to *nature*). Like Tronto, we believe that one of the reasons why the researcher's affects have, until our present time, only marginally been discussed within academia can be found in an underlying binary structure of values that ultimately rests in the dualisms of culture::nature, male::female, mind::body, private::public, rationality::emotionality, and so forth. The reproduction of these binary oppositions

remains a challenge to the subsequent developments within feminist and Marxist critiques.

By acknowledging the existence of caring relationships in the lives of field researchers, we cross the line between official academic work and private care work. Working for weeks, months, or sometimes even a year and longer on remote and potentially dangerous field sites, almost all of us face difficulties organizing our private lives. We leave people behind whom we should care for but are less able to do so, particularly when doing fieldwork abroad. Choosing to bring those we care for to our field sites causes new problems because of potentially burdensome processes of adapting to local environments and disparate infrastructural conditions. As field workers, we are not locked up in an office but physically, sexually, and emotionally present in the field (for better or worse). The messy materiality we come to deal with in caring and intimate processes—breastfeeding an accompanying child (Drozdowski and Robinson 2015), crying after splitting up with a partner, or moments when sweat and scent circulate (Stoler 2002)—tend to be excluded from our trajectories of data analysis, scientific fieldwork reflections, and ethnographic writings.

The three authors in this section, each representing different disciplinary backgrounds—geography, anthropology, and sustainability research—share a genuine interest in the incorporation of intimacy and care as analytical rubrics of fieldwork. Anthropologist Anna-Maria Walter examines her positionality and empathic participation during fieldwork in northern Pakistan through the local concept of *sharm*, which she translates as “female modesty.” She elaborates on the importance of intuition and embodied engagement. She highlights the advantages of having shared physical attributes with her local hosts and taking up a habitus of gender segregation and “female modesty” in her attempts to understand what others around her feel. At the same time, she understands that this process is not unilateral but always entangled with negotiation and interpretive authority. The development of intimate and caring relations in the field not only allows for her deep immersion in local lifeworlds but also impacts her personal life “back home,” to an extent that it even jeopardizes her marriage in Germany. Walter tackles the emotional complexity of fieldwork in her paper by laying open her struggle between the professional and personal dimensions of anthropological fieldwork. Indeed, as she argues, “No anthropologist stays unaffected when exposing oneself to new experiences” (this volume, p.147). In turn, her creative attempt to deal with her own changing subjectivities has enabled her to gain further insights into the lifeworlds of the people whom she spent time with during her field research.

Thomas Wimark, a social geographer, reflects on how sexuality is presented in different contexts of fieldworks (namely in Tanzania, Turkey, and Sweden) and interlinked with various emotions and spatial arrangements. In order to locate the intersection of bodily proximity, sexuality, and emotions, Wimark proposes a life-course perspective on emotions, which describes the different qualities between “feeling position” and “feeling experiences,” or in his own (Swedish) terms, *känsloläge* and *känsloupplevelse*. Through these two categories of affective positionality and experience, he illustrates how conceptions of sexuality are constantly

negotiated in research settings as they traverse along embodied dispositions, social contexts, and positionings, as well as personal biographies.

Last but not least, Janina Dannenberg, who has a background in sustainability research, conducted fieldwork in the rural Philippines. She explores her feelings of ambivalence toward the dominant understanding of ethnographic practice as an individualized endeavor which gives priority to professional engagement over domestic lives. As a wife and mother to three children, Dannenberg juxtaposes two stages of experience in the field: one when she was accompanied by family members and one without. Attaching the actual as well as the virtual presence of her family in the field to the notion of “normality,” she traces how multiple subject positions and emotional constellations are entangled in the way she engages in field research and makes meaning out of it. She points to the varying conditions where care work in family settings influences her relations with friends, informants, and interlocutors. By doing so, she rediscovers an intimate dimension of knowledge which had been mostly suppressed in normative structures of academia, highlighting the methodological bias of singling out the “lonesome ethnographer.”

As this overview demonstrates, intimacy and care in the field involve multiple nuances and operate across various relational contexts. The authors address important methodological issues related to intimacy and care in fieldwork that call for a dialog with departments and institutions catering for courses in ethnography and fieldwork training.

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Embodying Ineffable Concepts: Empathic Intimacy as Tool for Insight



Anna-Maria Walter

Introduction

In recent years, anthropologists have increasingly emphasized the importance of the physical body, affects, and emotions (Clough and Halley 2007; Gregg and Seigworth 2010; Howes 2005; Knudsen and Stage 2015). It is not only our interlocutors in the field who “feel” but also the researchers. At the height of the writing culture debate and postmodern critique, many anthropologists shifted focus from data analysis to testimonial accounts of their personal involvement (Kulick and Willson 1995; Okely 1996). Although this trend threatened to lose sight of the “researched other,” reflecting on the fieldworker’s role and positionality was established as an integral part of anthropological research (Barnard 2000, pp. 164–165). As knowledge is always situated (Haraway 1988), it is generally agreed that an account of the researcher’s subjectivity assists the reader in contextualizing the researcher’s involvement in the process of knowledge production (Pink 2009, p. 8) to create a—paradoxically—more “objective” argument.

Although most ethnographies are based on conversations, observations, and tacit aspects of participation that stimulate the researcher’s emotions, only a few academics disclose and dissect the sources of epistemological insights gained through the affective dimensions of fieldwork (Castillo 2015; Rytter 2015; Varley 2008). But what can we as participant observers learn from our emotions in the field? And in what way might reflections on empathic encounters open up new possibilities for enriching our understanding of ineffable local concepts? With the idea of ineffability, I aim at notions that are difficult to rationalize and articulate, such as emotionally charged, highly subjective ideas about love or decency. They lie beyond

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words, not because it is necessarily taboo to talk about them, but rather because they are difficult to articulate in linguistic terms (though this itself might to some extent reflect a certain level of taboo). Just as there is more to perceive than the visible, the focus on spoken words risks obstructing the view of more entangled and compound notions of emotions.

Anthropologists regularly struggle with semantic barriers and the variety of languages spoken in their field sites, which might result in multiple, difficult-to-discern epistemologies. Additionally, socially “correct” behavior is often embodied, tacit knowledge; thus, it is unconsciously enacted and eludes verbal description. Only major transgressions of decency are the subject of gossip. People do not need a guidebook for every possible situation to know intuitively what to do; instead, they are able to draw on a wide range of embodied behavior or habitus (Bourdieu 1977; Csordas 1990). Although the habitus concept explains why people act in certain ways on the basis of historic structures, it does not explain what they really think and feel, or how their behavior feeds back on them. Drawing on experiences from the researcher’s involvement with interlocutors enables an additional mode of communication (Howes 2005, p. 1).

My fieldwork in northern Pakistan was oriented towards the omnipresent—but never directly articulated—local concept of *sharm*, which can be translated as female modesty (Walter 2016). Based on examples of my own positionality and empathic participation, I argue that we can only apprehend our protagonists’ perceptions and emotional cognition when we dare to make use of our own embodiments of local culture and the feelings that arise from them. Many anthropologists who work on topics requiring supposedly less intimate involvement, such as development schemes or infrastructure projects, omit ethnographic reflexivity in a show of artificial professionalism, which Varley refers to as “quiet political correctness” (Varley 2008, p. 134). The contrasting approach taken in this chapter should not be viewed as simply a confessional account of my experiences in the field; instead, it is the exposition of a strategy in which the researcher herself is an intersubjective methodological tool for empathic communication.

Researching Emotions

Who’s the girl with you?

Oh, she’s one of Rubina’s relatives from the village.

Curious eyes turned towards me, parents of young adults of marriageable age assessing my exemplary shy posture sitting at the back of the group of women that I was accompanying. As the whole room’s attention was on me, I fastened the veil even tighter around my face and kept my head tilted down. My Gilgiti friends proudly presented me to their neighbors and enjoyed their inquiries about my family. “Yeah, yeah, from the very last village in the valley,” they added to explain the fact that no one recognized me. When I answered in a local Shina dialect, my friends

burst out laughing and had to confess their joke. Astonishment usually followed: “No, that’s unbelievable. She looks even more *shermāṭī* (shy, modest) than our girls.”

Much of what I did during my fieldwork—investigating the appropriation of cell phones in the area of Gilgit—I did intuitively, without having much time to contemplate how to react in certain situations. Living with local families and the unease I felt as a foreigner in public demanded a quick enculturation into daily life, which meant abiding by strictly defined gender roles and blending in as thoroughly as possible. I tried to avoid posing a threat to my hosts’ reputation through any kind of misbehavior, whether in their social circles or in public where I feared attracting the attention of Pakistan’s unpredictable secret services (see Grieser 2016).

Starting my fieldwork, I soon realized that Gilgiti women are reluctant to discuss research questions in a formal setting. Especially when I met a girl for the first time, conversation stayed on a very normative, impersonal level. However, for the purposes of my research, I needed to establish a genuine and intense involvement with my interlocutors. When working on sensitive topics, long-term and collaborative aspects of participant observation are especially suitable for slowly building trust and rapport (DeWalt and DeWalt 2002, pp. 40–41). Anthropologists depend on their interlocutors’ willingness to disclose, to allow the researcher to “sense” something. From a plain and interchangeable façade, no one can read or learn anything. But empathy is not a one-way sentiment. I therefore argue that empathy should be regarded as a form of communication. By being open and attentive, and willing to adapt to local conventions, I was quickly accepted, taught, and integrated into the daily lives of my Gilgiti “sisters” and “aunties.” For me this meant taking up a passive female role in public as well as sharing very private details in women’s circles. In other cultural contexts, the practice of empathy might take on different shapes. Empathic encounters depend on the researcher’s willingness and ability to expose herself to other ways of life, engage with normative behavior, and let herself be “impressed.” That my physical looks coincide with local girls’ complexions had the advantage that I did not disrupt people’s manner through my presence; tall, blond women are immediately identified as outsiders no matter how well adapted they are (Fig. 1).

In participant observation, anthropologists—often unconsciously—utilize a model of empathic embodiment: we are influenced by our material surroundings; we “sense” the social setting, adapt to daily life, practice different movements; we are affected by others’ actions and emotions; and we “feel” our field (Ingold 2011; Pink 2009; Stoller 1997). Csordas’ axiom of “being in the world” (Csordas 1994), deriving from Heidegger’s *Dasein* (being there), perfectly captures the “organic” character of our engagement with our environments. Against the conception of a private, closed-in subject confronting the external, public world, I argue that our personalities neither plainly exist within our inner selves, nor are solely created through public interaction, but rather develop within the net (Latour 2005), meshwork (Ingold 2011), or grid of life.



Fig. 1 The author among interlocutors/friends/“sisters” in Gilgit-Baltistan; photo: Anna-Maria Walter

Following Ingold (2006), I argue for the perception of human beings as continuously produced through the combination of all their—conscious as well as unconscious—experiences, the embodiment of discursive representations, and the interpretations of their engagements with others:

Like organisms, selves become, and they do so within a matrix of relations with others. The *unfolding* of these relations in the process of social life is also their *enfolding* within the selves that are constituted within this process, in their specific structures of awareness and response—structures which are, at the same time, embodiments of personal identity. Thus, personhood is no more inscribed upon the self than it is upon the organism; rather, the person *is* the self, not however in the Western sense of the private, closed-in subject confronting the external, public world of society and its relationships, but in the sense of its positioning as a focus of agency and experience *within* a social relational field. (p. 187; emphasis in the original)

This holistic view discards strict separations between notions of social identity, biological organism, and a more psychological conception of the self, and questions the more conventional understanding of personality or character as the combination of the mental and moral qualities of a person that makes up her distinctive and consistent character (Oxford 2017). Instead of blindly believing in a “naturally” given entity, I stress a person’s incessant remaking, based on the entanglement of her in social and material environments and the interaction with organic preconditions and influences. Examples of my own journey through fieldwork will underline a person(ality)’s constitutive enmeshment in life.

As people always have to relate to each other, they do so through empathic emotions, which can be any kind of emotion, even of the “negative” kind. What Engelen

and Röttger-Rössler (2012, p. 4) call “grasping” describes the process through which people read what the other is thinking, comprehending what she is feeling, and then relate to it. Halpern (2001), a psychiatrist, recognized that insight into others does not derive from distant, supposedly objective third-person observation but from first-person experiential perceptions that allow one to imagine how one would feel in the other’s position. Here methodological objections arise: how do we know what others feel or think? I am sure no one ever fully knows how another person feels; we are not even certain of our own sensual, emotional, and cognitive processes. Nevertheless, there is much anthropologists can contribute to the discussion on empathy: cultural background knowledge gives us the ability to contextualize why a person is, for example, angry. Understanding why someone is feeling or behaving in a particular way certainly facilitates the recognition of the other’s affect (Hollan 2012, p. 71). Another critique of empathy’s credibility might be a lack of mutuality; research situations are always loci of asymmetry, involving an educated outsider studying people who are different and “exotic.” It is not my aim to dismiss any such concerns, but rather to acknowledge an always present, immanent negotiation of power and interpretive authority.

Female modesty, for example, became clearest to me through corporeal experiences. Out of respect, women and minors never stretch out their legs in the presence of male elders. Consequently, whenever the head of the household or other men were present at home and sat in the same room with me, I kept my legs folded, which occasionally resulted in knee pains, an ailment that especially older women in Gilgit-Baltistan frequently suffer from: patriarchy physically tangible. Habit(u)s and behavioral norms might easily be replicated, but their emotional implications, the underlying ineffable concepts, are more complicated to access and comprehend. To achieve this, I had to let myself get “affected.” Cultural categories and ideas are manifested through performances and the discourses surrounding them, while at the same time feeding back on the protagonists. Embodiment goes beyond mere simulation; performance stimulates bodily sensation and imprints itself on the mind. This way local norms gradually become incorporated and part of oneself (Butler 1997, 2015; Walter 2016). Similarly, emotions are triggered through empathic expression: When someone is sad, we mirror her sadness with similar facial expressions and bodily postures; through this adjustment, we also become sad (Niedenthal et al. 2005, p. 32). By taking up Gilgiti women’s *sharm* habitus, I experienced the cognitive effects of bodily conduct and feelings firsthand. Once, when I walked into a room where a newlywed couple exchanged looks of affection and sat a little too close together for a public space, I caught myself feeling embarrassed and blushed instantly, although I would never have felt uncomfortable in a similar situation in Germany. Empathic embodiment, thus, has the power to collapse the dichotomy between an expression of interior motives and the incorporation of an observable behavior; subject and object or body and mind thereby become indistinguishably intertwined.

Of course, we cannot, nor do we wish to, completely discard our own personalities. An anthropologist’s perception is shaped by her upbringing, education, and social background, but it nevertheless is deeply affected through her immersion

(Davies 2010, p. 80). Having performed for more than a year the passive, downcast posture with spiritlessly dropping shoulders that is typical of local young women, I noticed how my attitude changed. Even now that I am back home, it still comes back to me when I put on Pakistani-style clothing. Rytter describes a very similar process of embodying his research topic: after engaging with followers of a Pakistani Sufi master over a long span of time, he could no longer differentiate a dream from a vision. Thus, in the ontological framework of his interlocutors, their Sufi has reflected God's light on him (Rytter 2015). The notion of reflection beautifully emphasizes the relational character of ethnographic engagement, capturing the idea of learning from others on a much deeper cognitive level than by simply copying their behavior. By recognizing shared feelings, the social, personal, and epistemological distance between the anthropologist and her interlocutors breaks down (Castillo 2015).

Often, empathically experienced emotions are left out of ethnographic accounts, as they seemingly cannot be validated, fall prey to scruples about "going native" (Geertz 1984, p. 124), or are regarded as too personal. However, as we researchers endeavor to convince our interlocutors to disclose their life trajectories, opinions, and feelings, it is only fair for us to lay bare our own entanglements as well. Instead of striving to shed an inevitable dimension of emotional subjectivity from our work, we should use these insights to contextualize ineffable moral concepts. A good example is Varley's very private and open account of her marriage into a Gilgiti family, which she uses to demonstrate how she evaluated protagonists through biased local prisms and personal (dis)regard (Varley 2008, pp. 142–145).

I argue for a holistic approach in which we use our whole selves, our bodily perceptions, feelings, and embodiments as instruments that facilitate insight into others' perspectives. Local knowledge and cultural meanings transpire through openness, sensibility, and intimate involvement, and thus gradually influence one's own emotions and perceptions. We can purposely direct our attention to sensations, "as part of a broader cultivation of capacities for relational attunement and nonlinguistic, somatic modes of communication" (Chari 2016, p. 228; see also Csordas 1993). Reflecting upon local concepts of shame and modesty, the following section will examine the complex interdependencies between empathic experiences and one's self in the process of knowledge production.

Intimate Connections in the Field

When my good friend Ali confessed his love to me, I realized that something had gone very wrong. "She [his fiancée] is my obligation, you are my love." I was dumbstruck by the words appearing on the screen of my mobile phone. What have I done that could be so misunderstood? When I was in Pakistan for a preliminary excursion for my PhD project in 2011, I traveled around and interacted with people in an ill-prepared and naïve manner, treating local men as I would do at home. Going on sightseeing trips and drinking alcohol together clearly sent the wrong

messages—messages that Ali interpreted as indication that the “easy,” “western” girl had singled him out. The lack of adaptation to local moral concepts obviously gives rise to misunderstandings and prevents deeper insights.

I took a different approach when I returned to Gilgit-Baltistan for my actual fieldwork, which was conducted between 2013 and 2015. Being more conscious of local norms and values, even though many of them are rarely directly articulated, taking up the habitus of gender segregation and female modesty happened rather quickly.

Being married myself had many positive implications for my fieldwork. It certainly strengthened my role in Gilgiti households: as a married woman, I was not a threat to the good reputation of the local families with which I stayed or was associated. After my wedding and for the period that I remained married, no man took the liberty of transgressing any boundaries; they did not see me as a potential mate and treated me with respect. And most importantly, I was a full member of adult women’s circles, privy to the gossip and intimate talk surrounding their relationships with their in-laws and husbands. When executing their daily chores in the household together, females exchange news, gossip, and talk about their lives. Only at first sight does it seem that all women behave with exemplary modesty; when sufficiently adapted to the local setting, I noticed many little acts of everyday resistance against established authorities that stretched normative boundaries (Scott 1985). Deliberately impudent or pert behavior is instantly apologized for through overt giggling and hiding behind each other’s backs or underneath the veil; by masking it as an accident and expressing regret, girls are able to prevent elders from scolding them for such behavior. Young women take advantage of the absence of their parents to go to the market, though never alone; they always travel in a group of girls, but without a proper chaperone.

The ways in which individuals implement established social norms also depend to a large extent on their individual personalities. One woman may be more daring and forward, while another may be reserved and more prone to shame when committing social mistakes. *Sharm* is often connected to a woman’s confidence: when she is settled in life, married with children, and comfortable in her personality, she will be less likely to feel threatened by exposure. Although Shia and Sunni women avoid music and dancing in public, many dance in private, playing local or Bollywood songs on their cell phones. Watching Indian soap operas and movies is also a popular activity. A warm, caring atmosphere exists among women: they enjoy physical closeness in acts of personal hygiene, massaging their heads, and looking for lice. Additionally, they exchange dirty jokes, and some women enjoyed teaching me the filthiest words in the local language, Shina. When things get lively, they romp around or put on “western” style clothing to highlight their bodies. Of course, these gatherings also provide room to discuss questions about sexual matters, such as the frequency of intercourse with spouses and how to increase or decrease it, or about experiences with controversial practices and whether these are allowed in Islam.

As unmarried women are not conceded much sexual knowledge, this intimate form of participation would never have been possible had I not been officially married. Local friends often curiously asked me about the nature of my conjugal rela-

tionship and commented upon the cultural differences: how do your parents-in-law treat you? When you go home, will you kiss him at the airport? What kind of ablutions do Christian women have to perform when they have their period? And most importantly, was it a love or arranged marriage? Many questions forced me along a tightrope walk, as I wanted to be honest to them but not shock or offend anyone with answers that were too blunt. I thus continuously had to culturally “translate” religious, moral, and other concepts and contextualize German ways of life, such as living with boyfriends and separately from in-laws. These interactions were extremely interesting and most valuable for my research. Due to this excellent access, I allowed the topic of my research to slightly shift its focus from mobile telephony’s impact on gender relations to recent discourses on conjugal relationships and love concepts, which often circle around the use of cell phones.

Being so deeply involved in Gilgiti family life left a mark on me. The accumulation of embodied knowledge of local cultural meanings gradually influenced my own emotions and perceptions. This also had an effect on the relationship with my husband at home in Germany, with our role allocation becoming more conservative. I wanted him to be more romantic and to take care of me, whereas he missed my former independence and self-confidence. Davies notices a sense of loss in anthropologists, the loss of one’s taken-for-granted interpretations of life, or as Sax (2014, p. 15) more positively puts it, the “intuitive somatic and moral universes had not been replaced; rather, they had been expanded”. Many fieldworkers describe feelings of dissonance after returning home; their thoughts are confused, and they realize that they might not fit into the “mold” that they originally left (Davies 2010, pp. 88–89). Unfortunately, being separated for about a year proliferated differences between my husband and me and drove us further apart. He could not reconcile himself to my altered personality, and I felt unable to bridge the widening gap, to successfully communicate and accept the way we had both changed in each other’s absence as a result of our different experiences. Thus, when I returned to Pakistan after a 6-month break, I had to communicate that I was getting divorced.

As divorce is a legal option in Islam and most Gilgitis proudly portray themselves as strictly following Islamic principles, my interlocutors reacted less disapprovingly than I had expected. Young girls were the sharpest at investigating my motives, repeatedly questioning me and having trouble accepting that their romantic illusion of a love marriage is able to fail. Older generations had their own answers more readily available: no husband would allow his wife to leave him and stay faithful for so many months; it did not occur to them that the separation might have been induced by the wife as well. Although I always tried to describe the development of our relationship correctly and take the blame on myself, their own logic made more sense to them. Some pitied me, and some admonished me to try to win him back for the sake of my social respect.

In Gilgit-Baltistan, marriage is not necessarily the site of romance; a secure and comfortable life is more important. Thus, it is advisable to guard one’s heart against the emotional vulnerability that can come either from falling in love with an unattainable person or from having a partner whom one dislikes. Women intentionally downplay their own empathic abilities with regard to men, and often also in

relation to women who threaten to allow their feelings to become too intense and over which they risk losing control. I was often surprised by the lack of comfort women gave to each other, even to little girls, until I realized its connection to emotional discipline. De Waal (2009) talks of a “turn-off switch” for regulating one’s empathic connection and response to others. This precautionary mechanism makes perfect sense when the fulfillment of one’s dreams is neither possible nor desirable.

My position as both insider and outsider in the families certainly aided the acceptance of my new marital status. Had they fully associated me with their families, they might have felt embarrassed by the scandal of a divorce. However, as I was a foreigner with a parallel, distant life, my separation provoked curiosity, needed to be accepted and made the best of. While many of the women of the local families were worried about getting me married again so I could have babies before I got too old, one or two men decided to try their luck with me. Although nothing happened, their verbal infringements were unexpected, as I felt I was one of their “sisters” (a term that also applies to distant “cousins”) who was well adapted to local customs, dress, and modesty. Despite widespread accounts of Pakistani men’s imaginations of the sexually easily available “western” women, I had so far only been confronted with the dubious chat-up lines of strange men on rare occasions when traveling alone. The approaches made by men from my host families felt completely different, and I experienced them as very uncomfortable betrayals of my trust. The worst aspect of it was that I understood how vulnerable women might actually feel in situations of harassment within patriarchal structures: in order not to destroy peace in the extended family and to avoid hurting the feelings of other women and creating distrust, they cannot tell anyone, and have to hope that their defenses are sufficient to keep them safe from men’s transgressions. Most importantly, they will intuitively search for the mistake they made that encouraged the men and will fear criticism by others for having done so. If men cross a line, women feel partly responsible, just like I did, for not having fended them off strongly enough; being too polite and failing to be more determined in warding off an approach is readily interpreted as giving men the wrong signals. As I could not persuade one particular man of his wrongdoing, I had to swallow my embarrassment, accept prevailing power structures, and convince myself that these were opportunities for insight that should not put at risk, but rather increase my empathy for my friends in Gilgit-Baltistan.

As described earlier, the relationships with my female interlocutors were characterized by their warm-hearted manners and the openness with which they repeatedly received me; the intimacy between us included sensuous experiences and private conversations. Many times, my sleep in Gilgit was interrupted by something lighting up and vibrating on our thin mattresses on the cold floors. The continuous “beep, beep” did not seem to wake up any of the other girls in the room, but somehow my young friend Noshīn recognized the sound of her mobile phone and excitedly dived for it from among her cousin’s bedding. The message caused a smile to spread over her pretty face. As she noticed me watching her, Noshīn crawled closer to me and pulled the blanket over our heads to share more details. “I swear I miss you, dar-

ling,” she translated for me, but it soon got too hot and suffocating under the heavy, synthetic blanket and she promised to show me more messages in the morning.

Ultimately, the boundaries between my private and professional self dissolved entirely: I found myself so involved in my fieldwork that I myself fell in love via the mobile phone. Being entangled with my local friends’ lives, as well as delving into theoretical considerations of shame, honor, love, and other emotions in South Asia, I became a subject of my own research. Although I met my boyfriend through mutual friends in Islamabad, we really got to know each other across vast distances—I was first in Gilgit and later in Germany—using SMS, WhatsApp, and Skype. After only a few weeks, we were madly in love. I am not sure whether these feelings would have developed over such a restricted communication channel in a different setting.

I had wondered for many months how so many of my local friends fell in love with their arranged fiancé(e)s over their phone; now, I was experiencing a very similar process, with the only difference being that our relationship was not predestined to lead to marriage. Disclosing one’s thoughts without anyone knowing and sharing intimate matters creates a secret bond, trust, and loving affection. When writing about my own emotions now, it seems my words do not live up to the actual experiences. Something gets lost when putting feelings into words. Comprehension is more than verbal articulation. The skill of the anthropologist is to metaphorically translate these embodiments and explain their value; a task I struggle to live up to in the analysis of women’s emotional perception of marital relationships and changing love concepts in Gilgit-Baltistan (Walter 2018).

Secretly having a love affair—even if it was only a virtual one in the beginning—took me to the limits of my cultural adaptation. I was caught between two stools, craving to text my boyfriend continuously while retaining a sense of guilt because exchanging phrases of intimacy and virtual caresses was highly indecent by Gilgiti standards. So, I did what women who chatted with their fiancés or husbands did: I instantly erased our messages and maneuvered my way out of situations when friends were asking me about my future plans or when I felt I had been distracted by my phone for too long.

In retrospect, I can discern strategies of “withdrawal” (Davies 2010, p. 84–85) in this period of my fieldwork. In my extensive use of the phone to connect to a different place and to someone else, I forced the device and the connection it stands for between me and the field, bringing the influence of my immersion in Gilgitis’ lifestyle to a relative halt. In these last months of my fieldwork, the phone returned a sense of agency to me; I rediscovered the traits of my older self and wanted to pursue my own interests—interests that were not fully compatible with my role in local families. We combine multiple identities within ourselves; they can coexist, overlap, and partly contradict each other at the same time (Sökefeld 1999). Although I was bending the rules by secretly being in touch with an unrelated man, the cell phone offered a channel of communication that felt acceptable to me in the local framework.

However, my refusal to share who and what was making me smile when reading these messages already felt like a betrayal to my gracious hosts. The code of hon-

esty is different in Gilgit-Baltistan though. One of my best friends, Aliya, had always stressed to me that her parents do not have to set strict rules for their children because they can “trust” them. As she resorted to the English term, I understood that this was a new concept for her. The absence of privacy traditionally served as a form of social control but now the virtual space of mobile phone connections delivered an invisible meeting place that cannot easily be monitored by others. Interestingly, Aliya emphasized trust, although she herself had developed a love relationship. It shows how confident she is in the decency of her behavior—she had never polluted herself with any physical intimacies and managed to convert her romance into an honorable relationship. By choosing a desired spouse from the circle of potential candidates within the extended family network and maintaining more or less “innocent” communication with him of a type that she could have had with any cousin, she perfectly aligned new ideas of romantic love and intimacy with kinship solidarity, Islamic teachings, and her own interests. Nonetheless, she would never have told her parents about the pre-wedding existence of emotional attachment. She knew they would consider it highly indecent, and keeping it concealed allowed her to push the boundaries of the acceptable framework a little further. Preventing exposure might not necessarily protect one from gossip, but it helps to keep one’s conscience clean. Although I cheerfully collected stories like hers, which taught me how different epistemologies are enacted and how a certain line of truth is established through performance, I could never quite feel content with this kind of maneuvering myself. Despite all my empathic openness, I might not have been enculturated enough after all.

Conclusion

With this very private account of some aspects of my fieldwork entanglements, I hope I have been able to demonstrate how personality and biography of the researcher influence a study’s direction, while at the same time being influenced by the social environment and encounters in the field. No anthropologist stays unaffected when exposing oneself to new experiences, the methodological reference to empathy identifies this adjustment and alteration of the self in favor of the ‘other’. Indeed, we want to be “contaminated by our encounters” (Tsing 2015, p. 27),¹ and I argue that by creatively using these changes in ourselves, our thoughts and emotions, we can gain further insights into other people’s social and cultural concepts. Through our body we can overcome ineffability; therefore, we should learn to be attentive to these experiences and reflect upon them. I am conscious of the fact that I am laying open intimate details of my own life, and I am confident in doing so, because that is exactly what we anthropologists expect of our interlocutors when we

¹[Encounters] change who we are as we make way for others. As contamination changes world-making projects, mutual worlds—and new directions—may emerge. Everyone carries a history of contamination; purity is not an option.” (Tsing 2015, p. 27)

use their most private thoughts on ethnicity, identity, gender, and emotions to acquire and extend anthropological knowledge and our understanding of the social world. With openness, sensibility, and empathy in participant observation, we are able to sense many aspects of our fieldwork's social environment and gradually embody local concepts, values, and morals.

This expertise develops especially around our research interest and question. I was, for example, hardly ever confronted with the electoral politics of Gilgit-Baltistan nor did I have to position myself in that regard, so current governmental discourses did not leave a lasting mark on me. Researchers who work on political parties might, correspondingly, be less influenced by matters concerning the social politics of modesty and marriage, whereas I was constantly confronted with practical, normative ideas of modesty as well as moral, Islamic, and academic reflections on them. The continuous performance of modest behavior quickly affected my inner sense of decency and my expectations of appropriate behavior, with the result that I unconsciously aligned my subsequent behavior accordingly. The embodiment process reinforces itself (see Walter 2016) and transcends long-standing dichotomies, such as nature and culture or private and public, offering an inclusive perception of individuals' agency which is neither determined by, nor free of, either of these multiple aspects; rather, they mutually inform each other. Life's entanglements are not unilineal, and we are always confronted with stimuli from various directions; at the same time, we take an active part in this process of becoming. We can only comprehend these complexities through genuine reflection on our own personal involvement and emotions that derive from interactions in the process of fieldwork; this goes hand in hand with and informs any other data analysis. Through affective involvement and empathic participation, researchers have the chance to use their own embodiment of local culture to perceive and gain insight into others' worlds.

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Sexuality and Emotions Situated in Time and Space



Thomas Wimark



Fig. 1 Walking hand-in-hand in Dar es Salaam. Photo: Mari Thorildsson

I was deeply ashamed and felt how the blood rushed to my face. He held my hand so tightly that I could not get loose. I panicked and tried to shake my hand to get loose without making a scene, but he did not respond in the way I hoped. Instead, he held my hand tighter. (...) (Field diary)

The picture above depicts John and me as we walked down the street, seeking shade under the trees in Dar es Salaam, Tanzania. To John, who was assisting me

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during fieldwork and has no reason to define himself as anything other than heterosexual, this situation represented nothing out of the ordinary: just two friends walking hand-in-hand to their destination.¹ For me, a privileged cis-gendered white gay-identified man conducting research in geography; however, the same situation was an excruciating experience during which I feared being exposed as gay in a country that criminalizes homosexual acts. Approximately a year later, I was in the field once again for a different project, but this time I was in Istanbul, Turkey, where homosexuality is not criminalized, although it is socially stigmatized. In Istanbul, I met a man, Ramazon, who also assisted me during my fieldwork and defined himself as gay. When he was guiding me through the city, he brought me to Taksim Square, a known location for gay hook-ups. On the way there, he took my hand, and we walked hand-in-hand along the street. This time, I wrote the following:

Since the Istiklal [street] was packed, and we did not want to lose each other, we walked near each other. In this confusion, he reached for my hand, and we continued hand-in-hand along the street. I had a bit of a flashback to Dar es Salaam, but it felt different this time. I don't stick out here, and I can blend in with other people. At the same time, it felt a bit uncomfortable, since I didn't know how it would be interpreted by others on the street. Do I really blend in, or does something reveal me? (Field diary)

Back home in Stockholm, Sweden, where homosexuality is neither criminalized nor especially stigmatized, I was out one night with my then Latin American partner, Oskar. On the subway on the way home, I tried to take his hand but he pulled away, making me feel annoyed and irritated when he said, "Not here." These vignettes highlight the situatedness of sexuality and illustrate that sexuality and emotions are both contextual and spatial.² All three situations reflect how my conception of sexuality conflicted with the conceptions of others. In the Swedish context, I have learned to connect adult men holding hands with emotions pertaining to sexuality. Young boys can hold hands, but as soon as they reach their teenage years, holding hands becomes increasingly connected with sexuality, and by young adulthood, it is generally associated with sexuality. Holding hands between men in Tanzania is conceived differently, and affection among men without sexual connotation is common. Young adults and adult men holding hands in Tanzania can be a simple gesture of friendship. In preparation for conducting fieldwork in Tanzania, I had read and heard of men expressing friendship by holding hands, but once in the field, I realized that this is something that they do and not an activity in which I could participate. At the time of the incident described above, I connected holding hands to sexuality, which led to my fear of being exposed, while John connected holding hands to emotions of

¹All names in this paper were changed to ensure some degree of anonymity.

²Throughout the text, I use the term context to denote the hegemonic social and cultural conditions that apply in a certain space and time. As many before me have noted (e.g., see Duncan and Ley 1993), a single culture cannot easily be fixed to a certain space and time, and several different cultures can exist at the same time, albeit in a hierarchical power system. Cultures are also not simply internalized by all individuals in a certain space and time but are contested, negotiated, and discontinuously adopted.

friendship. In Turkey, affection between men is likewise common, and holding hands does not necessarily have any sexual connotation. However, depending on the situation and where one holds hands, the interpretation can vary. In some areas in the larger cities, for example, holding hands can be interpreted as sexual, while the same act in other areas might presumably be an act of friendship. The situation in Turkey was thus confusing for me, because the emotions of both friendship and sexuality could be felt, especially as I knew that Ramazon also desired men. In the third example, my learned way of connecting emotions with sexuality conflicted with Oskar's experience, who had learned to associate acts such as holding hands with danger. In Ecuador, holding hands in public is generally considered an act of intimacy, and adult men do not generally hold hands unless showing sexual affection. Although homosexuality is becoming increasingly accepted, the society is still permeated by homophobia and violence towards LGBTQ individuals. Holding hands is therefore connected to the risk of being a victim of violence.

In this chapter, I reflect on the way we display sexuality in different contexts and how emotions are connected to that concept. In the first section, I draw on previous work from geographical scholars discussing the importance of space for sexuality. In this section, I argue that the previous research has generated important insights into how space matters for sexuality, while simultaneously failing to discuss sexual *emotions* in the field. In the subsequent section, I discuss recent research from emotion scholars in geography and offer an approach for analyzing emotions in fieldwork. Next, I draw on my fieldwork experiences to illustrate how this concept can be employed to include structure and context without losing the subject in the analyses. Finally, I discuss how this approach can contribute to current research on sexuality and fieldwork.

Sexuality and Emotions in Space

It is now widely recognized that space matters when researching sexuality. Geographical scholars have produced considerable research discussing the relationship between different spaces and sexuality. Key thinkers, such as Bech (1998, p. 219–221) and Hubbard (2013, p. xii–xiii), have made compelling arguments that sexuality and cities are inseparable. Cities “are known as sites of sexual experimentation, radicalism and freedom, as places where individuals can pursue or purchase a rich diversity of sexual pleasures” (ibid., p. xiii). Thus, it is not surprising to discover that urban spaces are also equated with non-heterosexual identities and practices (Knopp 1995). A rich literature exists illustrating non-heterosexual identities and practices in cities (e.g., see Bell and Valentine 1995; Browne et al. 2007; Ingram et al. 1997). More importantly, specific neighborhoods within cities have been highlighted and often referred to as gay enclaves (Reed 2003; Sibalis 2004). However, the relationship between other types of spaces and sexuality has also been scrutinized and highlighted by scholars (e.g., Shuttleton et al. 2000). Eroticized rural spaces may not be the norm, but they exist (Bell 2000), and non-heterosexuals can

also be represented in such spaces (Kramer 1995). Migration waves of non-heterosexuals from small towns and the countryside to larger cities occur (e.g., Weston 1995; Wimark and Östh 2014), but such waves do not represent the only form of queer migration (Gorman-Murray 2007, 2009).

Much previous research has focused on sexual identities and practices in the West. Consequently, sexuality has become associated with Western identities and practices. Critics have argued that this diffusion of Western identities and practices obscures the vast number of ways that sexualities are played out and experienced in other parts of the world (Altman 2000; Oswin 2006). To a certain extent, this bias has been rectified by later studies that have explored sexualities outside the West (see Oswin 2005, 2010; Wimark 2016a). However, there is a lack of stories from non-Western contexts, and the association between sexuality and sexual identity/practice appears to persist within the field. Such gaps do not aid researchers endeavoring to conduct fieldwork in different parts of the world, if these researchers, in planning their fieldwork, wish to determine what relationships they should expect between space and sexuality. Furthermore, even less research has been conducted on the relationship between the emotions of sexuality and space in fieldwork, which makes it more difficult for researchers to establish expectations and design a research process. To some extent, this uncertainty is desirable within qualitative research design; however, for less senior researchers, it can result in a difficult process and can, for example, lead such researchers to abandon their field plans.

Although *sexual* emotions have not been explored to any great extent, there has recently been an increasing number of geographical analyses of emotions, as noted in the next section.

Emotions in the Field

Within the geographical field, an increasing number of studies have been conducted on emotions and affect. As several scholars have noted (Pile 2010), there has been a key debate between scholars of affect and scholars of emotions. The main difference between the two groups lies in how they conceptualize subjects in relation to affect. Non-representational theory perceives the subject, or the body, as a mere object for affect that is flowing through different bodies, rendering affect non-cognitive and unattainable (see Anderson 2006, p. 735). For emotion scholars, however, affect is attainable by the subject through expressed emotions (Bondi 2007). Furthermore, for emotion scholars, affect or emotions should be understood as the lived experience of interactions between individuals, rather than as something internal to subjects (Bondi et al. 2007, p. 3).

A common trait of both sets of theories is to deny the existence of any biological determinant of affect and emotions. Instead, emotions and affect should simply be understood as memories or histories of encounters between subjects (see Bondi 2003, 2005, 2014; Cupples 2002; Widdowfield 2000; Laliberté and Schurr 2015).

In my recent work, I have criticized this perspective for ignoring the differences that exist between subjects (Wimark 2016a). In this work, I draw on life course theory and propose conceptualizing emotions through the Swedish word pair *känsloläge* and *känsloupplevelse*, thus distinguishing between “feeling positions” and “feeling experiences.” In this vein, *känsloläge* denotes a unique cognitive capacity that enables and disables *känsloupplevelse*. Subjects’ *känsloläge* develops over time in various directions and depends on both the situations they experience in life and the biology of their bodies. This implies that a child has an entirely different set of *känsloläge* than an older person, in addition to the different attributes these two people possess. As a subject matures and experiences the world, his or her *känsloläge* changes, for example, some place-specific norms and regulations are overcome as new norms are simultaneously added. As an individual’s *känsloläge* changes, new sets of emotional experiences unfold, while others are kept, developed, or closed. This perspective enables the inclusion of bodily differences in a productive manner. Thus, subjects are situated in their own specific time and space.

Känsloupplevelse represents the feelings that we perceive in a specific moment in time and space. In these situations, structural constraints and enablers, for example, from groups or networks, compel subjects to express their emotions within the limits of emotional regulations. However, a subject’s *känsloupplevelse* is not entirely determined by the rules and regulations of a specific social system; it is also determined by the distinctive *känsloläge* the subject has developed. This combination results in an understanding that embraces both the bodily subject and the social structure, and which centers the subject without making it the sole owner of the emotion. In the following, I use this concept to make sense of the fieldwork vignettes described earlier.

Fieldwork Through *Känsloläge* and *Känsloupplevelse*

How then should sexuality and emotions be understood in fieldwork according to this perspective? The first vignette illustrates that sexuality is conceptualized differently in various parts of the world. We may speak of a “global gay” identity, as discussed in the previous sections, but this identity has not replaced other conceptions of sexuality. As noted by several other scholars (Cardoso 2009), the concept of sexual identity has neither diffused to all contexts in the world nor become widely accepted. A common means of conceptualizing sexuality is through strong gender roles with a dominant male role. A man is expected to take the active role in sexual encounters and to penetrate a woman. He can also choose to penetrate a man without losing his manhood as long as he is the penetrating subject. However, the man who is penetrated loses his manhood and is considered a woman. In Tanzania, there are similar ways of conceptualizing sexuality (see Moen et al. 2014), which render same-sex affection between men, such as holding hands, perfectly acceptable without any sexual connotation as long as neither of them acts as a woman. This finding signifies that a connection between same-sex desire and holding hands would be

unthinkable for John. For me, however, the acts of holding hands with a man and having sex with a man are connected through the concept of sexual identities, in which same-sex affection connotes homosexuality. My memories of walking hand-in-hand with previous sexual partners while in love influenced my *känsloläge* such that this connection was undeniable, which caused me to panic and blush. However, this situation was further complicated by structure and race. Because I was aware of the laws in Tanzania, I had taken precautions to avoid becoming vulnerable. I did not, for example, disclose my marital status to other men, and I evaded questions about women and partners. In this situation, my race made it impossible for me pass as a local, someone who conceptualized sexuality differently from sexual identity. Although another conceptualization of sexuality exists in this part of the world, I could never have participated in it because my race undeniably connected me to the West and thus to Western identities. The same situation but between two white men in the same context would, for example, be unthinkable, as they most likely would be interpreted as gay men. Thus, the fear I experienced during this incident was justified.

The second vignette further illustrates structure and race. Because I knew that homosexuality is not criminalized in Turkey (although it is stigmatized), I was more relaxed during my experience in Istanbul and did not internally panic while on the street. Instead, I spoke of passing, which in this situation was connected to me passing as a Turk. Even though I am dark blond, I have a reddish beard, which is very common in Turkey. I have passed as a Turk many times in my fieldwork, and it was likely that I could do so again. Although a similar conception of sexuality exists in both Tanzania and Turkey (Wimark 2016b), my race did not cause me to stand out, which otherwise would have disclosed me as a gay man in Turkey. Moreover, as previous research has discussed, specific places in cities are known to have gay populations, i.e., gay neighborhoods. In this situation, we were walking on Istiklal Street on our way to Taksim Square. This street and square represent places within Istanbul where Western sexual identities have been most embraced and where gay and lesbian bars and clubs are located. Walking hand-in-hand in that location is certainly less dangerous than doing so in any other part of Istanbul if the two people involved are perceived as gay. Two privileged white men, tourists for example, could walk some, but certainly not all, streets hand-in-hand, displaying sexual emotions and being interpreted as homosexual without being harassed. Thus, my feeling of unease was less intense, and my lack of panic is thus understandable.

The last vignette describes both Oskar's experience and my own. Oskar grew up in Ecuador, and he has comprehended the concept of sexual identities since he was very young. Displaying emotions by holding hands was always possible in Ecuador as an act of intimacy, but it was also associated with danger. The memory of danger is cemented in an individual's *känsloläge*, and it does not disappear simply because the individual leaves the context in which related acts are heavily punished. Holding hands was still associated with the danger of repercussions in Oskar's *känsloläge*. Further, Oskar does not belong to the white majority in Sweden, and not belonging is likely to have made him feel less safe, seeing as public transport is used by the majority of the population. This is not to say that Stockholm is completely safe or

that no homophobic harassment or violence occurs there. In fact, before and after this episode, my previous partners and I had experienced several incidents. However, I have learned to associate certain places in the city with danger and others with safety. The subway did not represent a dangerous place for me. Oskar had warned me previously about looking other people in the eye on public transportation, signifying his association of danger and public transport. In my memories, however, the subway represents a place of pride after attending many yearly pride festivals during which the subway was filled with thousands of happy LGBTQ people. Although my spark of anger was exasperating, it signifies memories of working for LGBTQ rights and the fight to be able to show love on the streets without punishment. Thus, even though Oskar and I were in a context in which legal rights and protections have been established, we connected public transport to sexuality differently through our emotions. In the final section, I summarize these experiences and highlight how my proposed approach is essential for research involving fieldwork.

Conclusions

In this chapter, I used my fieldwork experiences to develop a framework for analyzing sexuality and emotions that recognizes the individual, the structure, and the context. In the first section, I drew from previous research by geographical scholars and discussed how sexuality and space are connected. Next, based on studies by scholars of affect and emotion in the field of geography, I proposed using the concepts of *känsloläge* and *känsloupplevelse* and suggested that all subjects have developed a specific *känsloläge* that is derived from the body and from the situations the body has experienced. This *känsloläge* affects how individuals express emotions in a given situation: the *känsloupplevelse*.

Thus, to conceptualize sexuality and emotions as situational, bodies, space, and structure are considered. Given that one context is different from another in time and space, sexuality and emotions will be conceived differently. Living in and traveling through various contexts and structures does not imply that you can avoid the context in which you are born or that you can alter learned behaviors, as your emotions will betray you. This property has several implications. First, any given context has a specific time and structure in which emotions are experienced. These structures merge and change over time, as does the *känsloläge* of subjects. People who travel to another context in which sexuality is conceived differently are exposed to individuals with a different *känsloläge*, while also bringing their own *känsloläge* with them. Simultaneously, biology and power dynamics cannot be avoided at any given situation. Racial hierarchies, for example, persist and affect each individual's *känsloläge* and are highlighted when exploring other contexts. Thus, time should be considered to be both situational and biological. Second, the connection between certain sexualities or sexual behaviors and space is not equally distributed in space. Contexts differ in terms of both the time–space in which they exist and how physical spaces are conceived. In some specific historical eras, some locations within cities

have been marked by sexuality, for example, in the form of “gay neighborhoods.” Simultaneously, the same or other spaces can be known as safe or dangerous places in which to display sexuality. Transitioning from one context to another does not mean that memories disappear; instead, they linger and affect us in our daily life. By considering sexuality and emotions as situational, this analytical approach incorporates the complexity of both time and space.

This analysis should be understood in the light of recent research on sexuality and fieldwork in geography. Previous studies have progressed from discussing challenges related to sexual identities and disclosure (e.g., Burkhart 1996), to how the field and fieldwork can be places of desire (Cupples 2002; Diprose et al. 2013), and how desire is actively played out during fieldwork (Kaspar and Landolt 2016). The present study has sought to further elucidate this field of research by highlighting the fact that structure and context are important issues to consider when analyzing power and positionalities. However, it should be noted that the present analysis is not entirely complete. The analysis is conducted from the perspective of a privileged, cis-gendered white man, and the emotions of the individuals in the vignettes are absent. As Gillian Rose (1997, p. 311) noted, “The search for positionality through transparent reflexivity is bound to fail.” Thus, a possible future avenue for researchers is to allow the analysis to expand to all the participants in the research process. Through such an analytical approach, the situatedness of sexuality and emotions in time and space could be further explored.

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“Normality” Revisited: Fieldwork and Family



Janina Dannenberg

Even though it is increasingly common for ethnographic researchers to reflect on their own emotional and relational positions in the field, the assumed “normality” of a researcher’s life is still that of a person doing field research alone. When I write about “normality,” I neither think that there is any such thing as “objective normality” nor that there should or even could be. I use the term based on two different but intertwined aspects. First, “normality” is what people assume, or what I assume they assume, unless told differently, that is, the being alone of a researcher. Second, since being recognized is a very basic need, “normality” is a relational reference that can influence one’s feelings. In this chapter, I analyze the situations in which I wanted to feel “normal” or I wanted to be perceived as being “normal” versus when I preferred, due to a sense of individuality, to be recognized as somehow special. This, in my case, is connected to the question of having my family¹ with me in the field or not. The notion of “normality” therefore serves as a vehicle for discovering emotions in the field. Merely writing about bringing the family along would probably perpetuate the notion of unaccompanied researchers being “normal.”² Therefore, this chapter not only focuses on how it felt to bring my family along with me into the field, but also how it felt to not have them there.

I will start this chapter by providing a brief literature-based overview on the issue of the (in)visibility of spouses in the academic outcome of ethnographic fieldwork and link this to an introduction of my own research project. Following this, I provide

¹“Family” here refers to my partner and our three children.

²I assume that this notion is held by at least the general public in my country, by academia, and by many local communities in the Philippines that have experiences with researchers.

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insights into my own family situation while being in the field. The first situation I discuss is doing field research without my family, its “(un)normality” with respect to different actors, my corresponding emotions, and how this situation influenced the research design, process, and results. I then reflect similarly on a field research situation where I was joined by my family.

The Historical Invisibility of Researchers’ Families

Based on numbers, it might appear that in the past most ethnographers did their field research on their own. In many cases, however, they did not. Spouses, usually wives, and sometimes children joined researchers and contributed to the success of their fieldwork. For example, it was common for wives to type up their husbands’ field notes (Cassell, 1987a, p. 259). Describing her routine in a joint research venture with her husband (with him as the principal researcher) on devitalization and revitalization in rural Asturias, Spain, in the late 1960s, Fernandez describes her experience as follows: “My activity differed from that of local women in one notable way (...). Their sedentary activity was needlework, mine was typing” (1987, p. 213). Social anthropologists’ wives also fulfilled one of the functions of housewives in capitalism by providing emotional care to their husbands (see Weeks, 2007, pp. 236–237):

There was another need [to join husbands’ fieldwork] (...)—the need to have, among strangers, loved ones of your own. It was important to John that someone else cared whether a village headman would talk or not, that someone got as excited as he did when progress was made—or listened with understanding when the week seemed lost in unfulfilled hopes. (Hitchcock, 1987, p. 175)

Scheper-Hughes (1987, pp. 217–218) goes further and proclaims that “a teamwork model in which spouses and children of all ages accompany the ethnographer” has replaced the “model of the solitary fieldworker.”³

To some extent, accompanying and supporting wives (not the children) were recognized as coauthors, but in many cases, they remained invisible. Okely (2009) even reports on a case where her former partner published on a joint fieldwork in Ireland without even mentioning her. Her assessment of the data gathered in the field, which she published decades later as a reaction to that invisibility, was considerably different to that of her former partner.

Historically, anthropologists rarely wrote about their family in the field.⁴ According to Butler and Michalski Turner, they were “unwilling to see the field mystique perforated” (1987, p. 15). The “sacredness” of their professional public

³She also points out that “these family members may or may not share the anthropologist’s affinity or enthusiasm for ‘basic strangeness’” and that children’s involvement may rarely be voluntary (Scheper-Hughes, 1987, p. 218).

⁴To complete the picture, it should be stated that, regarding research content, female lifeworlds prior to the women’s movement of the 1970s were rarely studied. The same is true for children who were not perceived as independent actors up to the 1980s.

lives was not to be mixed with the “profaneness” of their domestic lives (*ibid.*). Women anthropologists were warned by colleagues that they would lose their scientific reputation if they were to reflect on their experiences of fieldwork as mothers (*ibid.*). This gendered division between the private and the professional (and the many other divisions that go along with it) is not limited to anthropology alone but is also evident in the entire Western scientific and economic systems (e.g., Butler and Turner Michalski 1987, p. 16; Biesecker and Hofmeister 2010).

I am not an anthropologist. In the discipline I am trained in, sustainability science, the deconstruction of such hierarchical boundaries is an important issue. Hybrids of nature and culture, of social and natural science, and of scientific and societal knowledge are constantly being (re)discovered. Sustainability science aims to be recursive and reflexive. In this normative science, subject–object dichotomies are therefore challenged. Nevertheless, the absence or presence of the researcher’s family in the field shapes results and therefore deserves reflection. In particular, the “normal” case of an individual researcher has, however, not received critical attention.

In my dissertation project, for which I conducted the fieldwork I am reflecting on here, I examined boundaries between and hybrid zones of, on the one hand, paid work and commodified nature that markets recognize as productive, and on the other hand, unpaid work and processes in nature that are given the status of the “reproductive” in the rural Philippines. The framework involved recognizing the productivity of the (excluded) reproductive and to discover forms of productivity that included the aspect of regeneration. Thus, examining binary structures such as private and professional is not just a matter of reflection for general epistemological reasons, but connects directly to the content of research.

This contribution is based on my experience of conducting fieldwork in the rural Philippines in 2013 and 2014/2015. For most of my stay, I kept a diary on my emotions.⁵ In 2013, I stayed for 6 weeks and left my children Mila (7), Sven (4), and Ligaya (1) together with my partner (Chris) in Hamburg.⁶ The second trip was a 6-month stay starting 1 year later. Chris and the kids joined me for 4 months during the middle part of the field research. I was on my own for the first and the last month.

Chris and I had our first extended stay in the Philippines during my time there as an exchange student back in 2005. It was during that time, through exposures to and with local NGOs, that we—flexible, nosy, and full of energy—got to know two of the three communities where I would be doing my doctoral research 10 years later.

⁵This was done in cooperation with the research project “The Researcher’s Affects,” based at the Free University of Berlin. In the diary the project provided were questions about the days feelings and desires and a check box questionnaire with different emotion words (see Appendix). To answer this, I used my everyday concept of emotions as feelings that I was able to identify in the moment of writing. My own agency in these feelings, the involvement of my body in these feelings, my consciousness of my feelings in these situations, and other criteria that do specify emotions, differed in their degree.

⁶These names have been slightly modified. Chris gave his consent on publishing, the children I did not consult.

Research Without Family

Leaving Home

My emotions toward the “normality” of going abroad as an individual researcher were ambivalent. I am a German doctoral student who grew up in German middle-class surroundings, family-oriented but open-minded. At first, leaving my family behind made me feel strong. I enjoyed that I was able to pretend that I was as independent as I had been years before. I felt admired by other mothers⁷ being stuck at their part time jobs leaving neither enough time for their children nor for occupational fulfillment.⁸ Additionally, due to the fact that in our community the everyday small talk among parents is dominated by women, it was me, and not my partner, who won recognition for “having such a good husband who would take care of the kids seriously.” My relatives’ responses were a bit different: they were more worried about the children’s well-being and how to support their self-sacrificing father. However, in my situation, both bringing the children and not bringing the children to the Philippines was somehow special. I was fine with that. Living in a society that demands individual lifestyles while the reality of life can easily end up dominated by daily routine, I enjoyed the celebration of being different. In academia, on the other hand, going abroad without family is considered “normal.” The emotional and logistical effort of leaving a family behind is usually not considered. Being part of that “normality” for me meant keeping quiet about the limitations of these “private” circumstances. I felt that doing so had a negative effect on my personal integrity.

Research Design

Being on my own influenced my choice of research area as well as the content of my research. In 2013, my fieldwork took place in a village in the mountains of central Mindanao. Chris and I knew the place already; I conducted research there when Mila was 1-year-old. From the beginning Chris made clear that if I were to bring him and the children to the Philippines, he would not be willing to live in that village again. He felt we would not be able to run a, what he considered, independent life as a family there. We were also concerned about security issues. Therefore, having children influenced, not only the decision where I conducted research with my family (as in Fernandez, 1987, pp. 188–192), but also the decision where research without my family would be conducted: of the three areas examined in my research project, the most remote area was the one where I worked as an individual researcher.

⁷Neighbors in Hamburg, friends, and mothers from my children’s kindergarten and school.

⁸The style how I differentiate myself here from homogenized “other mothers” is symptomatic of my emotions at that time. In fact, of course I have the same conflict (such as many working parents) to an individual degree.

As mentioned above, my research question is very broad, and I followed the approach of being led by the field. Being on my own influenced what I actually worked on, and thus finally what would be published. As I was independent, I did not need a space where family issues could be handled. For this reason, I took up the offer to live directly in the office of a regional management organization. When there were meetings, I willingly consigned the last hints of my domestic life—my mat, mosquito net, and sleeping bag—into invisibility. I was awoken in the mornings by meetings of male indigenous leaders, and the issues they discussed were the leads I followed. Leaving my family in Germany, and thus having no domestic duties,⁹ influenced the content of my research. It led to my research being done in the public sphere. I believe the effect this had on me must be similar to other individual researchers following an open approach. Not to overlook domestic issues, but rather to take notice of work that is done beneath the surface, is something one must actively force oneself to do if alone. In my field notes I wrote down things such as, “It’s so incredible how much work had to be done just to serve us lunch,” partly driven by empathy, partly due to a sense of duty. Every now and then I helped out, but I also felt glad they did not really accept my help, so that I could strive for more exciting experiences in the other parts of the organization. Whatever I did, I would be fed anyway. My evenings were taken up by writing.

Staying Abroad

My emotions toward the “normality” of staying in the field as an individual researcher were ambivalent. In my opinion, during solitary fieldwork many researchers consider it “normal” that the fieldwork itself, or at least the community studied, be at the center of one’s concern. The life you leave behind is expected to be more of a side issue. As a form of “social fiction”¹⁰ this works out. In the field you are just there. You do not have much outside of the field to care about. Your whole personality is there, not only is your analytical brain engaged, but your body and emotions are as well. When I was researching on my own, it felt as if I took a break from having a family. I wrote very few e-mails to my partner, and for the children, I taped an audio message every other week but did not contact them via Skype. In a certain sense I practiced a professional life completely distinct and removed from my personal life. As people in the area were very used to dealing with researchers coming alone, I think we had a mutual understanding of perceiving what I did as “normal”; it was simply “how researchers tend to show up in the area.” Occasionally

⁹If I had time, I bought food at the market and handed it over to a young mother who usually cooked for the office staff with whom I took my meals. If not, I simply gave money.

¹⁰I borrow the term “social fiction” from Scheper-Hughes. She describes it as an “‘as if’ phenomenon” in which both sides, anthropologist and local people behave ‘as if’ the anthropologist were a normal part of local life, while knowing better” (1987, p. 219, citing Pelto and Pelto 1973). Acting “as if” there were no family at home for me was an additional “social fiction.”

mentioning my family felt like telling stories of another world and did not involve considerable emotional aspects.

This was different during the time when I was preparing for my family's stay in the Philippines and during the time after my family had just departed. Something from my life back home scratched on the perfection of the field. My family, my "private" life back in Germany, gained a stronger presence in the field during these times. The "social fiction" of being a "normal" individual researcher was still stable with regard to interactions but had been affected by emotions.

When I visited a family that I had known for 10 years, my favorite "sisters" from that time had children now. They were hardworking and exhausted. Little seemed to be left of the energy and youthful creativity I had noticed years ago. But for me it was different. My role in that family appeared to be the same as it was in 2005. I was still hanging around independently, still ready for a joke, representing a glance of carefree youth. I felt displaced, uncomfortable. Driven by feelings of solidarity and a sense of pride at the same time, I wanted to make the community see that my role had also changed and that I, too, was exhausted, that little of my energy was left, and that I knew how to run a household. However, my status as a guest did not allow for that. I felt that I was more similar to my "sisters" than what I was able to show them without having my children close by. While I did not experience general insecurities about feeling accepted as a researcher without children (Nichter and Nichter 1987, p. 66; Fluehr-Lobban and Lobban 1987, p. 238), when I was alone I still did not accept myself as authentic, something I consider important. My feelings were similar to Dreher's, who felt guilty that her informants were not able to see her in her own social context when she was without family (1987, p. 167).¹¹ My assessment and perception of the place where I was about to do research was framed by the question of how it would be there with my family. Deep inside I started to take over the role of the lonely mother. When I met another doctoral researcher from Europe, for example, I could not help but recognize how much time he seemed to have for his research and how little he seemed to be aware of that fact. Based on these kinds of experiences in my academic environment and the way I was perceived by wider family and friends, I felt my situation was somewhat special.

Not so among urban professionals in the Philippines. I started realizing that many people I met and perceived as independent individuals had children in their *provinces*.¹² Suddenly I felt very ordinary and I did not want to share many emotions. Nobody else shared theirs. My family and I were only apart for a few weeks. I was not going to ask for compassion from parents who experience this for years. Although I had no idea how they felt about being apart from their families, I felt slightly ashamed that I was doing this for a mixture of adventure, social change, and career, but not for survival.

¹¹The transparency of your own life, as Cassell points out, gives way to a more dialogic research relation because people can study how you deal with family issues (1987a, p. 258–250). This is true for my case also, but of course there remains a power relation.

¹²Rural areas from which people in metropolitan Manila had migrated.

Increasing Emotional Involvement

Being an individual researcher may increase the role of emotions in the field. In my case, my degree of emotional involvement¹³ was considerably higher when I was alone in the field. It was during my time without my family that I started to really love some people I was researching with and started to feel deep empathy for them. This strong emotional sensitivity enriched my perception in the field. Led by emotions I was able to identify very small details and, on the other side of the coin, I was also more prone to construct fitting details.

Many of my emotional notes, from my stay without my family, circled around interpersonal relations: Did I act the right way? Did the other person consider me arrogant in that situation? Why was he so brusque toward me? Should I have been visiting them earlier? These reflections are very detailed and go hand in hand with very detailed observations that contribute to the quality of research. Everything that others did seemed so important. What I often did not consider was that, in many cases, it was probably just me who put importance into a certain interaction or relation. When emotions occurred, I never thought to myself: "Anyway, this is just an issue at work. Soon it will be the weekend," or "Anyway, this is just this person's private opinion; it's not related to my research." On the other hand, the person my thoughts were concerned with might have done exactly that. They might have had something emotionally and practically more important to do than reflecting on their relation to a researcher. As an ethnographer without genuine care relations in the field, I think I somehow tended to overestimate my relations to others.

Interestingly, many of my strong feelings were connected to something that was missing in my life in the field: children and a loving family.¹⁴ Probably, my strongest emotional experience was when I felt that children I knew well were being treated unfairly by a teacher at their school. Also, I developed a strong emotional attachment to certain elderly women. As is customary in the Philippines, I called them *nanay*, meaning "mother," and I conceptualized them as strong, politically active, full of love, organized, and socially and technically skilled. In short, I saw them as mothers and enjoyed being temporarily attached to them as a daughter would have been.

¹³This refers to a rough combination: number of emotions, frequency of being emotionally affected, intensity of emotions, instability of emotions, willingness to surrender to emotions, the capacity to allow for emotions.

¹⁴I was conscious about missing my family especially when I prepared their stay and after they had left. But even when I was not so conscious about missing them, I think it still played a role.

Research with Family

It was indeed much easier for me to capture analytically how bringing my children¹⁵ to the field influenced my emotions and research. In many instances, I had the same experiences as others before me. As “there is nothing so soothing as throwing yourself into some practical activity” (Hugh-Jones, 1987, p. 40), prior to bringing my children to the field, I distracted myself from the feeling of fear by focusing on preparations for my trip (Cassell, 1987b, p. 5).¹⁶ As is customary, I did not set aside any additional time to emotionally and practically prepare for the fieldwork with the children in my official research timetable. This is an example of how the everyday structure of academia promotes the invisibility of researchers’ care obligations.

Similar to Hugh-Jones, who did joint research with her husband in the Amazon region and found it “an enormous pleasure (...) to have integrated the two great experiences of our adult lives: life with Indians and life with children” (1987, p. 62), I also felt that the whole idea of bringing Chris and the children would integrate the different aspects of our lives: as a family, as human rights advocates, and for me, as a researcher.

When we were there, the logistics of everyday life and the feeling of being responsible for almost everything were heavy to carry.¹⁷ Additionally, our inconvenient living conditions—no running water, no bathroom, a long distance from grocery stores—made it necessary that I physically help Chris in running the household and taking care of the children, including tutoring Mila.¹⁸ We did not hire a nanny for several reasons, one of which was to come closer to the “normality” of a family in that community.¹⁹

¹⁵Due to space limitations, I will focus on the role of the children. Surely, Chris also influenced how I felt in the field. His motivation for joining was low, that’s why he was moody most of the time. I felt obliged to compensate for that, meaning that I felt I had to ensure a positive atmosphere in the family, fix most logistics, and care for our relations to the neighbors. This situation was stressful to me and led to some tension between us.

¹⁶Upon my arrival in the field, similar to Cassell, the obligation to care for somebody other than myself provided me with security (1987b, p. 8).

¹⁷Dreher also describes this feeling of being occupied by childcare, even when you are rarely involved in the practical aspects of it (1987, p. 168–169).

¹⁸The writers in Cassell’s volume who tutored their own children did not mention that it was stressful, while for me, it was.

¹⁹The difficulties and ethics of paid support for anthropologist’s families are discussed by, for example, Cassell (1987b, pp. 6–7, 10), Fluehr-Lobban and Lobban (1987, pp. 246–248), and Nichter and Nichter (1987, pp. 68–70).

Participation Through Domestic Work

On the one hand, being caught up in domestic work was very unsatisfactory to me. But, on the other, doing domestic work made me a participant and invited the "normality" of the field into my house. In the beginning, my assessment was similar to that of Dreher (1987): "It took me twice the time to accomplish half the work that I would have normally [i.e. without children] accomplished" (p. 165). I did not yet question the "normality" of what Dreher calls "normally" and was not fully aware of how leaving my family at home influenced the research. I only knew that bringing my family would influence my research. After a while, however, I learned that what I did was a variation of participant observation and that it was deepening my understanding of life in that village. Until that time, my experience of participant observation had been that I always offered my help, be it in the kitchen or in the field, but never really helped. I was just not capable of doing so. In fact, people even stopped doing their work in order to explain things to me or to answer my questions. Indeed, I perceived these on-the-spot moments to be the most fruitful for data collection. Now, running my own family, what I did with my hands really made a difference; I really did what other parents did—be it fetching water or picking lice from my children's heads. While my family was with me, I did not stroll to a house and ask somebody, "What are you doing?" and listen to the answer, "I was just doing the laundry," and think to myself that I know what that means, finding the answers somewhat boring, and moving on to the next. Instead, on days when I worked a lot in the house or did the laundry, I did not talk much at all. I therefore learned to accept this experience as participant observation.

Accordingly, the field did not end outside my house but somehow crept inside it. During my fieldwork on my own, I wrote a note in my diary:

When I lay down inside my mosquito net I feel completely exhausted. Being alone in a room, surrounded by the net, so comforting. (...) I am overwhelmed. It poses such a pressure that my research is outside waiting for me. That, by just leaving the room I will be fully into it again.

The field was outside. I had, even though it was small in a physical sense, a space without research. With my family there, however, the field entered my home under the camouflage of housework. Coming to terms with this was a step toward challenging the power of the "professional" over the "private."

Insights Through Children

Similarly, of course, my family life never ended at the front door of the house. Having my children around widened my epistemological horizon. Like others (e.g., Nichter and Nichter 1987, p. 76; Cassell, 1987b, p. 9), I experienced that my children were door openers. Especially the youngest, Ligaya, seemed to open everybody's heart when she joined me during the interviews. The English meaning of her

Filipino name is *Joy* and this is what seemed to be her program. People enjoyed talking to me when I brought *Ligaya*, whom they referred to as *manika* (doll),²⁰ and that made me happy. With my son, *Sven*, it was a bit different. He was a late talker and had developed a repertoire of nonverbal ways of enforcing attention that he rediscovered when he was thrown back into speechlessness abroad. Interactions he had with local kids or adults sometimes ended in conflict. More often they did not, but we were still stressed out by this. When people were patient with him, I felt ashamed to bother them²¹; when they were impatient, I felt sympathy for my son. However, it was he who “helped” in the rice field. While I was worried about the mess he caused on the field, his activities opened up the opportunity for me to ask a sensitive question about ownership. I had been waiting for that moment for many days. As I was staying close to the rice field, watching my child, I had the chance to informally begin a conversation in situ. I could walk along the field boundaries while we were talking about them, which was helpful since my Tagalog is limited. Of course, this might have also been possible without a child, but the way it worked out was much more “normal” to everybody involved. Moreover, at the store it was *Sven* who used with ease a Tagalog phrase that I had not noticed at the time. Indeed, children “will be living the culture that you are only studying” (Dreher, 1987, p. 171).

In my research I avoided judging the lifestyles of the protagonists of my research. I perceived the research situation, as described above, as a kind of “social fiction,” where everybody pretended that everything was normal and where cultural differences were accepted. Children were also forced into this “contract,” which they (still not settled in their own identity) could barely understand (Scheper-Hughes, 1987, p. 219). In our case, we neither interacted with the children the way we would have done on vacation, nor as we would have at home. We were not on vacation and I would have considered it disrespectful to the local community to present the whole project to the children as a big adventure trip. They simply were supposed to adjust to a life in fictional “normality.” This was easier for the younger children, but for *Mila*, who was 8, it was more difficult.²² Like Scheper-Hughes’ daughter (1987, p. 229), she found us guilty of depriving her of everything she had at home, for example, friends, school, a bed for herself, and the ability to lead complex conversations with people other than her parents. She also recognized that our reaction to behavior such as excessive consumption of superficial television and violent movies or questions of diet and the behavior of and toward other children were different in the Philippines. As *Nichter* and *Nichter* point out, children make the cultural

²⁰ Bringing my own child made this objectification of children more visible to me. Fernandez even gained insights through an offensive situation experienced by her child (1987, pp. 200–203).

²¹ The whole issue of reciprocity is affected by family constellations. With a family of five, I felt even more helpless in expressing my gratitude when people were trying to make life more comfortable for us than I did when I was on my own. On the other hand, people might actually have enjoyed being with us even more.

²² That it is harder to adjust for older children is an observation others also made (Dreher 1987, p. 156, 170; Fluehr-Lobban and Lobban 1987, p. 239).

embeddedness of the researchers visible (1987, p. 77). I agree, but Mila also helped me to be more concerned about certain issues, that is, not to see them as “that’s just how it is here” and not to rely on cultural differences as an easy explanation. In order to help our children adjust, our style of education, that of others, as well as the similarities and differences in ways of life, became subjects to reflect on in a manner that I think is unique to family research, especially because emotions were so intensely involved. “Normality,” even for my daughter, turned out to be a process of negotiation.

Conclusion

In my fieldwork, I had the chance to discover and reflect on the effects of different family constellations on my work. Each of these constellations felt “normal” in relation to some groups of actors in my fieldwork surroundings as well as academic and private surroundings and their respective expectations, but strange in relation to others. “Normality” in research, if this involves research without ones’ family, reaffirms the powerful traditional perspective of alleged pure professional academe.

The question how the private (be it your private life in the field or your private life abroad) and the professional life are integrated or separated is something to be constantly negotiated in fieldwork—whether your family is there or not. However, with your family in the field, choices are limited and the boundary of private and professional life will be dissolved. Even though the research situation with a family is still a “social fiction,” I experienced it as a deeper form of holism in research than simply acting as if there was no connection to the world I come from. As an individual researcher, it is much easier to actively be in control of the extent to which people know about and share in your family life.

Similar to the issue of integrating your private life in the field, other common organizational and emotional challenges in research, such as reciprocity, health, or foreignness, take different shapes in different research constellations. It is not the family on its own that constitutes something special in the field, but its intersection with the abovementioned challenges that would also appear in research situations without the family present. When you bring your family, decisions such as “where to sleep?” are very prominent. But as I have shown, when you do not bring your family, these issues nevertheless subtly influence your fieldwork and are themselves influenced by what you chose to keep out of the field.

In my case, emotions toward research-related situations or people turned out to be more intense whenever I had no family life that kept me emotionally engaged. Therefore, my emotions were not just influenced by who was there, but also by who was not there. Additionally, even during the time my family was not there, my emotional experiences differed considerably depending on the level of contact we had.²³

²³With “level of contact” I refer not only to the aspect of communication, but also to the presence my family somehow had in my life when I was preparing for their stay.

General experiences of having one's own children in the field have been elaborated in the past, and I rediscovered many aspects of my experiences in the literature.²⁴ The focal point seems to be that children change your status in relation to specific social groups and therefore influence your insights. Local life is also affected differently.

Another important point is that doing domestic, reproductive work for and with your loved ones may increase its visibility in the outcome of the work. Research in my discipline (sustainability science) which, as a "post-normal-science" (e.g., Funtowicz and Ravetz 2008), claims to include different forms of knowledge, could gain from making reproductive work during processes of knowledge production visible. Furthermore, the reflection on emotions during fieldwork may also help scientists in my discipline to overcome the often criticized limitations of "normality" in science.

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²⁴It is important to mention that I do not see myself in the same cultural position as American anthropologists during research in the 1960s–1980s. Especially their conception of childhood and methods of child-rearing were in many cases different from mine. But overall, there are many similarities in how fieldwork with children is perceived, and thus I could easily connect to what they wrote.

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Part IV
Dealing with Illness and Dying

Dealing With Illness and Dying: Introduction



Marcos Andrade Neves and Tereza Baltag

We work under an illusion of detachment, an illusion that the subject of our research is elsewhere, safe within the limits of a field we have conveniently designed to come from and go to as needed and while we move ourselves in and out of that field, those we research with remain. This—expected—ability to approach and to distance ourselves from the field of our work is the basis of our research routine: it gives us the proximity we need to get in contact with our subject as well as the distance needed to reflect and write. This ability to detach is, however, an illusion. It fails to acknowledge not only the many ways through which the field makes itself present when we are *away* from it, but also the different ways we make ourselves present when we are *in* it. Nevertheless, this illusion can be useful. It gives us the perception of a comfort zone, a mapped out escape route we can resort to if necessary. But it also leaves us vulnerable to the moments when we realize that our research subject, its participants, and our personal lives overlap—moments when, suddenly, our research topic becomes a personal journey that stretches out to our family and friends, when we find ourselves in the field, but end up facing our own fears, doubts, and ability to stay open-minded. Moments when we confront ourselves with our own affects, turning the expectation of a detached field into a reality of constant immersion, a reality that touches us emotionally and that we cannot hide from.

The confrontation with such moments can take various shapes, triggering different experiences of the fading of this illusion. It can give way to uncertainty and fear, to discomfort and vulnerability, and to rethinking one's own positionality. When the

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illusion of detachment fades away, suddenly one's affects, personal issues, and trajectory become intertwined with the research, with its subject, questions, and participants. The chapters in this section present some of the shapes this intertwining assumes while researching health and illness—from the uncertainties of fieldwork to the impact it exerts on the researcher's affects—and the other way around. Anthropologists often find themselves at hospitals, clinics, health institutions, and general settings where health, illness, and dying are experienced and are subjects in everyday life. These are settings where we can feel intense grief, joy, sadness, and compassion, observable emotions that help us understand how important these moments are for individual persons, families, and communities. Emotions that not only give us an insight on what is going on, but also make us realize how vulnerable our positionality as researchers is.

Anthropologists have occupied themselves with the cultural dimension of emotional experiences for a long time (Douglas 1969; Geertz 1973). In the 1980s, for instance, several medical anthropologists focused on the effects of emotions in relation to experiences of illness, pain, madness, and dying (Kleinman 1982, 1986; Kleinman and Good 1985; Levy and Rosaldo 1983; Lutz 1985; Rosaldo 1984). However, the development of new medical technologies has led to new situations and challenges for the researcher. One such case can be found in the context of tissue economies, where anthropologists have questioned the emotional challenges of those who are involved in this process (Lambert and McDonald 2009; McDonald 2011; Waldby and Mitchell 2006). However, little has been written about how these emotions and affects exert an impact on anthropologists in the field, and on the research process itself.

Does this absence imply that the researcher is not affected while in the field? That a researcher's affects and emotions have no impact on the ways they approach their research participants or pose questions to them? By making ourselves oblivious to our own affective experiences, we end up ignoring a substantial part of our own work; more importantly, we remain under the illusion of detachment. The three chapters in this section on health and illness are self-reflexive pieces, written by researchers from different professional backgrounds, who carried out fieldwork in different places and on different topics, but who are bound together by a common interest: rethinking their fieldwork experience from the standpoint of their affects. They open up about their emotions and affects, their worries and difficulties when dealing with a field that can no longer be dissociated from their personal lives. And by reflecting on their research from this angle, they offer us not only interesting takes on the intricacies of doing fieldwork, but also tools for acknowledging it as a research space able to affect and be affected by our personal lives.

Julia Rehsman provides an account of doing fieldwork in-between her professional and private lives, describing how her research on transplant medicine and her personal life became interlaced from the moment she received an e-mail from a close friend telling her he had been diagnosed with cancer. The friend, Philipp, had first informed her about the diagnosis when she was just beginning fieldwork in Germany, and kept sending her frequent updates on the condition of his health. While she tried to balance her fieldwork in Germany with visits to Philipp in Austria,

it soon became clear to her that she was not coming and going from the field, but rather entangling both facets in one single relational dynamic.

By exploring her affects in the course of her fieldwork, Rehsmann noticed that doing fieldwork creates relational spaces that are hybrid ventures between professional and private lives. If our research space is a hybrid, she argues, it is necessary to be aware of our own positionality and reflect on our emotions in order to inhabit it—thus turning this self-reflexivity into an epistemological and methodological tool. This unsettling situation of accompanying Philipp while, at the same time, engaging with people awaiting or undergoing transplant procedures has affected her even after the completion of the fieldwork. She left the field, but kept living in this hybrid space through different ways and experiences. Once back at her desk, this hybrid space lived on through fears and dreams. She feared dying of cancer, often-times dreaming of herself being diagnosed with it.

Rehsmann concluded that her experiences while inhabiting this hybrid space had an impact on her research, on her “being-in-the-field.” Her experiences with life-threatening diseases position her in close dialogue with Natashe Lemos Dekker’s take on her fieldwork in three nursing homes in the Netherlands, where she had encountered people with dementia. Death, as Lemos Dekker puts it, is a process that is experienced not only by the dying person, but also by the ones around them—by family and friends and by the researcher who is *there*. In the nursing homes where she carried out her fieldwork, “being there” (Hollan 2008) meant quite a lot of waiting: waiting in the common areas, silently sitting or staring out of the window, or waiting by bedsides. How to negotiate her space in the nursing homes? Where to stand when death is occurring, and how to approach the person dying or the family members who are by their side? How not to appear, as she phrases it, voyeuristic in her interactions with death and dying?

To illustrate some of the concerns surrounding being present in such intimate moments among close relatives, or even with the nursing home personnel, Lemos Dekker employs the metaphor of the doorstep. As she writes, the doorstep materializes through “engagement with sensitivity, proximity and distance, and being respectful” (this volume, pp. 195–205). This is a space from which her positionality in the field could be negotiated and her intimacy mediated. Just as the doorstep provides a means of illustrating her worries regarding being there, the role played by materiality in reconciling her affects with the intimacy of death and dying goes beyond it, as can be seen her discomfort led her to open a notebook during a family meeting, a notebook that would somehow reposition her role in that gathering, in an attempt to become “transparent” (pp. 195–205).

For Lemos Dekker, self-reflection on her positionality throughout fieldwork was essential to acknowledge the limits of her being there. This is an aspect that becomes evident when she describes how uncomfortable she felt by taking part in family discussions, somehow trying to reconcile her role as researcher with the uncomfortable feeling of being there during very personal moments. There were moments where she needed to avoid crying, where she questioned her own “right” to feel a sorrow that was perhaps not hers to feel. If opening the notebook was a way of establishing a specific role for her during that family meeting, what role would tears grant her?

As in Lemos Dekker's piece, the question of how to reconcile positionality with affects is central to Tereza Baltag's account of her research on substitutional drug therapies and its users in Prague, Czech Republic. However, in Baltag's case, she had to transition from her background as a therapist at the Healthcare Centre Gestalt to her new role as an anthropologist. This was a professional transition that encompassed the transformation of patients into interlocutors and move from making observations inside healthcare facilities to fieldwork in the so-called open drug scene. In this way, Baltag follows people addicted to buprenorphine who, upon being removed from a substitution program that takes place in a clinical setting, seek this substitution drug outside of the healthcare system—places where heroin and methamphetamine are also sold. Just as Rehsmann's questions started after Philipp's cancer diagnosis and subsequent death and Lemos Dekker's account is based on the proximity to death and dying, it was no different for Baltag. Her transition occurred 1 year after one of her patients, Sarah, died following a lifetime struggle with drug addiction, from methamphetamine as a child to opiates later in life.

Baltag asks herself how the changing of roles over the course of her professional trajectory, from psychotherapist to anthropologist, changed her perspective on the topic of drug addiction and shaped her research. If, within the organization where she previously worked, she had a close relationship with her "clients," once out of it, she felt more distant from her research participants to the point of them appearing to be without history, as decontextualized people. She felt distant and somehow shielded from them. However, as she writes, this distance was reduced over time, triggering different emotional reactions from her according to which role she was performing at the time and according to her positionality.

Nevertheless, her attention remained focused on her experience and reflections. She highlights how psychology deals with the therapist's emotional experiences, and how this approach contrasts with the figure of the lonely anthropologist left to deal with their emotions on their own. The move from a small therapist's room full of intimate relationships to the so-called open drug scene changed her experiences and brought up emotions she would not have been able to experience in a clinical setting. By reflecting on the anger and frustration she felt while researching the open drug scene, she began to bear witness to the injustice of the healthcare system, a system that is barely available to those who need it the most. This experience constantly brings Sarah's story back to mind, and raises questions such as how her death could have been prevented.

Despite dealing with different health issues—that is, cancer and transplant therapies, dementia, and drug addiction—all of the cases presented in this section revolve around death and dying. To Rehsmann, it was Philipp's dying that made her aware of the relational hybrid space in her work. In Lemos Dekker's experience in the nursing home, dementia, death, and dying walked hand in hand with each other. To Baltag, it was Sarah's death that preceded her own transition from therapist to anthropologist and prompted her to begin earnestly reflecting on her feelings. In this manner, if health and illness offer a general context to the three pieces in this section, underlining all of them is death. A death that triggers reflections and emotions shifts our positionality and makes us realize that personal and professional lives are not

two separate spheres, but rather a relational hybrid space, where affects should play a prominent role. Despite this broader context and its underlining subject, it is precisely to this prominent role that the focus should be directed. Only then the illusion of detachment can be lifted and our research brought out of this comfort zone and into our lives.

Each of the pieces in this section have contributed to highlighting how the systematic attention to the researcher's own affects and emotions can foster anthropological insight. For instance, Rehsmann's permanent sensitivity to her own feelings and dreams had an impact on the way her research came to be, as did Lemos Dekker's in realizing how this constant awareness played a key role in an ethically delicate research environment. In turn, by paying attention to her own anger and frustration, Baltag helped draw attention to social injustices in relation to drug users, and the sadness that remained after Sarah's death was the driving force of an engaged anthropological research. The epistemological relevance of constantly being aware of our own emotions and affects during fieldwork goes beyond just acknowledging our own positionality. It generates new ethnographic data and insights, shifting our perception of the research topic and making us experience the field in a different manner. It makes us aware of this illusion of detachment and, by doing so, offers a way out.

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Dancing Through the Perfect Storm: Encountering Illness and Death in the Field and Beyond



Julia Rehsman

Just as human existence is never simply an unfolding from within but rather an outcome of a situation, of a relationship with others, so human understanding is never born of contemplating the world from afar; it is an emergent and perpetually renegotiated outcome of social interaction, dialogue, and engagement. And though something of one's own experience—of hope or despair, affinity or estrangement, well-being or illness—is always one's point of departure, this experience continually undergoes a sea change in the course of one's encounters and conversations with others. Life transpires in the subjective in-between, in a space that remains indeterminate despite our attempts to fix our position within it—a borderlands, as it were, a third world. For these reasons, intersubjectivity is not only what an ethnographer studies; it is the matrix, method, and means whereby an understanding is reached, albeit provisionally, of the other and of oneself. (Jackson 2011, p. xiii)

This chapter is about uncertainties. The uncertainties of life and death, crystallizing in the face of a life-threatening disease. The uncertainties of diagnosis, prognosis, and treatment. The uncertainties of “doing fieldwork” on life-threatening diseases, while one's loved ones face illness and death. This chapter is about the unsettling aspect of these unknowns and the impossibility of preparing for them. But, moreover, it is also about their affirming aspects, in order to understand and accept these uncertainties as a central part of the anthropological endeavor and human existence in general (Strasser and Piart 2018). Just as pointed out in the passage cited above, understanding is the outcome of encounters, interactions, relations. Anyone who has experienced these moments of realization, of grasping a thought, knows about their emotionality. Understanding itself is a highly emotional process. Moreover, I argue, recognizing one's own emotions in the field is important for anthropological knowledge production. I consider emotional reflexivity to be a meaningful way to gain a deeper, more nuanced understanding of the topics anthropologists investigate.

The topic I was keen to explore for my doctoral research project was liver transplantation in Germany. Conducting fieldwork included ethnographic work in transplant clinics and at hospital bedsides, talking to people suffering from cancer and

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other life-threatening diseases. A challenging topic in itself, my research coincided with a close friend's cancer diagnosis, treatment and finally death. In this chapter, I want to approach the question of how my personal experiences of illness and death during fieldwork affected my ethnographic research and analysis. I weave this very personal account together with a discussion of anthropological fieldwork, emphasizing the relational spaces that open up through this form of inquiry. The anthropological mode of research, with its emphasis on long-term, in-depth qualitative data collection, entails that the actual "doing" of anthropology very often turns into a hybrid venture between professional and personal lives. I argue that due to this hybridity, emotional reflexivity is of crucial importance for the self-critical approach anthropology requires of its scholars—especially in clinical contexts when exploring, and encountering, illness and death.

Questions to Live With

For me, it all began with an e-mail. It was on September 15, 2014, and I had just started to settle in to the place I would call home over the course of the following months of fieldwork, when a close friend of mine sent me a message.

Subject: Necessary note¹

Dear Resi,

Please don't be shocked, but I have to tell you something very concerning.

I've been in the hospital since Friday with a suspected malignant tumor, a sarcoma. (...)

I'll keep you up to date and hope that you have more enjoyable news, which I would love to read. I'll let you know as soon as I know more and hope that life is better at your new place, which I'd love to read about. I love you very much and send you many kisses (the children and Maria would too, if they were here now). With all my love,
Philipp

I had arrived at my new field site, a German city, just 2 weeks before receiving this e-mail. I was about to start my fieldwork on transplant medicine in Germany, on how people get access to this life-saving, high-tech, high-end medical procedure and the ethical dimensions it entails. As I made my way into the medical world of transplant medicine, a seemingly mundane issue began to intrigue me. What had caught my attention was how important the waiting time had turned out to be in patients' lives: how their past experiences of waiting for a transplant had a tremendous effect on their lives in the present, in terms of the quality of that time, but also with regard to being able to prepare for the life-changing event to come (Rehsman 2017). I began to explore the morally configured time before transplantation (*ibid.*, 2018), when people seem to be waiting between life and death—waiting for one, or both, of these things to occur.

The only certainty we face in our lives is death; it is the one thing that we share with all fellow human beings alike. But the certainty of death brings with it the

¹ I translated Philipp's e-mail and text messages freely from German into English.

uncertainty of *when* and *how*, and it seems that this uncertainty is the source of our fear of death (Bauman 1992).

Death and I had rarely crossed paths, in real life as well as in my thoughts and imagination. When I left Vienna and moved to Switzerland for my doctoral studies, it started to lurk from behind my books, from between the sheets of paper on my desk, from between the notes about my fieldwork preparations. I decided to ignore its presence and focus on other aspects of my upcoming research, notwithstanding the fact that death will inevitably play a certain role in research on life-threatening diseases. From time to time, it came to mind, and as the months of preparations came to an end, I posed in doctoral seminar the uncomfortable question of how to prepare for the possibility of encountering death. I raised it at the very end of the session, barely giving any time for an answer, and stated in the next breath: “I know it is an impossible question, we all have to figure those things out for ourselves.” Back then, I could not have imagined how true this statement would turn out to be.

Sherine Hamdy, an anthropologist working on organ transplantation and donation in Egypt, faced similar issues when her father suffered from a fatal brain tumor while she was in the field. She wrote in the preface of her book that the “[Q]uestions that had formed the bulk of my research about how people come to difficult bioethical decisions when faced with tremendous pain and the imminence of death were now questions that I was living with” (2012, p. xxiii). In a similar vein, the questions I had thought I would have to deal with in the field suddenly confronted me from another, mercilessly personal, angle. They became questions I was living with.

A Perfect Storm

Liver transplant medicine as an anthropological research topic tends to be a fragmented and intangible field. As I had decided to try and enter “the field” through patient associations, I was really excited (and quite nervous) when given the opportunity to participate in a meeting of a local patient support group—my first fieldwork encounter for my new project. After months of reading and preparing, I was finally “entering the field.” What could I expect from people who were either waiting for a life-saving liver transplant or who had already received one? I tried to play it cool, but my heart was racing on the train ride to the medieval town where the meeting was about to take place at a local monastery. As I had no real-life experience with organ transplantation before that day in September, I did not know what I had gotten myself into. People in pain? People suspicious of some young researcher who was interested in their stories?

The minute before I entered the seminar room, where a group of local patients and relatives regularly come together and share information and support each other, I got another text message from Philipp:

Dear Resi!

Things are so-so. I'm relieved they haven't found any metastases, but it's going to be a major surgery and they can't keep my leg. The tumor I have doesn't react to any other kind

of therapy; it's really, really aggressive. That's why everything has to happen super-fast right now—which means the 8th of October. It sucks.

Lots of love,
Philipp.

My head was spinning, my heart pounding, my hands shaking, no longer just out of nerves, but now shock. He was about to lose his leg. I had no time to process, I just entered the room, sat there at the table, introducing myself, answering questions and listening to the conversations taking place. My mind drifted. I tried to concentrate, but it came popping back into my thoughts: Philipp. His cancer. His leg. What to do?

I made it through the meeting and I said my goodbyes to the group. But as soon as I left the seminar room, the consternation and helplessness overcame me. Although I had seceded from the Catholic Church years ago, I went into the monastery's church. I sat down and appreciating its quietness and emptiness, I tried to process what was going on. I remember that I lit a candle and read some of the prayers, which were written down on paper and pinned on a corkboard. What I cannot recall is whether or not I wrote down any words myself. But I definitely sent a quick prayer out into the universe. And thus, it happened that on the same day that I was first becoming acquainted with illness and organ transplantation, a deeply unsettling process began.

What might seem too obvious to be overlooked—the first fieldwork encounter coinciding with unsettling news from home—became apparent to me only months later, when I traced back my messages and matched them up with my diary. I was struck by the synchronicity of those events and the fact that I had not noticed it before. Then again, it seemed quite reasonable to me that this had been overlooked: the emotional shock I experienced that day blurred my recollections, and I had other things to think about than the unfolding synchronicity.

At the beginning of October, a week after his text message, I went to Vienna to see Philipp before and after surgery, the first of many trips that followed over the course of the next 6 months. I recall my anxiousness on the way to the hospital and intense feelings of insecurity about what and whom I was about to encounter. I still remember the tension in my body, and how I overcame the urge to turn around and not face him and his family—the minor accomplishment of staying put and keeping going. I can still feel the weight in my legs as I put one foot in front of the other, making my way through the clinic's corridors. I realized afterwards that the pictures in my mind, my imagination “running wild,” had been more unsettling than actually being there—seeing, touching, and talking face to face with Philipp and his family.

I met Philipp in the hospital's cafeteria, and he showed me the huge bump the tumor had formed close to his spine, on his lower back, bigger than my fist. During our conversation, Philipp asked, “Why me,” adding in the next breath, “But why shouldn't it be me?” Talking about the unfairness of the situation, we realized that notions of fairness did not help in grasping what was going on. Suffering from a life-threatening disease, like cancer, is never fair, to anyone, at any time. Philipp told me that he was afraid to die, to not make it through the complicated and highly invasive surgery. Nonetheless, he was also optimistic and hoped the amputation of

his leg would be a big enough sacrifice for the cancer—a sacrifice that this destructive force in his body had asked of him.

After surgery, I visited him again. I saw his damaged body, the emptiness beneath the blanket where his right leg used to be, the cotton sheet lying flat on the bed, the haunting absence. He explained to me how during surgery his doctors cut off his leg and removed the right section of his pelvis, but left parts of the muscle of his upper leg to “build” the pile of flesh he was now supposed to learn to sit on.

Philipp was in pain and all I could do was to be there with him, to be present. I told him about my life in Germany, the beginnings of fieldwork, trying to entertain him with sweet banalities from my everyday life in a situation that was far from banal. Philipp laughed in spite of the pain about life’s ironies and cynicism. As far as possible, he tried to maintain a positive outlook on the future, made plans and refused to allow his life to be defined by his reconfigured body and illness. Philipp was hospitalized for months, and repeatedly developed a fever, the cause of which nobody seemed able to detect. He needed surgery again, suffered from fever again. It seemed like an endless cycle.

He was discharged in December, having been hospitalized for almost 2 months. I tried my best to support him and his family from afar as they suffered because of his amputation and the therapeutic regimen that came along with his cancer diagnosis. During one of our rare Skype conversations, he proudly showed me his Mohawk, pointing out his resemblance to Robert de Niro in the movie *Taxi Driver*. He had shaved off his curls before starting chemotherapy, in an attempt to decrease the visibility of the toxic treatment and regain some autonomy in a situation beyond his control. From time to time as we talked, he would convulse and groan in pain, but he pleaded with me to take no notice of it and carry on talking. When I visited him and his family over the Christmas holidays, it was striking how eager he seemed to get used to his transformed body. Philipp craved a sense of normalcy in circumstances that were anything but ordinary.

In February, he found out that he had developed metastases in his lungs, something that had been indicated in his clinical report back in December, but which he claimed no one had communicated to him. His cancer had spread. He had become metastatic. Philipp’s leg and pelvis had not been sacrificed enough. It did not take long for his tumor to return right where it had started, gradually making its way up his spine, vertebra by vertebra—causing pain beyond imagination.

In her powerful book *Malignant*, Lochlann S. Jain explores the paradoxes of cancer and points out how the disease constitutes “a perfect storm” (2013, p. 5), and how each instance of it “comes with its own unique way of torturing people” (ibid., 38). Philipp’s cancerous body was his perfect storm; it became his very personal torture device. As uncomfortable as it may seem, we *are* cancer—or at least, as the subtitle of Jain’s book points out, “Cancer becomes us.” “My flesh had become the pathology report” (ibid., p. 3), she described her thoughts while receiving her test results. Cancer is many things, as Jain’s book has shown. The metaphors used to describe cancer refer predominantly to battlefield scenarios, to scenes of fighting or being strong survivors. These metaphors obscure an uncomfortable truth about

cancer: it is not an intruding virus that is making us sick, but it is our own cells turning cancerous, growing rampantly, and destroying the body they are part of.

Philipp's cancerous body had decided to do exactly that, with no regard for his life. He wanted to know how he should prepare for death, as it became clear that this really was about to happen, that it had become inevitable. Philipp was skeptical about the idea of being transferred from the hospital to a hospice, reluctant to accept what it implied: leaving a space in which people could be cured, and moving to one *beyond* the possibility of cure, healing and survival. Philipp told me about the helplessness he detected in doctors' eyes, how they seemed unable to communicate the approaching inevitability in a clear manner.

It seems that these kind of conversations—breaking bad news and dealing with patients who face death—are not among the core competencies of Western biomedicine. Jain describes “my doctor's uncomfortable avoidance of the Bad News Experience” (ibid., p. 216). The uncertainties in medicine, the often very individual trajectories illnesses trace, and the recognition of our mortality tend to be issues pushed to the margins of medical training in Western biomedicine (Fox 2000). Death has to be deferred with almost all means possible, and the realization that at some point there is nothing more one can do is also painful for many physicians—something they have to learn along the way with experience. It seems learning to support patients in dying and the importance of palliative care are kept separate from the more dominant conceptualization of what medical practice is supposed to be.

This may have to some extent been the source of the feeling of helplessness that Philipp thought he detected in the eyes of most medical professionals taking care of him. Eager for some clear, straightforward words, he asked me about books, articles, as I surely must have read something about death and dying. He still had this curiosity, his academic mind trying to make sense of the things happening to him. I tried to be there for him and his family, but I had no answers.

Philipp's cancer was indeed a perfect storm, which finally calmed with his death at the end of April. We had talked on the phone a couple of days before, and he seemed weak and disorientated as large amounts of morphine were running through his system to alleviate his pain. When his wife, Maria, called on a Sunday evening to tell me that she was unsure what was happening but that it seemed as if he was “preparing” himself, I immediately cancelled all my appointments for the week and booked a ticket home. Prepare? How? What? It was just at the beginning of the day-long train ride when my phone rang again, and Maria told me that Philipp had died that night, and that she and her baby daughter had been with him when it had happened.

It was early evening when I finally arrived at the hospice where Philipp had spent the last weeks of his life. I remember I was looking for a toilet after I had arrived, and I followed Philipp's mother, who wanted to show me to the bathroom. I recall taking a small step into a room—realizing it was *his* room—the room where his dead body was lying in bed. I forced myself to look straight ahead when I passed by his bed, seeing him out of the corner of my eye, but feeling not yet ready. I felt so unprepared for what I was about to encounter. But after a couple of moments, I

stepped beside his bed and took a look at him, lying there with his hands crossed in front of his chest. I remember how, when I touched him, his hands were already cold and felt stiff, but close to his heart Philipp's chest was still warm. I spent hours next to his deathbed, and late evening I fell asleep in the bed next to him exhausted and overwhelmed, waiting for his mother to return. Candles were lit and scented oils from a lamp covered the slowly spreading smell of death.

I spent the following days at his family's home, preparing the funeral together with his wife and sister. We cried and laughed, listened to songs we wanted to play at the service and danced to the music of Philipp's favorite band, Queen. Amid the tears, laughter and dancing, we organized a colorful and very personal service. Thankful, we said no when some guests asked us whether we had considered doing this kind of work professionally, as they had never experienced a service so beautifully special.

During this week of funeral preparations, I read some of Philipp's diaries, which he had written over the course of the preceding months. In one of his first entries, he referred to his cancer diagnosis as "infantile nightmare." As a child, he believed that because his star sign was cancer, he would sooner or later get the disease with the same name. As it had turned out, his infantile nightmare became reality. For his youngest daughter, who was 4 months old when he died in April 2015, "cancer" has remained an enigma. It still seems highly confusing to her how people could possibly suffer and die from cancer—a crab, a sea animal.

The Space In-Between

The months that elapsed between my receiving Philipp's first cancer-related message and sitting by his deathbed and leading his funeral service were filled with experiences and encounters I had never had before. It was the first time I experienced someone close to me going through a life-threatening illness, and the first time I had painfully honest conversations about the possibility of death and the helpless wish to survive. For the first time, I saw a dead body close up, right next to me lying in a bed, only hours after death. It was the first time I touched a dead body, felt the fading warmth, the stiffness in his fingers—even smelled death's presence.

Over these months, I realized that I could bear more than I had imagined. While I kept getting closer to my perceived limits, those limits expanded, extending my conception of what I was able to cope with. Understanding, as pointed out in the passage quoted at the very beginning of this chapter, "is never born of contemplating the world from afar" (Jackson 2011, p. xiii). It happens in the "subjective in-between" (ibid.), and through my personal experiences I became more aware of how to apprehend illness, death, hospital life, myself and my emotions in the future. This more nuanced understanding has benefited me personally but also professionally, as an anthropologist in the field, encountering exactly these topics.

Philipp's illness and death did not complicate the research process for me, as one might expect in a society where death is considered a disturbance of normalcy; they

had quite the opposite effect. Although my time in the field was “disturbed” and interrupted by my visits to Austria, my overall research process and analysis benefited from these experiences “back home.” These helped me to more fully comprehend the experiences of those affected by a life-threatening illness as well as the experiences of their relatives, and enhanced my understanding of their narratives. The insecurities I had at the beginning of fieldwork about what to expect and how to encounter those affected by life-threatening illnesses became more nuanced, as I was indeed able to relate to some of my interlocutors’ experiences. The confidence I gained by being able to “manage” the events surrounding Philipp’s illness, surgery, and death—although “managing” seems an insufficient term to describe the emotional processes it entailed—helped me to be more focused in conversations, especially during interviews with patients and their relatives.

I spent most of my time during this year of fieldwork—in the field as well as back home—at university hospitals, in the waiting rooms of clinics and at hospital bedsides. Gitte Wind (2008, p. 87) argues that we should be more specific in the way we describe ethnographic fieldwork as it “has become a cliché we often use without much reflection.” Wind points out that in many ethnographies the broad term *participant observation* lacks a detailed description of what it actually entails in specific circumstances. Participant observation in a Swiss mountain village means something different than participant observation in a hospital setting. In the latter case, for example, the term often seems inadequate in capturing the limits and potentials of ethnographic fieldwork.

The limits of participant observation become especially evident in settings where conducting research requires permits and informed consent forms (Hoeyer and Hogle 2014), like clinical settings often do. Informed consent also became an issue in my research, which meant that before conducting an interview I needed my interlocutors’ signature as proof of their consent, confirming that they had received all the necessary information about the research project in which they were going to participate. I had to draft forms, adhering to the ethical guidelines for research with humans, which were drafted for medical or quantitative research, and which were far removed from reflecting the priorities of anthropological inquiry.

The process of explaining and answering questions before talking about personal experiences and creating “critical dialogical relations” (Wind 2008, p. 87), was for the most part aimed at legally protecting all parties involved, but was furthermore a way to create a feeling of trust and safety. From time to time, during interviews but also informal conversations, I shared parts of my experience with Philipp’s cancer, a sharing of personal information that helped to build a bridge to their experiences, connecting my interlocutors’ experiences to mine.

A physician at the clinic asked me once how I protected myself emotionally, as he himself had to learn to distance himself from his patients’ stories, as they became too much of a burden to him. I replied that I had not been “protecting” myself, that I had allowed these stories to come close. I refused to maintain an emotional distance for my own protection, because I wanted people to tell me about their personal experiences with illness. With some, I talked about death, what to expect after dying, hopes, and dreams of the future. Keeping a distance while they opened up did

not seem fair and feasible to me. My understanding of “doing” anthropology is inspired by Jackson’s (2011) intersubjective approach, emphasizing the importance of the relational space opening up between the researcher and the world. I follow his argument that it is through this in-between space that we are able to comprehend the people we encounter, and ourselves, with the necessary complexity, and by doing so, gain a temporary understanding of the other and of oneself in the world.

Feeling It

The discipline of anthropology in itself is characterized by a high level of self-reflexivity, but Spencer (2010) goes one step further by including the researcher’s emotional life in this reflective approach. I agree with her statement that “self-reflexivity is incomplete if it does not include emotional reflexivity” (ibid., p. 32). Asking myself, “How does that situation make me feel?” and more importantly, “Why do I feel the way I feel?” became a way to better understand my encounters and experiences in the field and beyond.

For me, discomfort became the most interesting emotion at work in the process of emotional reflexivity. Following an uncomfortable feeling and trying to trace its source became an insightful tool for reflecting upon and questioning the things I was used to. Uncomfortable confusion diminishes or even vanishes with routine, with the recurrence of situations, pointing to the importance of temporality in this regard. Discomfort, or the lack thereof, in situations which had elicited it in the past, points to the impermanence of our reactions to certain unfamiliar situations. It points not only to an increase in the knowledge, but also to the process of becoming accustomed to circumstances—a process at the very core of the anthropological endeavor.

At the beginning of fieldwork, hospital settings were *not* familiar to me. I was not used to people’s scars, their afflicted bodies, tubes and drains piercing them, liquids flowing in and out of artificial openings. While hearing about these things was challenging to me, seeing them was even more so. Similar to the anxiety I had felt before meeting Philipp at the hospital, meeting some of my interlocutors for the first time involved considerable discomfort on my part: sitting next to hospital beds whose occupants’ bellies were punctured, with liters of liquid flowing out of their abdomens via tubes into bulging bags on the floor. I recall touching those bags, as I was asked to move them a little to the side as the flow of the fluid seemed somehow blocked. It also made me uncomfortable when people pulled up their hospital gowns to show me their scars from surgery. But nonetheless, I also remember how these moments of discomfort reduced over time.

While at the very beginning of fieldwork, hospitals seemed like very strange places—places one usually avoids—those strange places and their atmosphere soon became familiar. While at the beginning I was hesitant and insecure during my encounters with patients and relatives, questioning myself about how I could relate to their experiences of pain and suffering, of dealing with a life-threatening illness,

this hesitation gave way to a confidence that I could *in fact* relate to some aspects of their experiences.

The continuous process of reflecting on my emotional responses and remaining attentive to feelings of discomfort was not only a useful guide to becoming aware of and protecting my personal limits. More importantly, it served as a meaningful “tool” of ethnographic fieldwork for gaining a better understanding of the human condition. Because of my experiences with Philipp, I felt more comfortable talking with people about bodily limitations, their fear of surgery, the possibility of death, the haunting questions of what to expect when life ends. Topics I had brushed aside before fieldwork suddenly occupied considerable space in professional and private conversations. And the discomfort that the lurking presence of death had caused me *before* fieldwork was replaced by the deep conviction that by delving into these very existential questions, by facing one’s temporal limitedness and bodily fragility, a deeper understanding of life and one’s place in the world is possible.

I understand the emotional reflexivity as a meaningful “tool” of the intersubjective approach (Jackson 2011). Intersubjectivity urges us to be attentive to the space in-between subjects, in order to gain a deeper, if temporary, understanding of the other and oneself. It highlights the importance of the relational space that opens up between the researcher and the world. Just as private encounters are enmeshed with our emotional inner lives, so too are professional ones. This is especially the case in research contexts where the boundaries between professional and private lives tend to blur and dissolve, as they so often do in ethnographic fieldwork. Because of these relational characteristics of ethnographic inquiries, I consider emotional reflexivity a meaningful methodological and analytical tool for the practice of social anthropology—especially when working with people who face life-threatening illnesses.

Concluding Remarks: On Hubris and Hybridity

I came so close to illness and death during these months of fieldwork that I reasoned I had to prepare myself emotionally for instances of sudden death among the people I loved. At some point, I thought I had accepted mortality—mine and that of others. I remember sitting on the train, thinking, “Okay, that is how it is, death is part of life,” while in the next moment being shocked at the pragmatism of my thinking. What hubris! The fear of cancer and dying came crawling back, haunting me in my dreams when I was back from the field, back in academia, back at the university preparing papers and panels.

Stressed and questioning everything I was doing, I woke up again and again from nightmarish dreams. Once I was diagnosed with cancer and had only a few days left but nobody seemed to care. Once a tumor in my mother’s throat had returned, taking over her body, threatening her life. Was that how I wanted to spend my time, my life? Being a stressed academic trying to make sense of such an existential topic? What if I really was about to die? What if my mother was about to die? Or my sister? What would I be doing? And on the other side of these haunting questions, the

intangible understanding that all these people so dear to me and I, all of us, are sooner or later going to die, that there was no way around that, no escape. Not once over the course of my fieldwork, when illness and especially cancer were so close to me, did I have nightmares like these, and although they disappeared after a couple of months, they made me aware that my hubris was fallacious. I realized that although I had thought, read, and talked so much about death and dying, the issue was not resolved and probably never would be.

If I were to keep the experience of Philipp's cancer and death apart from research, my being-in-the-field and the analysis of my material would be unreflective and insincere. The synchronicity of my personal and professional engagement with illness and death seems too substantial to be overlooked and ignored. I am convinced that doing fieldwork and working as an anthropologist are often a hybrid venture of the professional and the personal, that the line between private and work life often becomes blurred. Consequently, our experiences in either of those spheres affect and influence each other.

Because of this interrelatedness, I am convinced that my experiences back home in Austria influenced my research. Although they were intensely challenging, I feel confident in saying that they enriched my fieldwork, my empathy, my being-in-the-field, my understanding and analysis; they enabled me to more fully comprehend the experiences of people affected by a life-threatening disease. The confrontation with Philipp's illness and death affected my views on living and dying and my emotional capacity to grasp patients' experiences; conversely, my interlocutors' stories also helped me in my conversations and encounters with Philipp and his family. This interrelatedness is not only a characteristic of anthropological fieldwork; it is an essential part of the intersubjective approach. It points to the junctures where the subjective lives of the researcher and those being studied fold in and out of each other.

What I took with me from those encounters with death was an awareness of being alive. As pretentious as it may sound, being aware of one's mortality helps to put things into perspective. I decided I would not put death into a hidden corner of my mind, ignoring it so it could hit me even harder when it inevitably appeared, but would instead try to accept the uncertainties that come with being alive. Bauman (1992) argues that mortality is such an essential part of our existence and our imaginations that the *overcoming* of it serves as the driving force of human culture. We create to transcend our temporal actualities. What I aim to do by writing this chapter is not only to create a text to transcend my own temporal situatedness and boundedness, but by including Philipp's story, I aspire to take him along with me.

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Standing at the Doorstep: Affective Encounters in Research on Death and Dying



Natashe Lemos Dekker

Anthropological writing about death has to be, if not an ice-axe to break the sea frozen inside us, at least an ice pick to chip at the conventional forms of representing and narrating the encounter of the anthropologist with death. (Behar 1996, p. 86)

Introduction

Ethnographic fieldwork, it could be argued, is always an emotional endeavor (Davies 2010) replete with unsettling encounters, and moments when anxiety and joy coincide. While the reflexive turn in anthropology sparked lively debates on fieldwork ethics and the positionality of the ethnographer, these topics should be further considered from perspectives that take the emotional aspects of ethnography seriously. This chapter addresses how emotions feature in the ways we, as ethnographers, position ourselves as moral actors in the field. It does so by taking the values ascribed to emotions and the emotional components of moral value seriously (Lutz 1998) and by taking a close look at the researcher's attunement to interlocutors and situations during ethnographic fieldwork.

I address these specificities in the context of ethnographic research on death and dying, though the discussion is also pertinent to ethnographers working in other settings that are, to a greater or lesser extent, emotionally charged. Researching death and dying has been characterized as a sensitive endeavor (Borgstrom and Ellis 2017; Woodthorpe 2011), yet few authors have addressed their own emotions while doing research (e.g., Pool 2000; Visser 2017; Woodthorpe 2011). "Emotion and the personal involvement of the researcher are undervalued in social scientific studies of death," Pool (2000, p. 16) writes, underlining that emotions play an essential part in the research process as the researcher interacts with their interlocutors. This also became apparent to me during 18 months of fieldwork—in three different nursing

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homes as well as during individual interviews in the private sphere of the home in the Netherlands—where I focused on the moral constructions surrounding the end of life with dementia, by scrutinizing ideas of a “good” death, dignity, and suffering. Sensitivity, awkwardness, intense intersubjective bonds, and ontological questions of human existence were at the core of this ethnographic endeavor.

With this chapter, I will not attempt to answer the question of how to do research on death and dying, but rather aim to demonstrate the need to come to a better understanding of the affective states that come with ethnographic encounters. Thinking of ethnography and ethnographic writing in Karen Brodtkin’s words, as a way of “linking our stories to larger stories” (2011, p. 21), my aim in describing some of my own experiences is to relate to ethnography as a practice, tradition, and personal passion. I would like to underline the importance of writing about and through emotions, in line with Robert Bellah’s statement that “knowing in the human studies is always emotional and moral as well as intellectual” (1977/ 2007, p. xxxi). Negotiating, for example, the insider–outsider position that is inherent to participant observation and through which my positioning in the field with regard to the people I am with is formed, cannot be seen as a purely intellectual process. This is to say, that deliberating over the extent to which ethnographers can engage in certain topics, with certain people, and in certain situations is not only a matter of moral considerations which have been debated extensively in anthropology, it is indeed also a deeply emotional matter.

Emotions are intrinsically related to value and morality. On the one hand, moral judgment is often expressed in words which describe emotions, pointing to the importance of emotions in how moral claims are communicated and negotiated. On the other hand, morality and emotions are intertwined in the sense that “morality requires emotion because affect provides the motivation for taking particular moral positions towards events” (Lutz 1998, p. 76–77). Here, I build on Lutz’s approach to explore and reflect on the intertwining of emotions and morality in how we navigate and position ourselves in fieldwork. How, then, do the emotions we experience motivate moral considerations in the field, and how can the researcher’s moral positioning become emotionally expressed?

Writing about the emotional layers of ethnography, I find, requires the ways in which ethnography moves us to be taken seriously. In *The Cultural Politics of Emotion*, Sara Ahmed writes that “emotions involve (...) affective forms of *reorientation*” (2004, p. 8, emphasis added). “Being moved” through the emotional force of an ethnographic encounter also means moving toward or away from people and situations, and can become a sign to do things in a different way or even withdraw. Emotions, then, are central to ethnographic encounters and the negotiated positionality of the researcher.

In the following text, by reflecting on my ethnographic fieldwork in nursing homes in the Netherlands, I will demonstrate some of the emotional and moral entanglements that came with researching death and dying. I go on to show how my own movement through fieldwork—my own *attunement* to situations—has been formed via emotional encounters. As Jason Throop writes, “the ethnographic encounter is an encounter that is often defined by the recurrent frustration of the

anthropologist's attempts at achieving an intersubjective attunement with particular interlocutors" (2012, p. 85). I understand attunement here as informed by emotions and morality, and as referring to the way I, as a researcher, relate—emotionally, morally, physically—to my interlocutors and fieldwork itself.

I found attuning especially important due to the levels of intimacy, taboo, and the sensitive nature that the topic of death and dying is often charged with. Being there with bereaved family members saying goodbye to a loved one, I took part in what I considered to be intimate moments. The rawness of the confrontation with death, and the profound absence of presence visible in the inactive body of the deceased, was sometimes also accompanied by uneasy relief to suffering (Lemos Dekker 2018). The confrontation with death and loss oftentimes triggered grief and sorrow related to previous experiences of loss, finding family members and professional caregivers grieving past and present losses in a moment of emotional entanglement. Moreover, it also stimulated the realization of finitude, both in interlocutors and myself, and with it the anticipation of losses to come. While witnessing the end of people's lives during fieldwork, I realized it was always the end of *a* life; that the dying person in front of me had unique meaningful relations, personal habits, and ideologies. I reflected on and fantasized about how their life used to be, a life filled with dreams, emotions, and perhaps secret desires, and was struck by the complexities of a being coming to its end. This rawness, our responses to loss, to be left in nothingness, and "the sense of death as no longer abstract possibility but present actuality" (Fairfield 2015, p. 14) provided the context and matter of my research, as well as the lens to think through the emotions that accompanied it.

While a part of what happens in death will remain incomprehensible, as ethnographers we look at and explore death through its manifestations and affects—we study the social and cultural structures surrounding death, its rituals, the experiences of grief, anticipation and loss, and how relationships are made and unmade. I will elaborate on this by reflecting on the emotional and moral entanglements in my own navigation through the ends of residents' lives in Dutch nursing homes. By using the plural form "we," here I position myself in a larger community of ethnographers. This is not so as to generalize my experiences, but to serve as a reminder that the experiences I share may speak to others' concerns and can be seen in line with central methodological debates within anthropology, of proximity and distance, the limits of empathetic understanding, and moral considerations in doing fieldwork.

Participating in the End of Lives

"Being there" in a nursing home meant spending hours sitting in the living room, drinking coffee, or staring out of the window with residents. Marveling at the color of the leaves and life outside alternated with moments of shared boredom. I took part in meetings between the general practitioner and family members wherein the well-being and treatment options of residents were discussed. From time to time,

care workers would ask me if I could lend a hand by, for example, taking a resident to the hairdresser, which was located on the ground floor of the building. In the afternoons, I often walked through the nursing home hallways with residents, passing the coffee corner with the birdcage, the photographs of the market, the antique clothes dryer, and again the coffee corner with the birdcage—over and over again. During these walks, residents would recount parts of their lives and talk about their work, their families, and memories. Sometimes, these were entire stories, sometimes fragments, and sometimes sounds that made no sense to me mixed with words that encouraged me to interpret and read between the lines.

When the end of a resident's life drew nearer, I spent most of my time sitting at their bedside, sometimes with family members and sometimes by myself. At the bedside, family members shared with me stories about the dying relative, who she was and the life she had lived. While I could not engage in dialog with the person with dementia herself, I could listen to the stories told by family members and the professional caregivers caring for her. Often, the dementia had advanced to such a level that verbal communication was challenging or in some cases impossible, especially when morphine (administered to manage pain) had reduced consciousness even further. Thus, I wasn't able to know how the person with dementia experienced the process of dying, how she was facing the end of her life, if she was afraid or not, and which values mattered most to her. As such, I tried to get to know the residents of the nursing home and their family members and to establish a connection as much as possible, before the process of dying started—so I would be a familiar presence for them and they for me at the end of their lives.

Once, after spending a couple of hours with Anna¹ in her mother's room, she took a book from the shelf and sat down beside me at the coffee table. It was a printed photo album that showed pictures of her mother, both from the past and from the present in the nursing home. Together, we browsed through the pages and talked about her mother's life. She characterized her mother as a proud woman who cared about her appearance. The next morning, Anna changed her mother's earrings, applied eye-shadow, and dressed her in a pink shawl. Her mother, who was in her final days, was bedridden and her eyes were already closed. Anna mentioned that pink was her mother's favorite color. Gaining insight into the lives being lived in the nursing home and the histories that went with them helped me to understand what mattered to the person with dementia and their family at the end of life.

Participating in the end of people's lives also came with specific complexities. I learned that the specific context of the end of life of people with dementia requires a thoughtful approach to establishing contact. The researcher must take a careful step closer and rely on nonverbal communication, such as physical contact, body language, and facial expressions. Often, when I visited a dying resident, the question of whether it was appropriate, emotionally and morally, at that time to enter the room arose. As a result, in an unexpected way, the doorstep became a metaphor for my engagement with sensitivity, proximity, and distance, and being respectful. Although a physical doorstep is often absent in nursing homes to allow for wheel-

¹I have anonymized interlocutors' names for reasons of confidentiality.

chairs and beds to make their way through the building, there is a clear, symbolic border—a door, a line between different colors on the floor—between the semipublic space of the nursing home hallway and the semiprivate space of the resident's room. The doorstep came to symbolize the emotional and methodological negotiations of involvement and detachment. How close are you allowed, how close do you dare to go, and how close is close enough to be empathetic? And what distance is enough to be respectful?

One occasion in which these questions arose was 3 days before Mrs. Van Doorn passed away. I was sitting at the kitchen table waiting for the doctor to arrive when her son Henk entered the unit. He sat down next to me and said to me, "This is not what one wants." Upon which I asked, "What would you want?" He answered, "That it would be over soon." He told me about the death of his father years ago, but then added, "*This* is completely different, she is screaming from pain and suffering." We looked at each other without saying anything more. When the doctor arrived, the three of us walked to the room where Mrs. Van Doorn was lying in bed. I waited at the doorstep as they entered the room. The doctor lifted the blanket and uncovered Mrs. Van Doorn's left shoulder to check if the morphine needle was still in place. It was a small needle with plastic tabs on either side—I now understood the name "butterfly needle." Henk asked the doctor, "How long?" And the doctor replied, "Before she dies? That is difficult to say. I do see that her functions have decreased and she could die of that, but still it is hard to predict." In the meantime, Henk's sister Marta who I had briefly spoken with that morning arrived. While greeting everyone with a smile, she entered the room and took a chair to sit by her mother's bedside. Just before he left, the doctor told them this was all he could do for now, and that he would be back to check up on Mrs. Van Doorn later in the day. Then, Marta looked at me and said, "Come in, take a chair," and pointed to a chair by a small table in the corner of the room. I took the chair and placed it at the end of the bed.

When Marta asked me to come sit with her by the bedside, she accepted my presence and involvement. I had waited at the doorstep, because I considered this a deeply personal experience. I was anxious about imposing in such a personal moment. Waiting for interlocutors to invite me in became one of the ways I was able to negotiate my position as outsider and insider. I also hoped that my reserved attitude would demonstrate respect.

I constantly negotiated how to approach moments that might be experienced as private or sensitive. I find it hard to describe what sensitivity precisely entailed or what it meant to have a sensitive approach. For me, it was about being attuned to the emotions of the other and myself in the moment, being able to sense the emotional interactions and what matters most in that specific situation. It was about seeing and listening, as much as about voicing the things I was not sure about.

One afternoon, after Anna and I had spent several hours talking by her mother's bed, I was just about to go home when the general practitioner informed me that there would be an unexpected family meeting with Anna's brothers and sisters. We walked back upstairs to the unit where the family was waiting and the general practitioner asked them if it was all right if I was present during the meeting, to which

they agreed. After looking for a place to sit down in the living room, which was too crowded with residents, we moved to a table in the hallway. At first, I decided not to take place at the table but sat down on a chair by the wall to seem less obtrusive, upon which Anna said, “No, come and join us, you are also a part of this.” While taking a seat at the table, I experienced an increased self-awareness—as if my expressions, how I moved, and my presence all mattered at that moment. As I took my notebook from my bag I felt the need to place it open in front of me, an attempt at being transparent toward the family, making visible what I was writing and what I wasn’t. I could see that Anna and her family were somewhat agitated and restless, as well as very serious in that moment, and being there as a researcher I did not want to cause more harm. In this way, attuning to my interlocutors and the situation was both a moral consideration and about the emotions I experienced and those that I perceived in others. My response—taking out the notebook and placing it in front of me—was a form of self-discipline borne out of a deep concern for transparency, which resonated with Paul Rabinow’s remark that the anthropologist is required to be aware of the codes of conduct and should control herself, adapting to the situation, accordingly (1977/ 2007, p. 47). In this way, attunement was also expressed by holding back, standing waiting, and not wanting to impose my own presence. Being moved, emotionally, was closely related to bodily movement in deciding how, and to what extent, to engage in interactions. There is no blueprint for encounters like these, but I tried to conform to the setting and the person in front of me.

At the doorstep, a range of emotions and deliberations came together. Respecting this cultural boundary between outside and inside, or as was my intention, also meant respecting the private sphere of the family witnessing the final moments of the life of their mother or father, husband, wife, or sibling. Also when there was no actual doorstep, there could be “doorstep situations” that entailed emotional and moral thresholds, such as in the meeting described above where I considered whether, and how, to sit with Anna and her family at the table. It was a point at which I considered my own invasiveness in the moment, and whether my presence was accepted and appropriate. It was a threshold, perhaps more for myself than for the people I was with, which highlighted my own discomfort. Would I dare to step over my own worry of being invasive? This deliberation, however, could not take too long, or it would acquire a voyeuristic character. In this sense, the doorstep presented a clear choice: step inside and fully engage, or walk away. Doing otherwise, standing outside while peering in, was not an option. And thus, I entered.

We are sitting all around her while she is lying in bed. She, Mrs. Van Doorn, is dying. On her nightstand an old photograph, of her first husband I am told. Mrs. van Doorn was born on the 3rd of September, 1919 in a rural area in the south of the Netherlands. She was the oldest of fourteen children. She started working at the age of fourteen and met her first husband at nineteen. They married and had seven children, of which two passed away at a young age. Mrs. van Doorn knew many losses in her life, she lost her husband in 1982, and later another son and two grandchildren. After many years she met Martin, with whom she enjoyed traveling and with whom she lived together before moving to the nursing home. Next to the photograph, a plastic cup with water, with a small stick with a green sponge on the end, to keep her mouth moisturized. She gasps for breath, while we, her daughter, granddaughter, son, the spiritual counselor and I fix our eyes on her. We look at her and at

each other, but not for too long. It is as if we fear we might miss something, her final breath, or whatever comes. No sounds are entering the space except for Mrs. Van Doorn's breathing. We talk in a low tone of voice, almost as if speaking under our breath. The daughter Marta, who is sitting next to the bedside and has her eyes still fixed on her mother starts moving on her chair and clamping her hands together, while she says "no not yet." She seems to panic, while the pace and rhythm of Mrs. Van Doorn's breathing becomes slow. Then, a long pause, we hold our own breath and wait. Will this be her last one? No—a new burst fills the air and her daughter startles. Then, once more, a deep breath and Mrs. Van Doorn has passed away. Marta holds her mother and presses her lips on her forehead while her sobbing becomes louder. While at first we were all sitting and the room was filled with tension, now there is movement, we are holding each other, shedding tears and saying our condolences. Besides the sadness, tension has given way to a kind of relief. (field diary, author)

Awkwardness, Sensitivity, and Intense Bonds

While sensitively balancing distance and proximity often resulted in in-depth connections, it was at times also paired with awkwardness, and sometimes also resonated with humor. One Monday morning, I entered the nurse's office and asked cautiously if any of the residents' condition had worsened or if anyone was dying. I was aware of my own discomfort in posing this question, and I tried to convey this consciousness in the manner of asking. Perhaps, reflecting on this now, what I was trying to do was to soften the question for both the nurses and myself. Although it was not as if I wished anyone would die, I feared my asking would give that impression. Still, I had to ask, first because I could not visit every individual room (the nursing home consisted of 14 units with six residents each), second because I did not have the knowledge to assess someone's condition—perhaps the person was just staying in bed for a day—and third because knowing someone's condition had worsened or if the person was dying required a different, more modest and sensitive approach while entering their unit. In her response, the nurse joked: "Well, that's a nice question to begin with on Monday morning!" Both my own discomfort in asking and the nurse's response, although with humor, implied that death should not be asked for. Even though death and dying are part of everyday life in the context of the nursing home, it remained a topic of conversation that could evoke discomfort or giggly responses. This resonates with Glennys Howarth's observation that in modern societies, "death is confined to medical or scientific discourses; anything outside of that is taboo or viewed as "pornographic." [Death] has been removed from the public realm and placed firmly within the private sphere of the family and individual" (2007, p. 16).

As such, I feared the people I met would perceive my interest in death and dying as voyeuristic, as if I was looking at something one is not supposed to look at, invading another's intimate and personal sphere, and thus feeling inappropriate (e.g., Visser 2017). For example, after spending a month or two in the nursing home, professional caregivers started associating me with the topic of death and dying. This was valuable since it often created a space wherein professional caregivers

would start talking about the subject themselves, reflecting on past experiences and current situations, or at times would approach me to tell me when a resident's condition had worsened. But being related to the topic of death and dying could also feel troublesome given the morbid nature of it, even if it was packaged with humor. Once, upon arriving at a unit, one of the professional caregivers who saw me entering exclaimed, "Uh oh, who's dying?" and we both laughed at it.

I have also removed myself from encounters with family members, sometimes because I sensed they were unable or unwilling and at other times due to my own discomfort. During one cold morning in December 2015, I arrived at the nursing home, finding team manager Isabelle in an afflicted state behind her desk. In a high trembling voice that was unusual for her, she told me Mr. Langedijk had fallen in the bathroom only a few minutes before and had died. She looked at me in panic and continued in a rushed tone, "They [the care workers] found him in the bathroom, but we don't know what happened. If he died and then fell, or if he died because of the fall. They are so confused and upset." Together we walked toward the unit. Upon arriving, the care workers were busy attending to the needs of the other seven residents for whom they had to care. Nicole, one of the care workers, told me she had already notified the family; they were on their way. Her face was red and I could see that she had been crying. About an hour later, Mr. Langedijk's daughter and her husband arrived. Isabelle briefly introduced me. We shook hands and I expressed my condolences. Isabelle continued talking with the daughter about what happened. Without being able to pinpoint exactly why, I felt slightly uncomfortable in the situation. When Isabelle had finished, she returned to the nurses' office.

Mr. Langedijk's daughter, her husband, and I stood in the empty corridor just outside Mr. Langedijk's room. On their faces, disbelief and shock, but also something which I cannot quite describe, it was not quite anger, but a certain sternness. As the conversation progressed, my initial feeling of discomfort increased. Both my questions and their answers became shorter. I wanted to end the conversation and leave the couple to themselves. I thanked them for their time and expressed my condolences once more.

Reflecting on this encounter, I have tried to dismantle what it was that made me stop: Was it the look on their faces? Their manner of talking and interacting with me? Or was it my own discomfort in asking questions at a moment where I assumed they could be out of place or inappropriate? The moral and the affective were inextricably interwoven in this situation. While it was difficult in these instances to pinpoint the specific reason, it became clear to me that, as Thomas Stodulka writes, "as embodied products of researcher-researched interactions, emotions may either motivate or discourage further engagement" (2015, p. 86). Considering that emotions move us (Lutz 1998; Ahmed 2004), they move us toward actions and toward others, but they can also move us away from a specific person or setting. Whether to withdraw or engage is a moral consideration as much as it is driven by emotional experience, and whether and how we attune and make sense of them as affectively aware anthropologists.

At other times, as was the case with Mrs. Van Doorn's family, intense bonds emerged, perhaps precisely because I was not part of the family or a member of staff, and because I had the time to be present and listen. My position as a familiar stranger at times facilitated discussions about sensitive topics which might have been harder to talk about with family members or others who had a personal stake in the process. Many interlocutors explicitly told me they appreciated my presence and our conversations. Some expressed their gratitude for me being there while I expressed I was the one who should be thankful. One evening, I was preparing to go home to have dinner when Marta asked, "But you are coming back, aren't you?" That evening I had dinner in the nursing home's cafeteria and quickly returned to the unit, not wanting to disappoint her. Creating a space to discuss death openly was oftentimes valued and experienced positively by interlocutors.

Hence, researching death did not only bring awkwardness and discomfort. Engaging in conversations about death and dying, going beyond discursive taboos, also enabled in-depth connections, even if doing so required working through the discomfort. Having such encounters that allow for vulnerability and trust fostered meaningful relationships. In some cases where I did not know the family before we spent time at the bedside, the relationship was forged in *that* moment. Being present at such a defining moment in one's life strengthens a relationship precisely because this moment is shared.

However, I also noticed there were limits to how vulnerable I could be, and how much emotion could be shared. Several times I felt the need to suppress my own tears while standing at the bedside with grieving family members. Why did I do this? Did I not want to appear vulnerable? Or did I feel it was not my sorrow to express? Reflecting upon this now, from behind my desk, I feel the loss belonged to the family, as if there was a certain legitimacy to feeling it. They were the ones losing a loved one. Can we then, even if we allow ourselves to be vulnerable, understand the grief and pain of others? While I could understand their suffering because they were losing a loved one, and could also feel sad because I was witnessing a person dying, at the same time there was a distance, as it was not me who was losing someone. I understood the pain but did not experience the loss (e.g., Pool 2000). Such limitations in understanding interlocutors' emotions have been discussed by Renato Rosaldo (1989/ 2004, 2014) who describes in his classic essay "Grief and a Headhunter's Rage" how he only learned to understand "rage" among the Ilongot after experiencing the loss of his wife in an accident during fieldwork. He writes that only after experiencing a loss of his own could he appreciate the "powerful rage Ilongots claimed to find in bereavement" (1989/ 2004, p. 168). Helpful in engaging such limitations is Ahmed's (2004) point that feeling sad about another's pain is a form of alignment with the other, but a form that works by differentiating between the other and the self. This is about recognizing my own emotions instead of equating them with those of interlocutors. My emotion emerged from seeing someone dying and was mediated by witnessing the family's sorrow and my own ability to empathize with what it means to lose a loved one. But, obviously, I did not feel the same loss.

A Final Note on Writing About Death

I had to figure out how to observe and participate in the process of dying, to learn at which moments I could be present and when it was time to withdraw. However, the preoccupation with sensitivity does not end with fieldwork. For me, ethnographic research is as much about writing as it is about the encounters I experienced. Doing this, however, requires a return to emotions, picking up the ice-axe Ruth Behar called upon. In writing about death and dying I have sought to transfer the charged nature of loss into words that do justice to the complexity of encounters and emotions. I have tried to convey the difficulties and strong connections I encountered, as well as the discomfort and gratitude I felt during my research, to explore how emotions feature in the ways we, as ethnographers, position ourselves as moral actors in the field. This brings to the fore a togetherness of contradictory emotions, impotence, awkwardness, and humor that enabled connections while researching the end of life.

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From Therapy to Fieldwork: Reflecting the Experiences of a Therapist and Anthropologist when Researching Substitutional Drugs and Their Users



Tereza Baltag

Introduction

Did Sarah have to die?¹ This question crossed my mind many times during my fieldwork research on Subutex pills and their users in the capital city of Prague, Czech Republic.² Subutex was developed with the intention of substituting heroin. Its users are clients at healthcare facilities, but also junkies on the open drug scene (ODS)—sites that are known for their high incidence of drugs, drug users, and more obvious drug use. While a patient at the Healthcare Center Gestalt on the Subutex Maintenance Treatment (SMT) program provided to opioid addicts, Sarah frequently visited these places, often having consumed other addictive substances. I met her at the healthcare center in 2012, when I became her therapist. In 2015, 1 year after Sarah's death, I left the comfort of my counseling office and set out onto the ODS as an anthropologist, motivated by many unanswered questions pertaining to the field of substitutional therapy and its benefits for addicts. In the midst of the ODS, I realized that my relationship with Sarah was still not resolved. I had to face the same kinds of questions and feelings that I was forced to face immediately after her death: is such a destiny unavoidable?

On the ODS, there was one thing in particular that I had to struggle with. I did not have a team to share my thoughts with, to make jokes with, or to ask for help from. I did not have colleagues with whom I could immediately discuss what was on my mind. So I began to wonder, how do anthropologists work with the emotional challenges they face in the field? Can the reflection of emotionally challenging moments tell us something important about the issues we are studying? I attempt to

¹I have anonymized personal names and the names of healthcare institutions.

²This is the commercial name of the preparation carrying the active substance of buprenorphine.

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answer these questions by reflecting on my own experience of my emotional trajectory from the therapy room to the ODS. The reflection of my feelings throughout this trajectory helped me to better understand not only some of the aspects of Sarah's life, but also opened up new perspectives on SMT and the limits of healthcare in Prague, as it excludes some of the people who are supposed to benefit from it. The disciplines of anthropology and psychotherapy have something to offer to each other not only in terms of their theories but in practice as well. I argue that anthropologists can learn from psychotherapists in acquiring a sensitivity toward their own emotions and toward the motives underlying their work. This can enrich the research process, its outputs, and the ways in which researchers handle the countless challenges they encounter during this process.

Some Similarities Between Psychotherapy and Ethnography

Medicine is not solely a technical activity based on biological and biochemical knowledge; it implies also a moral intervention grounded on values and expressing sensibilities, with claims of altruism by professionals and expectations about the role the sick should play in the management of their illness (...). (Fassin 2012, p. 16)

Suffering, hardship, illness, and people who find themselves in difficult life situations are often the focus of both psychological or medical anthropologists and psychotherapists. The situations in which one finds oneself when practicing these professions can challenge one's emotions and relationships; these challenges are often intensely discussed in the psychotherapy profession, but less so when one is preparing for anthropological fieldwork.

During psychotherapeutic training and in various courses taken by future therapists, one is prepared for emotional and other challenges.³ The study, over several years, required for working with people and among people should prepare future therapists not only to handle and recognize the emotionally trying relationships and moments that arise in this profession, but should also inform the therapist about the support system that they can rely on, and should rely on in practice. Support can come from meeting with colleagues, continuous education, or the supervision of the therapist's work. Such systematic support in working with people should not only serve the therapist as a buttress in their profession, but should also protect those whom the therapist works with. If, during the course of their work with a client, the therapist consults on the therapy process, there is a much smaller risk of various types of omission or error. Such omissions or errors do not necessarily have to be the result of the therapist willingly wanting to hurt the client, but may simply result

³These courses focus on mental hygiene, stress management, how to work with various types of difficult clients (e.g., how to recognize who is a difficult client and why), and working in environments that are considered especially mentally challenging (and understanding why a person would want to work there).

from human nature, replete with nonreflected wishes, motives, and tendencies, to which therapists themselves are susceptible.

Many anthropologists whom I have met deal with issues that psychotherapists also often confront in their work: how to enforce the boundaries of a relationship (What are the limits of our cooperation with our interlocutors?); how to separate one's professional life from one's personal life; and how to cope with unpleasant moments in the field, such as the sorrow and the injustice we may witness?⁴ In her study of postdoctoral anthropology students, Reis points out that most anthropologists mention mental problems related to field research and the lack of opportunities for them to share or discuss this distress (R. Reis, personal communication, June 11, 2015, Medical Anthropology Young Scholars Conference, Amsterdam).

While psychotherapists enter the workplace armed with self-knowledge (acquired in their training and gleaned from the studies they have read) and with support in the form of systematic supervision and continuous education, anthropologists embark on their fieldwork equipped with a sensitivity toward social relationships, a respect for cultural difference, and an openness toward the relativity of predominant social concepts. This, at least, was my impression of these disciplines after finishing my master studies of in both subjects and completing my psychotherapy training.

The anthropological perspective on (for example) social relations underlying the development, prescription, or use of medicine is a perspective that is not so pronounced in psychotherapy, but which therapists working with SMT patients could profit from. This divergence, as well as the wish to see something of the lived reality of those I spoke to in the comfort of the therapy room, stood at the root of my decision to examine substitution drugs and their users from a different perspective: I decided to go back to my anthropology studies as a postgraduate student. As a psychotherapist, there was a certain dimension of the Subutex lifestyle and of its users that I could never get to know as closely as I wished. Persistent questions surrounding the practice of substitution treatment in Prague, and answers that never came to light in the therapy room, eventually led me to return to anthropology, which I had studied before studying psychology and undergoing psychotherapeutic training.

On another note, the similarities in working in both these professions with people who confront difficult life conditions have led me to start reflecting more on how I feel as an anthropologist in the field, and what I have dealt with since I found myself alone on the ODS in the role of the anthropologist. Furthermore, I have wondered if reflecting on emotions in the field can be as beneficial as the reflection of emotions in therapy.

⁴There are also many positive aspects of both professions—meaningful moments, funny situations, nice relationships, new experiences, adventure, and the feeling of empowerment that one can change something or help somebody who is in need, etc. But these moments are not the focus of this chapter.

Sarah

“There was some sort of fire last night at the homeless shelter,” said one of my colleagues during my first days working as a psychotherapist with SMT. “Several people died, and I think Sarah could be one of them.” I had been at my office for only a few days and was not used to these kinds of statements. I never had been in contact with homeless people, and I had never thought that doing psychotherapy also entailed dimensions such as homeless shelters and fires. A few days after this conversation, Sarah was in the waiting room of our office. “You have a client in the waiting room,” my colleague said, “This is the girl we were worried about during that fire at the shelter.” As an inexperienced psychotherapist, I was anxious before each consultation, but this one was already marked by a specific air of death, homelessness, and fire. I welcomed Sarah to the office. She came because she wanted to be readmitted to SMT, to get a prescription of Subutex that she could later pick up at the pharmacy. Therapy was one of the conditions for entrance into SMT, and I was to be her new therapist.

Sarah was open and talkative, very warm, and polite. She was 32 years old when I met her. She had been a drug user since the age of 12, and had been using drugs intravenously since she was 13. She had started with methamphetamine, but from the age of 14, she combined speed with opiates—heroin. She was without a stable income or home. Her three children were in the custody of her parents. She had no contact with them. She evoked my sympathy, but also caused concern—she had many health problems, her living conditions were bad, and she did not have anyone who could protect her on the streets. We met once a week for 3 years. We talked about her social situation, drug use, her feelings, her worries, her relations, her family issues, or the difficulties related to living on the street, always at my office.

Subutex

Sarah had come primarily to get a prescription for the substitution drug Subutex. Due to the relatively small amount of substitution centers in Prague, Sarah could not really determine the conditions of her Subutex distribution, and so the threshold to her treatment was high, full of rules and conditions that had necessarily to be met in order for the substitutional drug to be administered.⁵

Subutex is a substitution opioid distributed in pill form. In the Czech Republic, Subutex began to be distributed in healthcare facilities and medical and psychiatric practices in 2001. Substitution should, at the right dose, reduce the desire to use heroin and substitute for its use (Lintzeris et al. 2006). Subutex should thus help

⁵The various institutions in Prague differ in their treatment regulations, which are, to a certain extent, determined by the institution itself and/or by its employees. Legislation sets limits for the treatment, as well as the licensing of institutions in a given area, as do various political aspects (e.g., the willingness to finance certain treatments, while not providing funds to others).

prevent withdrawal symptoms caused by discontinuing the use of other opioids, for the purpose of detoxification or as a long-term substitution for opioid addicts. Its qualities are similar to heroin, just milder (pain relief, euphoria, sense of calm, lower stress). Many users complain that they do not feel a high comparable to the one delivered by heroin; they describe heroin as a substance that provides them with the needed high, a feeling of complete relaxation that Subutex is unable to provide. Among long-term users, intoxication is practically not observable at all. Subutex has long-lasting effects, and hence, it can be taken just once a day. The popularity of Subutex has increased in situations when the supply of heroin is very unstable, its price is high, and its quality is less predictable. The logic is: better Subutex than nothing. Sarah and many others argued that these were the main reasons to exchange heroin for Subutex. In 2001, when Subutex entered the Czech market, it was prescribed by regular doctors or by specialists in substitution centers. These prescriptions then had to be picked up at a pharmacy. Soon, news surfaced about the first Subutex available on the black market, as well as reports of its intravenous use.

Subutex is not fully covered by health insurance and is still difficult for many opioid addicts to afford (about 50 Euro/prescription for 1 week⁶), especially for long-term users without regular income, formal education, or work experience, or who are homeless. Some opportunists, who “help” those who do not have enough money purchase their pills, lie in wait in front of pharmacies: the price for such help is usually five tablets out of seven. The person who was prescribed the medicine, but does not have the money to pick up the prescription, is thus left with only two tablets.

Within a few years, Subutex gained a bad reputation among public and health-care professionals working with the general population (not only with drug addicts), and became known as the “legal heroin” traded on the ODS, usually injected and used by junkies. Therefore, regular physicians stopped prescribing it in order to avoid a problematic clientele. Today, Subutex is available in specialized centers for addicts, such as Gestalt. In these institutions, it is usually issued only under specific conditions. Many clients have problems with the rules of the high-threshold treatment that prevail in Prague. The SMT at Gestalt contains a pharmaceutical and a psychosocial component, namely, counseling sessions with a psychologist, urine tests, and meetings with other healthcare specialists (hepatologists, etc.). If clients regularly breach the rules of the treatment, they can be expelled. It is not uncommon that some clients move from one treatment center to another, until they eventually end up at the ODS.

Sarah also had many difficulties remaining on the program at Gestalt. She often missed her consultations or was evidently under the influence of some other drugs. From time to time, she had no stable or safe place to sleep, and coming on time to the scheduled consultation was an unachievable task for her. It was also obvious that her limited income could not cover a sufficient dosage of Subutex. When she did not have enough money for Subutex, she usually increased her dosage of benzodiazepine or reached for alcohol.

⁶One prescription contains seven tablets. This was the most common official dosage in Gestalt, 8 mg of buprenorphine/day. Therefore, clients usually came once a week for their prescription and used the pills on a daily basis.

Mutual Expectations at the Counseling Office

As a therapist of the SMT treatment, I often found myself in a difficult position, stuck between a concern for the well-being of the client, and the need to both respect the client's free will and the stringent conditions of treatment. The rules are meant to help prevent the distribution of the substance on the ODS and to expel clients who take advantage of the treatment, using it only as a way to acquire Subutex for its further paid distribution (although this is very hard to determine, since many users participate in the ODS in various ways). In a sense, the rules should also serve as motivation and contribute to the social, financial, and physical stabilization of the client. The tension between enforcing the rules and respecting the individual needs of a client brings about emotionally and ethically difficult situations, in which the therapist, often supported by discussions with the whole team, must choose how strictly they wish to enforce a given rule with a given client—a certain amount of flexibility with the rules, especially in Gestalt, was expected. This negotiation of rules also affected my relationship with Sarah.

It was not difficult for me to establish a relationship with Sarah. She was very pleasant and communicative. She readily listened to what I had to say to her, and she made plans for the following days and communicated the dreams and desires of what she wanted to achieve in the future. Her main motivation for making changes in her life was re-establishing contacts with her children and parents. I had sympathy for her, and I think she knew it. My office served as a place where she did not have to conceal her weaknesses or her sadness. During our cooperation, she renewed contact with her parents, and for many weeks, she was on an upswing. She tried to stabilize her dosage, did not use other addictive drugs, visited doctors to treat various infections, and took action to find better accommodation. She was happy. I felt happy with her. But these ups were soon followed by downs. Sarah got robbed, she did not have enough money to purchase her medication (and was therefore using other drugs), she was kicked out from a squat, and eventually she lost all of her documents when she got drunk. During these times, she did not show up at our appointments at all, and when she did, she looked troubled. She repetitively apologized and she cried a lot during these weeks.

Often when Sarah did not come, I was not only worried about her, but I was also concerned about her future in the treatment. I had to issue her warnings that informed her that she had breached the rules and was at risk of being expelled from the treatment. These episodes continued to occur, and I felt that it was increasingly difficult for me to deal with them and to accept them. Sarah probably felt the same way, since she commented on this difficulty several times. Once, when I was waiting for her and she, as she had many times before, did not appear, I realized that I was not only worried, but I felt that I was beginning to get angry with her and to lose patience.

Sarah evoked many emotions not only in me but also in the rest of the Gestalt team. The team was small, and everybody knew each other's clients. The team members shared their opinions weekly at meetings, and when a client's situation was becoming too complicated or confusing, time for supervision was arranged.

This gave us the opportunity to discuss the client with colleagues outside of our institution.

Supervision is intended to protect the clients and help the counsellors reflect their own feelings, thoughts, behavior, and general approach to the client. These opportunities to reflect more deeply on how one relates to a client, as well as to receive insight from the perspective of another therapist, should help the therapist increase the value they provide to their clients. Supervision often reveals unspoken intentions or uncovers sources of emotions that may be the cause of feelings unconsciously harbored toward the client. If such feelings are not reflected upon, they can negatively affect the cooperation.

Supervisions helped me take the imaginary step outside of the therapist–patient relationship, to calm my emotions, and reflect on my feelings from a professionalized distance. Through this process, I realized that my worry about Sarah was provoked by a tendency to protect her. I had to remind myself that Sarah had her own intentions that did not have to overlap with my wishes and expectations *for* her. My unfulfilled desire to help her caused frustration every time she “failed” in my eyes. Due to these unfulfilled wishes, I also had the tendency to be angry with her, which was surely not the emotion that I consciously wanted to feel toward her. During my supervision sessions, we also toyed with the hypothesis that Sarah probably understood my ambitions and wishes and did not want to disappoint me, and so could be trying to fulfill them. Until I addressed these affective dynamics in our sessions, the situation could allow Sara to feel guilt or discomfort, which would be counterproductive to our cooperation.

Supervision always brought me relief. It explored the themes and emotions that were already present in the relationship, but were hidden from me. These could often unconsciously influence the relationship and turn it into unwelcome or unintended directions. I gained the courage to reflect on our relationship more openly, and my unspoken ideas were adjusted in later meetings with her. I utilized much of what I discovered during my supervision sessions in my consultations with Sarah, which advanced our cooperation—a fact that even she positively evaluated many times.

Although Sarah had difficulties adhering to some of the rules of the treatment, we never expelled her from it, since there was no other institution that could provide her the medicine she needed, and we believed that our work was still beneficial for her. After 3 years of cooperation and our mutual efforts, Sarah died a sudden death from multiple organ dysfunction syndrome (total organ failure).

From the Therapy Room to the Open Drug Scene

During my last year in Gestalt and more than 1 year after Sarah’s death, I decided to look at Subutex and its users from an ethnographic perspective. One of the many reasons for this decision was my increasing interest in the position in which drug addicts in Prague find themselves in, located somewhere between the healthcare

system and the ODS. Another reason was the ever-present question of whether people undergoing Subutex treatments still gained more than they lost.

Moving to another site of research was not logistically complicated. I merely needed to quit my job, and to take the metro just a few stations further. Then, just as many of the clients and Subutex products did, I ventured out beyond the confines of institutions, onto the ODS, to the places where Subutex is distributed and is used alongside methamphetamine or heroin.

I already knew about these locations through stories I had heard from my clients, from colleagues at other institutions, and from newspaper articles. On the ODS, there are usually long-term users who inject the drugs and have to deal with complex webs of social, health, and financial problems and challenges. The Prague drug scene is not a ghettoized jungle on the outskirts of town; rather, it encompasses locations primarily in the center of town, where users conglomerate to a greater degree. They do not stay for long periods of time, but flow through numerous sites in great numbers throughout the day. Addictive substances are used more openly, and they are also sold and purchased there. This is all accompanied by the needle exchange service and its social workers.

From Remorse to Anger: The Anthropologist's Emotions

Although I had no one in this field with whom I could regularly discuss my emotions, I tried to continue the practice of doing so while researching on the ODS, just as I did in the previous years as a therapist, when I had the advantage of undergoing supervision if needed. From the onset of fieldwork, I felt a great deal of freedom, probably resulting from the fact that I was free from the active and direct intervention and from the rules of the medical system, which often created ethical dilemmas.⁷ But I was also slightly shocked by the roughness of this place, by the concentration of physically afflicted people, or by the high number of drug addicted persons in one place. It is one thing to see a client in the waiting room, and quite another to see, all at once, dozens of users marred by 30 years of drug use, exchanging needles near the dirty bushes of an unattractive nook in the public spaces of Prague.

In the beginning, I felt a certain distance from the people that were flitting about, shouting, and communicating in ways I was not accustomed to. Neither did I know their intimate life stories, or what lay behind their suffering. This initial distance helped me to remove myself from the emotionally challenging stories of the Subutex users, from their personal dispositions and indispositions. Social relationships and structural variables (such as a lack of Subutex and its high price) had come to the fore. I was observing the sphere of their day-to-day lives, their behavior, and its

⁷I wondered whether those patients expelled from the SMT treatment, and who thus find themselves on the ODS once again, felt the same way.

structural causes. There was no time to discuss the nuances of subjective experiences or to plan, paper and pencil in hand, the lowering of their Subutex dosage, milligram by milligram. This was a world that was rough and hectic, yet which, just as in any other setting, still offered room for humor, love, and gossip.

Witnessing people desperately looking for some Subutex, seeing others stalking a tough guy carrying a box of Subutex, or people rushing to inject the drug in the dirtiness and stench of the place, all made an impression on me. An image of a 22-year-old girl named Helena stood out. I met Helena during the summer at one of the most frequented places on the ODS. I was introduced to her by one of the needle exchange workers. She was talkative and open. She used Subutex along with methamphetamine, which is one of the most common combinations. During our first meeting, she described how she had been raped some days earlier during her long quest to find some Subutex: “On the last day, I still could not find anybody who would sell me some Subutex. Later at night, I met some boys that told me they have some Subutex. I was already desperate and the withdrawal symptoms were coming on, so I followed them to the car. There, it happened...” Then, she rolled up her pants and showed me her bloody legs, a sight from which I had to divert my eyes.

What I saw had left a strong emotional impact on me. Since that time, and more often than I would expect, Sarah came to mind. Not only was I recalling her repeatedly, I was also thinking about the inevitability of her death. This time, however, not so much by questioning my possible failure as a therapist, but from the perspective of the circumstances under which substance users search for their medicine. This was the main issue that I encountered on the ODS—subjecting all of one’s time to chasing a medicine that there is constantly too little of, and whose price is extremely high for most users. Suddenly, I stopped asking myself “why did Sarah miss so many of her consultations?” Rather, I wondered, “how is it possible that Sarah was able under such conditions to come to our appointments so many times, and usually on time at that?”

What was new for me on site in the ODS was the intense and relentless feelings of frustration and injustice. This sense of injustice emerged from the (un)availability of treatment and medication. Not everybody on the ODS wanted to be a client of an institution such as Gestalt, or to get the medicine from a medical institution, but there were many Subutex users for whom this possibility was excluded because they were not able to manage it due to structural and institutional obstructions, even if they wanted to. They included especially those with mental-health problems, those who were not able to stop using other addictive substances, or those without valid documents (an ID or an insurance card). Reflecting on my own experiences of anger and injustice led me to critically question a healthcare system that excludes precisely those persons who are supposed to benefit from it. I often contemplated the fact that almost no one on the ODS would pass the initial screening for most of the substitution treatments in Prague, coercing them to wander through the whole city for hours on end or to steal in order to find and pay for their medicine.

Medical Addiction and Addiction on the Streets

Sarah's and many other opioid addicts' problem was not the Subutex itself, but its unavailability—the unavailability for those who are not able to fit into the narrow category of “eligible” patients, or for those who are not able to follow the stringent rules of a substitution treatment. The official healthcare concept (World Health Organization 2016) of medical addiction is based on a logic that is far from able to provide appropriate interventions for the reality that I encountered on the so-called open drug scene. The official definition of addiction tells us that an addicted individual gives a higher priority to drug use than to other activities and obligations (*ibid.*). However, I witnessed something else. Subutex users did not put themselves at high health risks, in the sense of the official concept, as the result of substance abuse. On the contrary, their risky behavior was often caused by the unavailability of the substance, by a lack of finances for its procurement, or by circumstances that prevented regular intake and risk-free application (using a clean needle or dissolving it under the tongue). These circumstances exposed them to higher risk and deepened their health deficiencies. The lack of the medicine caused a situation in which users had no time to do anything but to search for it, left no time for them to take care of other significant issues in their life. Let me put this into comparative perspective: who would let a diabetic take their insulin using a needle found on the ground in the bushes near the main train station? How many diabetics would accept being sent to compulsory consultations with a psychologist? And mainly—what would diabetics undertake to get their dose of acutely needed medicine? Perhaps, in such extreme situations, their behavior could be similar to the behavior of someone on Subutex.

“Addiction” is a concept that provides a medical explanation for repeated and long-term abuse that has a detrimental effect on the health of a person. The illness caused by addiction has cognitive, moral, and institutional aspects (Conrad and Schneider 1992). Historically, branding addicts as ill changes their moral status as social pariahs. Instead of stigmatizing their behavior, their illness should legitimize their access to medicine, whether or not the medicine carries a bad reputation. Yet, those who are in need of Subutex must play by different rules than most other patients, even within the healthcare system, where viewing a person through the prism of illness should guarantee their access to medicine according to humanistic ethical codes.

Conclusion

Did Sarah have to die? I tried to explore this question by reflecting on my unresolved relationship with her, full of emotionally challenging moments and situations. As a psychotherapist, examining my relationship with her was part of my professional curriculum. By reflecting on my feelings with my working team, I

could better understand why I felt what I felt toward her, and what was going on in our relationship in the therapeutic setting. As an effect of this reflection, I tried to ease off from my own demands and ambitions and to better respect Sarah's abilities and limits. I tried to manage my tendency to feel frustrated, and thus also angry, at her when she did not fulfill my unacknowledged wishes.

This reflection among the working team cultivated a sensitivity to my feelings that brought many new aspects to our cooperation and opened up many topics that I would have otherwise overlooked or that I was afraid to confront. Regular supervision sessions were also a form of support for my work as a psychotherapist. The purpose of supervision and reflection among team members was to be aware of our own projections and to thus create a more productive relationship between the therapist and the client. However, despite this effort to improve my understanding, a fundamental part of Sarah's everyday life, as well as of the everyday lives of other Subutex users outside of the walls of institutions, still escaped me. This was one of the many reasons why I ventured onto the ODS as an anthropologist to find answers to some of my unanswered questions, still greatly affected by Sarah's death after 3 years of mutual work.

Being familiar with this kind of self-reflection proved to be beneficial for my ethnographic fieldwork. As an anthropologist, free from having to maintain my focus on the dynamics of our relationship, on the inner dynamics of a client, and within the confines of the therapy room, I could witness opioid addiction from a different perspective. Yet, it was the examination of emotions that helped me understand, as an anthropologist, what was going on on the ODS. Reflecting upon my anger and feelings of injustice led me to critically evaluate the care provided to Subutex users, and also changed the perspective from which I viewed my relationship with Sarah. Since experiencing the ODS, I no longer asked myself why she had "failed" so many times; on the contrary, I asked myself how it was even possible that she could cooperate so well for a period of 3 years when considering the extremely difficult conditions of life on the ODS.

Throughout my entire period of fieldwork on the ODS, I had to deal with the strong feelings that not only accompanied my data but which also had an impact on any attempt to get closer to the field. The events that I could observe there again revived my memories of Sarah, and after reflecting on these feelings, I now saw her story from a broader angle. There were many other Sarahs that revealed their fates to me. I experienced the anger that was caused by a sense of injustice observable on the ODS, an injustice deriving from the unavailability of medicine for these sick people. I witnessed the vulnerability of those who are on Subutex, but do not have direct access to this medicine. I witnessed their despair and the real price of Subutex in the form of amputated limbs, violence, or prostitution. I was most intensely frustrated at the moment I realized that, from a medical perspective, the persons I had encountered should in fact have free access to their medicine.

The work of a therapist is generally considered to be emotionally taxing. This is one of the reasons why the experiences of a therapist are subject to greater attention. The greater discussion of such issues forces us to deal to a greater degree with how we feel after work or what we experience while working. A therapist's education

forces them to reflect why they pose the questions they are posing and allows them to venture into the difficult places and relationships that other people try to avoid. Similar discussions might also be beneficial for anthropologists, whose work is often just as emotionally taxing as a psychotherapist's.

The disciplines of anthropology and psychotherapy have something to offer to one another in theory and in practice. I argue that not only can anthropologists learn from psychotherapists when preparing for fieldwork, but also in acquiring a sensitivity toward their own emotions and motives. Without the assistance and support of someone outside of the situation, such reflection might be much more difficult, because fieldwork and ethnography are characterized by the oscillation between immersion into the field and a critical distance to "the data" at the same time.

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Part V
Failing and Attuning in the Field

Failing and Attuning in the Field: Introduction



Dominik Mattes and Samia Dinkelaker

We can only fail if we strive to *do*. (Takaragawa and Howe 2017)

Perhaps, more than any other method in social science, conducting fieldwork in social anthropology is prone to failure. Throughout the entire process of producing anthropological “knowledge,” it seems, failure lurks just around the corner. Ethical clearance may be hard to obtain. The research topic and locality may reveal themselves to be unproductive. Establishing access, trust, and rapport may pose unforeseen challenges (Coffey 1999). Carefully designed research methods may be difficult to implement and might not yield the desired results (O’Brien 2010). The physical, mental, and emotional demands placed on the ethnographer may be unsettling, if not overwhelming (Stodulka et al. 2018). Ethical questions of equality, fairness, and reciprocity may be left without satisfactory answers. The amount of data acquired may not suffice or may surpass any manageable measure. Interpreting and lending relevance to ethnographic data may turn out to be a tedious task unrewarded by success. The vividness of the ethnographic experience may be lost when writing up (see Hovland 2007; Papageorgiou 2007). And the impact of the study, once all these hurdles have been overcome, may be marginal or nonexistent.

It is reasonable to assume that any anthropologist working in the field, and not just the novices among them, will at one point or another face failure in at least one sense of the term: that is, as an “omission of occurrence of performance,” a “failing to perform a duty or expected action,” a “state of inability to perform a normal function,” an “abrupt cessation of normal functioning,” a “fracturing or giving way

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under stress,” and, in very general terms, as a “lack of success” (www.merriam-webster.com). Which ethnographer, one wonders, would not be able to offer a story of failure, an incident or phase of unmet expectations, a narrative of how people, things, relations, and her or himself “misbehaved,” broke down, or went in a wrong direction? Experiencing such disruptions, slippages, and deficiencies may provoke disappointment, anxiety, shame, depression, guilt, frustration, and a wide range of other profound emotional reactions in the researcher (see Pollard 2009). Notwithstanding the fact that such emotions are often glossed over in scientific publications, we argue that they are highly relevant to the anthropological endeavor, inasmuch as they significantly shape how anthropologists subsequently engage with people, objects, and places in the field, how they interpret their findings, and which style of written (and other forms of) representation they opt for. In other words, attending to and transparently reflecting on failures during (and after) fieldwork, along with their concomitant emotional disturbances, is of vital epistemological significance. In this sense, we agree with what Gillian Goslinga and Gelya Frank propose in their foreword to the edited volume *The Shadow Side of Fieldwork* (McLean and Leibing 2007):

The willingness of [anthropologists] to describe moments of perceived failure or dissonance, or of intense identification and gut reaction, or of uncomfortable feedback or silence from their subjects, enables them to engage with something beyond the representational veneer of the ‘successful’ ethnography”. (Goslinga and Frank 2007, p. xvii)

In fact, we would argue that it is precisely these additional efforts of self-reflection that determine what we would conceive of as a successful ethnography. For failure does not exhaust itself in rupture, collapse, and defeat. Rather, it designates the awkward moment preceding a new start, before one reorients oneself, reappraises a situation, and recalibrates one’s sensibilities. In other words, “failure is inevitably productive” (Carroll et al. 2017; see also Okely 2009). In this sense, anthropologists’ emotionally charged experiences of failure always have to be thought of together with the researcher’s subsequent retuning of their self-understanding, adapting their appraisals of other people’s behaviors, their engagement in social relations and with material-spatial environments, their moral positions, and their methodological approaches.

Sincerely occupying oneself with and providing analytical room for one’s own perceived failures in fieldwork—both in situ and retrospect—may be a challenging, even painful exercise (see chapter “[Attuning Engagement: Methodological and Affective Dimensions of a Failed Collaborative Research Project in Timor-Leste](#)”, this volume). Yet, as the papers assembled in this section illustrate, it is highly rewarding in that it opens up new avenues, both for researchers and their readers, to a more nuanced understanding of the analytical—and affective—trajectory “of transforming fieldwork—with all its complexity and indeterminacy—into a text” (Delamont et al. 2000, p. 94), a specific fragment of scientific knowledge. It is our hope that candid reflections of this sort will help students and young scholars in anthropology gain a sense of how their aspirations concerning fieldwork may be thwarted, but also of how instances of struggle can be overcome and turned into

moments of additional insight. Ultimately, we wish that this will enhance their (self-)confidence prior to and during the experience¹ of ethnographic research, notwithstanding the lacunae that any fieldwork preparation will necessarily entail. For “mistakes are made, whatever the prior experience of the anthropologist, precisely because the relevant and detailed contexts cannot be predicted, because they are part of the emergent discoveries” (Okely 2012, p. 7). More than that, if any anthropological fieldwork went strictly to plan, it would actually have to be considered a failure.

The fieldwork experiences portrayed in the three contributions of this section cover accounts of a collaborative research project with East Timorese university students and young graduates on the topic of citizenship, of a study on the identity constructions of street-associated youth in Indonesia, and of the process of studying and learning the Indonesian martial arts *Pencak Silat* in Central and West Java, Indonesia.² Moments of failure, when “things behave *unexpectedly*” (Carroll et al. 2017, p. 15), caught the authors off guard. Sara ten Brinke found that the relationships with her research partners in East Timor were not as nonhierarchical as she had conceptualized in her collaborative research design. Paul Kellner was not prepared for the fact that the challenges of his fieldwork would not lie in his engagement with the street-associated youth who participated in his research, but in the anxieties that he experienced in the neighborhood where he and his wife resided during his fieldwork. Patrick Keilbart had not anticipated the ethical dilemmas surrounding the frictions that arose from his approach of studying *Pencak Silat* as an apprentice in different, competing schools, whose varying styles he endeavored to compare.

The authors describe feelings of disappointment, disillusionment, sadness, bewilderment, anxiety, self-consciousness, worry, reticence, nervousness, discomfort, frustration, guilt, and shame. This spectrum of “negative feelings” could be read as the “moral underpinning that makes failure such a horrible affront to the ego” (ibid., p. 14) and as the emotional undertone to the inability to perform. Finding themselves in “interstices of breakage” (ibid., p. 2) between what they had expected and their present realities, the authors experienced impasses, discouragement, and inhibition. Ten Brinke reflects on the collapse of the research group she was initially committed to conducting her collaborative research with. Kellner describes how the sense of discomfort in his neighborhood affected his fieldwork and impeded his capacity to engage attentively with research participants. Keilbart gives an account

¹We deliberately do not use the term “conduct” here because, as Judith Okely aptly notes, “it implies that fieldwork is managed and pre-directed” (Okely 2012, p. 5), and thus implicitly precludes the idea of methodological, personal, and cognitive openness, spontaneity, and flexibility or, put more abstractly, a general susceptibility to the unexpected that is central to the anthropological enterprise.

²Given the above-mentioned inevitability of failure in anthropological fieldwork, the articles collected in this section are not the only ones to depict moments of (experienced) failure (see chapter “[Conflicted Emotions: Learning About Uchawi](#)”, this volume). However, it is in this section where we explicitly invite the readers to look at fieldwork experience from the viewpoint of the productivity of failure in fieldwork.

of the feelings of failure that accompanied the realization that he could not fulfill his mission as an international “ambassador” of *Pencak Silat*. The exclusiveness of *Pencak Silat* apprenticeship seemed to make it impossible to publicly represent his *Pencak Silat* schools as a foreigner and thus “give something back” to his teachers. Moreover, he risked losing his teachers’ trust. Ten Brinke made the emotional effort of accepting that her fieldwork involved power imbalances, notwithstanding her efforts to dismantle and overcome them. Kellner sought support from friends and a mentor in trying to regain a sense of (self-)security. Keilbart made an intellectual effort and reexamined his own understanding of reciprocity in *Pencak Silat* apprenticeship.

The authors provide valuable reflections regarding methodological questions, research ethics, and coping in the field. Ten Brinke’s contribution thoroughly explores the problem of how to approach engaged and collaborative research projects in a way that is more sensitive to hierarchies in the field. Keilbart highlights the fact that the embodied knowledge he acquired as a *Pencak Silat* apprentice proved an effective method in navigating the challenges of his fieldwork. And Kellner addresses the importance of self-care through supervision and resorting to spaces that give feelings of security. While this should not replace other forms of support such as mentorship by postfieldwork PhD students (see Pollard 2009), Kellner’s proposition might be a helpful resource for researchers in managing tensions, stress, or self-doubt. Moreover, Keilbart’s reflections on how he negotiated the expectations that he encountered as a martial arts apprentice might stimulate other ethnographers who face ethical questions related to reciprocity and responsibility (see the section “[Reciprocity in Research Relationships](#),” this volume).

In addition to the importance of reflecting upon the process of fieldwork, we particularly wish to highlight the productivity of failure with respect to deepening the authors’ understanding of their research topics. Addressing their emotional experiences has illuminated the authors’ engagements with their respective subjects of research. By contemplating her frustrations and the dissolution of her collaborative research group, ten Brinke gained a more profound understanding of what “participation” means to young Timorese—not only in the context of participating in collaborative research projects but also in the sense of political participation. We agree with Kellner when he suggests that the rich description of his own anxiety and his efforts to regain a sense of comfort and security in the spatial environment could be a fruitful point of departure for further inquiry into space-related strategies of comfort-making that are practiced by the street-associated youth who participated in his fieldwork. In outlining how he addressed the quandaries he faced upon realizing that he could not fulfill his teachers’ expectations, Keilbart underlines his epistemological process of gaining deeper insights into the shared values of different *Pencak Silat* schools.

Each of these contributions demonstrates the productivity of (reflecting on) failures in fieldwork and acts as an invitation to make use of “failure” as a heuristic tool. When approaching their oftentimes messy research material, ethnographers might not only profit from the question, “What did I find out?” but also from the questions, “When did I fail, how did I cope, and what did this do to my research?”

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How to Be a Good Disciple (to a Martial Arts Master): Critical Reflections on Participation and Apprenticeship in Indonesian *Pencak Silat* Schools



Patrick Keilbart

Introduction: From “Karate Kid” to “Oriental Monk”

In 2010, the Chinese-American martial arts film *The Karate Kid* (Columbia Pictures) was released (Weintraub, 2010). The plot of the movie closely follows the story written by Robert M. Kamen for the original 1984 film of the same name. In both films, a newly arrived boy gets into trouble in his neighborhood, but he manages to solve his problems with the help of an old and wise master who teaches him martial arts. Yet, unlike the original, the remake is set in China and features Kung Fu instead of Karate. Despite its misleading title, the remake won a number of awards and topped the US box office, grossing \$359 million worldwide.¹

Images and discourses circulating in the global media influence the general perception of martial arts in their countries of origin and abroad. Yet, such romantic or symbolic representations mostly fail to correspond to the profound implications of martial arts education, both for the student or apprentice and for the master. Retracing the construction of martial arts as means of self-actualization and self-improvement, Berg and Prohl (2014) analyze “Shaolin Kung Fu” in relation to the Shaolin Temple in Germany. According to Berg and Prohl, the Shaolin Monk comes to stand for the Asian martial artist par excellence—the “Oriental Monk” (Iwamura, 2011)—a certain stock figure of popular culture. Different cultural influences converge in the discursive formation of the Oriental Monk, whose general features—extraordinary physical abilities, spiritual commitment, calm demeanor, Asian face, and manner of dress—have been absorbed by popular consciousness through mediated

¹<http://www.boxofficemojo.com> (accessed September 9, 2016).

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culture. The practitioners of Shaolin Kung Fu in Germany follow the living example of the Shaolin/Oriental Monk and apply the “technologies of the self” provided in martial arts training, accepting its inherent promise of self-actualization or self-improvement: “The martial arts myth or promise is precisely this: through nothing more than physical discipline, dedication, devotion, and diligent training, you too can [come] closer to the invincible ideal depicted in these films and programs [i.e., martial arts movies and series]” (Bowman, 2012, p. 5, as cited in Berg & Prohl, 2014, p. 6).

Reflecting on my personal and professional interest in martial arts, much of it stems from my own affection with the figure of the Oriental Monk and the “martial arts promise.” As a schoolboy, movie and television series of the 1970s and 1980s, such as *Kung Fu* (Thorpe, 1972–1975) or (Weintraub, 1984, 1986, 1989, 1994) fascinated me. I could identify myself with the main character, the “Karate Kid” Daniel, who moves to California and gets bullied by other students at his new school there. When the Japanese house caretaker, Mr. Miyagi, initiates him into self-defense techniques and the philosophical foundations of Karate, Daniel can hold his ground against his bullies. Despite similar experiences during my early school days, I never joined the local Karate club in my hometown. (Instead, I joined the local soccer team, tried to integrate, and adapt myself.) Only on the occasion of carnival, when boys usually dressed as cowboys or knights, did I act out my fantasy as the Karate Kid.

I started to practice martial arts about a decade later, around the same time that I began my studies at university. What drew me then to martial arts was not practical self-defense, but their athletic, aesthetic, and spiritual aspects—the martial arts promise of self-improvement. Again, I did not join a local martial arts school, of which there would have been a great variety. Instead, I learned Kung Fu from a master who had come to Germany from Bagdad, as a political refugee who had been persecuted by the regime of Saddam Hussein’s Baath Party. So for me, the Oriental Monk manifested as a somehow both Middle Eastern and Far Eastern character at the same time. Being his only student at that time, my apprenticeship was a very personal and intense experience. Besides physical and organizational discipline, cultural antagonisms or contradictions between my sociocultural background and the values and ideals (of honor and masculinity) conveyed through martial arts training were most challenging. Yet, the masterly self-control of my teacher, that is, his body control and control over himself, in my view provided a perfect role model and example of an Oriental Monk. He implicitly and explicitly extended to me the martial arts promise of achieving that same level of self-control, under the premise of carefully following his instructions and training diligently.

At university, I majored in cultural studies with a focus on Southeast Asia, particularly Indonesia. It turned out that the Indonesian martial art *Pencak Silat* represented a rewarding field of research. In Indonesia, *Pencak Silat* schools represent institutions of social knowledge communication for a large part of the population. Reasonable estimates run into the tens of millions of practitioners in Indonesia. The importance of *Pencak Silat* schools is reflected clearly in the figure of the *guru*, the master of a school (*perguruan*). School leaders are “usually national figures in

ritual, religious, political, and economic domains, and they are often practitioners themselves” (De Grave, 2014, p. 48).

Ultimately, my personal and professional, anthropological interests in martial arts were mutually beneficial. Learning about Indonesian martial arts and culture helped me reflect on my personal, intense experiences with a master personifying the Oriental Monk. Linking personal interest in martial arts with academic interest, I was able to write my *Diploma* dissertation on *Pencak Silat* and religious nonconformity in Java. I am currently writing a doctoral dissertation on *Pencak Silat*, mediatization and media practices in Indonesia.² The dissertation is based on a total of 24 months of ethnographic fieldwork, conducted over a period of 10 years (between 2006 and 2016).³ This chapter provides critical reflections on my fieldwork, on observant participation and apprenticeship in Indonesian *Pencak Silat* schools. Especially, the relationship between *guru* and student, the obligations and challenges of being a researcher and apprentice at the same time, are illustrated. The focus of this chapter is on embodied participation and apprenticeship as research methods, a perspective that is necessary to understand processes of mediatization in martial arts and body cultures in general. A notion of martial arts as embodied knowledge (Farrer & Whalenbridge, 2011) sets embodiment as the “existential condition in which the body is the subjective source or intersubjective ground of experience” (Csordas, 1999, p. 143). Content and capacity of the body as a medium are developed and changed in *Pencak Silat*, especially in connection with the use of new media technologies. The construction of *Pencak Silat*, the *guru* as Oriental Monk, and the researcher as apprentice can be retraced both in immediate performance and in media representations. In this study, I illustrate the specific concept of embodied knowledge in *Pencak Silat*. It is based on situated, relational, and transformative learning, and results in particular bodily and mental states. The study also takes into account the related knowledge management in *Pencak Silat*, which is characterized by exclusivity, competitive thinking, and a sense of mission. Based on this, the obligations, emotional predicaments, and reciprocity issues that arose from embodied, spiritual, and cultural apprenticeship in various *Pencak Silat* schools are described. As will be shown, these predicaments arose not only from bodily experience but also from a comparative research design (which was problematic due to the schools’ demands for exclusive membership). Despite the emotional challenges I faced due to the comparative analysis of different *Pencak Silat* schools, this approach revealed shared values in martial arts education and apprenticeship.

² *Martial Arts, Mediation, and Mediatization. Pencak Silat and (dis-)embodied Media Practices in Indonesia* (University of Cologne; Working title).

³ In 2006, I joined a guided excursion to Eastern Indonesia and made contacts with *Pencak Silat* practitioners there (and also in Jakarta). In 2008, I was accepted to the scholarship program ‘Darmasiswa’ of the Indonesian government. Eight months of studies in Bandung, West Java, between September 2008 and April 2009, resulted in my *Diploma* dissertation. In 2014, I received a 1-year scholarship from the German Academic Exchange Service (Deutscher Akademischer Austauschdienst, DAAD) for my PhD research in Yogyakarta. In 2016, another 5-week stay in Yogyakarta was funded by the department of South Asian and Southeast Asian Studies at the University of Cologne, allowing me to conclude my PhD research.

Approaching the “Oriental Monk” in Indonesia: Becoming a *Pencak Silat* Apprentice

My first encounter with *Pencak Silat* and “Inner Power” was a video I had been shown in the Jakarta headquarters of the prominent national *Pencak Silat* school *Merpati Putih* (indon., White Dove). It was September 2006, the Muslim fasting month of Ramadan, so I had no chance to join training or watch a live performance. Membership in *Merpati Putih* is independent of religious affiliation, but during Ramadan, physically strenuous performances are avoided to ensure the well-being of fasting members. For me, this was disappointing. I had been practicing martial arts in Germany for several years, and I was eager to experience the Indonesian fighting art, to get my first lesson in *Pencak Silat*. Instead, the management team of *Merpati Putih* (MP) presented me with their demo-DVD. It showed practitioners breaking concrete blocks and metal bars with their bare hands, driving a motorbike through an obstacle course or recognizing different colors of cloth with their eyes blindfolded. My disappointment quickly changed to excitement. The martial arts I had trained in before concentrated on self-defense and sport competition, not on spirituality and the Inner Power with which the MP management explained the extraordinary skills displayed in their demo-DVD. I also became excited from a scientific perspective, because the explanations given were based on metabolic processes of regeneration and clearly emphasized distance toward mystical beliefs; they implied a very rationalist approach to spirituality. I had the impression that my Indonesian hosts were somehow able to read my mind, because—despite all excitement—I felt that the rational and intellectual dimensions of my own “belief system” made me doubt the exceptional powers shown in the video. After watching the DVD, the founder and grandmaster of *Merpati Putih*, Poerwoto Hadi Poernomo (Mas Poeng⁴), talked to me about its content and asked me about my impressions, my intentions, and about my personal background. I addressed my doubts, but assured him of my enthusiasm and expressed serious interest in learning at MP. At the end of our conversation, Mas Poeng held out the prospect of training during my next visit to Indonesia, with the goal of qualifying me to open the first *Merpati Putih* branch in Germany. His offer took me by surprise, but I felt honored and returned home with a new personal as well as professional objective.

Two years later, in September 2008, I joined the Indonesian government’s “Darmasiswa” scholarship program and was sent to study in Bandung (West Java). At the partner university, the High Conservatory for Indonesian Arts (*Sekolah Tinggi Seni Indonesia*, or STSI), I joined mandatory *Pencak Silat* classes in the dance department and as extracurricular training. I also joined *Merpati Putih* at the local *Universitas Padjadjaran* (UNPAD), where I eventually started practicing the control

⁴ ‘Mas’ (sir, brother) is the Javanese form of address used for males without a higher status, but in *Merpati Putih* also teachers and masters are addressed with ‘Mas’ and the founding father is commonly known by ‘Mas Poeng.’ This emphasizes the equality of all members and the overcoming of old hierarchic structures (see also below).

of my Inner Power. The regular MP training sessions (two times a week) included a set of breathing techniques which aimed at influencing physiological processes within the practitioner's body. Together with hardening and conditioning certain body parts, these breathing techniques were employed for breaking hard objects (*patahan benda keras*). Reflecting on my first personal experience of expressing Inner Power, I wrote in my field diary:

Yesterday, after only one month of *latihan* (training), my *pelatih* (trainer) made me try to break a concrete block (60 × 20 × 10 cm) with a vertical blow of the edge of the hand. I performed the breathing techniques and tried to concentrate my Inner Power in my hand. Again, my rationality and skepticism made me doubt the success of this undertaking; I was afraid I'd rather break my hand than that my hand would break the concrete block. However, when the performance of the breathing techniques generated a warm, tickling sensation in my hand, I summoned up the courage to complete the task and actually broke the concrete block to pieces. Beyond that, my hand remained uninjured and after performing certain breathing techniques again, I didn't feel any pain at all. (Field note November 16, 2008).

Of course, my aim here is not to promote *Merpati Putih* or convince the reader of the effectiveness of Inner Power. One of the points I like to make is that, for me at least, seeing was not believing.⁵ Watching the extraordinary powers displayed in the MP demo-video made me doubt their authenticity, their realness. Experiencing the effectiveness of MP breathing and Inner Power myself did not completely eliminate all my doubts and make me a "believer" right away, but it made me question my doubts, trace my impressions and perceptions, and reflect on my comprehension. Closely related to this is another associated phenomenon, namely the intermediate position of Inner Power in *Pencak Silat*, hovering between rational, scientific explanation, and religious practice or belief. After a few months of *latihan* MP, one of the trainers, who had also become a friend of mine, gave me a private lesson and introduced me to *ilmu getaran* (knowledge of vibration) or "Vibra Vision":

After I had performed some of the breathing techniques, Danil blindfolded me with my MP belt and then asked me to try and feel different objects he placed on the ground in front of me. He explained to me that all objects, and living beings as well, emit certain electromagnetic vibration which humans could learn to sense, and ultimately see without using their eyes. My efforts to sense the objects in front of me were, unfortunately, not very successful. Danil asked me to carry out some of the breathing techniques again; when I had finished and was about to try again, he said, "This time, try to let go and give yourself to 'God'—according to your own understanding of 'God'." (Field note February 18, 2009).

Apart from the failure of my first attempt to use Vibra Vision, this instruction made it a religious experience for me. It urged me to look into myself, and to deal with the question of how I actually understand "God." This eventually reoriented my research project toward the relation between *Pencak Silat*, Inner Power, and religion. When I started my research in September 2008, one of my key informants, a *Pencak Silat* master and lecturer at the STSI in Bandung, stated: "*Pencak Silat*

⁵The idiom "Seeing is believing" can be explained as "[I]f you see something yourself, you will believe it to exist or be true, despite the fact that it is extremely unusual or unexpected" (<http://dictionary.cambridge.org>). A more detailed reflection on the difference between "seeing something in a video" (mediated), "seeing something yourself" (immediate), and actually experiencing or performing it yourself is provided in the conclusion.

bukan Agama”—*Pencak Silat* is not [like] religion. Six years later, in 2014, I met a *Pencak Silat* master and lecturer at the *Universitas Gadjah Mada* (UGM) in Yogyakarta, Central Java, and she told me: “*Pencak Silat* is like religion.”⁶ The question as to what extent each statement applies to different *Pencak Silat* schools is not relevant here. Instead, I will give another example of challenging personal experience as an apprentice in a more traditionalist, Muslim *perguruan* named *Sang Cipta Rasa*:

Today, I participated in a *debus* performance [a demonstration of invulnerability]. The master, GusMus,⁷ cut cucumbers and bamboo sticks with his razer-sharp *golok* (machete) to demonstrate its sharpness. Then he cut himself with the *golok*, at his arm, his throat, and his tongue, without being hurt. He also brought viewers from the audience on stage to demonstrate his *debus* skills. After splashing a little water at them and mumbling something (in Arabic?), he cut them with his *golok*, too, but they too were not hurt. At the end, GusMus asked me to come on stage. He smashed a glass bottle and, after splashing me with water and mumbling his prayer, he used broken bits of glass to cut me at my arm and my belly. I could feel the sharp edge of the glass splinter scratching my skin, and felt the pressure GusMus applied on it, but I was not wounded or hurt. I was not frightened, but rather felt surprised. Later, when I watched the video one of his students had taken with a mobile phone, I saw that all participants GusMus used for his demonstration—including myself—looked really uneasy or stressed. GusMus laughed at these reactions and told me there was no need to worry, since his knowledge (*ilmu*) and God’s strength protected us. (Field note March 01, 2015).

In the course of my fieldwork I conducted participant observation as an apprentice in different *Pencak Silat* schools, mainly in West and Central Java. The exemplary field notes and the two different *perguruan* presented above illustrate my engagement in embodied anthropological fieldwork, and indicate the expectations in me held by *Pencak Silat* masters and educators. The tests or demonstrations of strength in the two different schools set the ground for grasping the profound implications of *Pencak Silat* education, and the particular challenges of reciprocal exchange and structural power relations within *Pencak Silat*.

Power Relations, Expectations, and Impasses

Compared to other fields of scholarly inquiry, Martial Arts Studies has a distinctly democratic flavor. This stems from a number of sources. Most obviously, the martial arts are widely practiced in both the East and West in the current era. (Judkins, 2015, p. 1).

Judkins does not further substantiate his claim; it appears to stem from the accessibility to the field (martial arts as an “Eastern” cultural export), and the collaborative nature of research carried out by practitioners in different parts of the world.⁸

⁶Interviews with Pak Nanan, Bandung, October 24, 2008, and Ibu Wening, Yogyakarta, December 11, 2014.

⁷GusMus is the short form for Gusti Ali Mustofa, with Gusti being a Javanese title for a master.

⁸The collaborative nature of martial arts studies is presumed to lie in the common ambition of both (Eastern) masters and (Western) students to elaborate research and ensure a more intellectually rigorous, academic consideration of martial arts.

According to my experiences during fieldwork, as an anthropologist and as a martial arts practitioner, the “distinctly democratic flavor” of martial arts studies is often covered by strict hierarchical structures in a school and expectations that students demonstrate respect and unconditional obedience toward their masters.

Pencak Silat masters have sought to modernize, that is, liberalize, their schools and *Pencak Silat* in general, in particular since Indonesian independence. For centuries, martial arts in the Indonesian archipelago were characterized by secrecy, privilege, and the unquestioned authority of the master of a *perguruan*, who represented a spiritual preceptor and source of knowledge. In the context of nation-building in Indonesia and the inclusion of modernization theories into state ideology, the term “*mental-spiritual*” was introduced to replace former, established concepts such as *kebatinan* or *kanuragan* and common notions of martial arts mysticism and magic.⁹ The introduction of the *mental-spiritual* has affected the roles and understandings of a *guru*. In progressive schools like *Merpati Putih*, the master is no longer viewed as a powerful spiritual medium, but rather as a facilitator of self-development. Implementing the *mental-spiritual*, Inner Power in MP is “substantive and focused on the individual, cultivated through disciplined practice” (Wilson, 2011, p. 73). The modernizing processes of *Pencak Silat*, however, create a paradox: “modern” *Pencak Silat* is envisioned as rationalized and liberal-democratic, while secrecy and privilege, hierarchical structures, and ideas of unquestioned authority remain. The latter are romanticized and valued as adequate behavior toward and of a real master, which contradicts the idea of “modernization.”

This is also reflected in the relationship between a master and a foreign (Western) student and researcher. As an apprentice in MP, the leadership expected me to contribute to the modernization, that is, rationalization and scientific foundation of Inner Power, and to share the knowledge I gained in MP upon my return to Germany. When the founding father and headmaster Mas Poeng directly addressed that issue at our first meeting in 2006, I did not fully assess the implications of his offer. In early 2000, Mas Poeng appointed the two US-American brothers Mike and Nate Zeleznick to become *pelatih* MP; together they officially inaugurated *Merpati Putih* USA in Ogden, Utah. Since then, the Zeleznicks have spread and promoted MP in the USA, as a spiritual practice and New Age movement, and they have contributed significantly to the scientific foundation of *Merpati Putih* and Inner Power. In mid-2000, they invited a delegation of MP members to Utah, to test and validate Vibra Vision scientifically (Keilbart, 2017). As an anthropologist, I could not and cannot contribute to the scientific investigation of Inner Power and Vibra Vision as the MP leadership intends. In my ethnographic account of *Merpati Putih*, scientific explanations based on metabolic processes and wave theory are identified as part of MP’s

⁹The concepts of *kebatinan* and *kanuragan* are related to mystical practices and beliefs. They represent the basis for and an expression of a Javanese worldview (*kejawen*). At its core lies the aspiration to acquire cosmological and spiritual knowledge. The intrinsic link between martial arts, war magic, and warrior religion has been analyzed comparing various martial arts (Farrer, 2014). The role and influence of Javanese *kebatinan* and *kanuragan* on Indonesian ethics, (religious) beliefs, and ideologies, and its various social applications, have been studied intensively (De Grave, 2014; Mulder, 1998; Schulte Nordholt, 1991; Sihombing, 2011; Wilson, 2002).

modernization approach. This does not provide scientific proof for the existence or efficacy of Inner Power. In contrast to findings in the medical and natural sciences, the kind of knowledge a social scientist produces is not perceived as useful by the school leadership. The same applies for *Sang Cipta Rasa* and GusMus' *debus*, but for a different reason. The master of this rather traditionalist Muslim *perguruan* did not see any sense in a scientific examination of his invulnerability skills and practices at all. On the contrary, he repeatedly emphasized that *debus* could not be explained rationally or scientifically, that the secret knowledge can only be passed from master to student, who must experience it himself. Thus, my scientific knowledge and my proposal of mutual sharing of knowledge were not well received, neither by the modernist nor the traditionalist master. This was also due to the fact that my comparative analysis of different *Pencak Silat* schools runs contrary to the established principle of secrecy and the exclusivity requirement of membership in a school. The lecturer at UGM also referred to that exclusivity requirement when she equated *Pencak Silat* with religion. She said for her, as a member of a certain *perguruan*, it was impossible to change to another school or do comparative research in different schools. For her, affiliation with a certain *perguruan* was like religious affiliation—implying exclusivity.

For me, this statement helped explain the limited acceptance of my (scientific) contributions. It entailed a sense of failure, which came from the concern of having misjudged both the obligations of a *Pencak Silat* apprentice toward his master and school, and my opportunities for cooperation and research. I felt stupid for having overlooked something I thought I should have known, due to former studies and also to former experience with my master back home in Germany: competitive thinking among martial arts schools and styles, and claims of exclusivity, superiority, and nonappealability appear to remain overriding principles. So my comparative analysis of *Merpati Putih* and *Sang Cipta Rasa* was problematic in several respects. At some point, this made me doubt my comparative and embodied participation approach. My apparent misjudgment of possibilities, of my ability to contribute to the development and modernization of a school made me feel guilty for not fulfilling the expectations masters had of me. It made me feel frustrated for not being able to fulfill my own expectations. I had started my fieldwork with a concept of dialogical research in mind: I sought to work in a collaborative, comparative research endeavor, and to make (preliminary) findings available to all contributions. Although I disclosed my comparative approach ab initio, and the masters of both schools gave their approval, the democratic flavor of martial arts studies and the dialogical research I had planned could not really develop. Not only were my scientific findings of no use for my informants, my comparative approach ran contrary to the prevailing principle of exclusivity, and restrained the method of performance and participant observation. I could not attend public performances of a school to which also other schools potentially had access. So I could not fully meet the expectations of *Pencak Silat* masters placed in me, as a foreign (Western) apprentice, to attend public performances, to represent the school in Indonesia and abroad, and to demonstrate its success expanding to foreign countries.

This problem expanded with schools providing photo and video material via social media, and discourses evolving around such material. As Patrick Eisenlohr (2011, p. 45) notes, media representations can be used for “claiming a more immediate link to desired political or religious destinations or moral goods.” In *Pencak Silat*, this does not entail the revelation of any secret knowledge, but is rather deemed proof of efficacy and the master’s ability to exploit and impart this kind of knowledge. In *Merpati Putih*, it is used to demonstrate that in MP anybody, even a Westerner and anthropologist, can learn to control and harness their Inner Power for breaking hard objects or to use Vibra Vision to “see without using the eyes.” In *Sang Cipta Rasa*, it is used to demonstrate that GusMus, as a master, can transfer the strength of God (*Allah*) to anybody, even a non-Muslim Westerner, protecting him from bladed weapons or sharp objects. For me as a researcher and apprentice, who was both portrayed in social media and engaged in it, this further complicated the situation. It entailed the difficult task of very consciously engaging in social media activities while selecting networks or groups according to the (potential) access of members of different *Pencak Silat* schools. When users and members of a certain *perguruan* found material on social media which displayed me practicing or demonstrating routines of another *perguruan*, this clouded our relationship of trust. The negative effect on my research was that important information and knowledge would then not be shared openly with me anymore.

Since my scientific knowledge was not really valued, performing for a school (immediate and in media representations) seemed the only possibility for me to “give something back.” I felt it was what the masters and leadership of a school expected from me. In my research, the paradox of modernization, exclusivity, and hierarchical structures in *Pencak Silat* schools obscured the “distinctly democratic flavor” of martial arts studies. It restrained my performance and dialogical research approach and gave me a feeling of failure. When assessing these preliminary findings and developments during my research, the situation seemed to represent an impasse.

Nonetheless, I argue that apprenticeship and embodied participation in different *Pencak Silat* schools is an anthropologically fruitful approach to assess the shared common principles of Indonesian martial arts education. Furthermore, the following section illustrates that the comparative perspective and methodology does not prevent the researcher and/as apprentice from performing his main and essential obligations.

Apprenticeship, “Taking and Giving” in a *Perguruan*

Martial arts fieldwork may involve a higher degree of participation as compared to observation in regular anthropology. (...) Observation without participation may leave the fieldworker with scant appreciation for what is really going on. (Farrer, 2015a, p. 1).

What makes martial arts fieldwork a potentially more intense form of participation, compared to other fields, is its demanding requirements, both physically and

mentally. Similar to Wacquant (2004) “carnal sociology” of boxing, which is based on participant observation, the researcher joins in and learns a martial art from the ground up as a basis for research, understanding and writing. In my case, such apprenticeship and embodied participation included strenuous training sessions, physical confrontation and combat performances (unarmed and armed), physical and psychological conditioning, and challenging tests that pushed me to the limits. Being a researcher and apprentice at the same time poses questions of reciprocal exchange, structural power relations, research ethics, and epistemology. Using the methodology of “performance ethnography” (Farrer, 2015b; Zarrilli, 1998),¹⁰ learning *Pencak Silat* in Indonesia provided me with the demanding challenges of mysticism and embodied (mystical) experience. According to Michel de Certeau, “Mysticism arises against the backdrop of modern Western discourse, while the mystical (...) is an adjective that describes a secret and concealed aspect of experience” (Napolitano & Pratten, 2007, p. 5).

The mystical presents physical, intellectual, and emotional challenges for the researcher, who has to deal with (and work through) embodied mystical experiences that potentially question personal convictions and traditional anthropological ways of reasoning. Dealing with martial arts mysticism and transforming embodied mystical experience and knowledge into scientific knowledge—its adequate representation and introduction in “Western discourse”—is a difficult task. But the essential obligation of the researcher and apprentice, and the most important expectation of a *guru*, does not go as far as scientific representation. It is to open up toward and be receptive to embodied experience and knowledge, allowing it to influence personal convictions. First and foremost, an apprenticeship in a *Pencak Silat* school is a relationship of mutual trust. The researcher as apprentice has to trust the *guru*, accepting the instruction provided and a regimen of training that includes potentially dangerous methods or examinations. The *guru*, by accepting the researcher as an apprentice, trusts him to handle responsibly the knowledge imparted and skills learned, according to the master’s guidelines and expectations. Despite different ideological foundations and explanations for embodied experience and knowledge, the common shared values and ethics in *Pencak Silat* nonetheless became clear in the long term. Maryono (2002, p. 47) names them the principles of harmony and etiquette, and the Javanese cosmological concept of *manunggaling kawula Gusti* (the unity of humanity and God). Both in *Merpati Putih* and in *Sang Cipta Rasa* these common shared principles became comprehensible for me through personal experience as an apprentice. This is why performance ethnography and apprenticeship are viable methods of choice, also and especially for a comparative analysis of *Pencak Silat* education. As Coy (1989, p. xv) has summarized, apprenticeship as research method provides insights into “ways of knowing and ways of seeing things

¹⁰Performance ethnography is an arts-based method of qualitative inquiry, designed to bridge the gap between scholarly activity and education in performance arts. Following Zarrilli (1998), it represents an ethnographic research strategy that is implemented through the full participation of the researcher in the performance genre. The aim is an ethnography based on communication and dialogical conversation rather than observation.

that can only be experienced” and “an excellent way to learn about learning (Goody, 1989, as cited in Coy, 1989, p. 2).

Apprenticeship is often viewed as an education in the ‘secrets’ of a craft (...). These secrets might lie in any of the dimensions of training. Often they are secrets only inasmuch as they are not shared with those who have not engaged in apprenticeship. (Coy, 1989, p. 3).

My apprenticeship put me in a position to be introduced to at least some of the “secrets” of *Pencak Silat* that lie behind the extraordinary feats practitioners are able to perform. As indicated in the field notes above, extraordinary feats, explanatory approaches, and fundamental ideas differ considerably. Apprenticeship in essentially different *Pencak Silat* schools disclosed secret, implicit, specialized knowledge in terms of performance skills, but also concerning physical, mental, and social education. Approaching the Oriental Monk and the “martial arts promise” of self-improvement as an apprentice in different *Pencak Silat* schools provided the basis for an examination of martial arts education beyond an individual case study. Acquiring specific body knowledge and learning performance skills through participation, long-term observation, and experience revealed the implicit knowledge of how the senses are educated in *Pencak Silat*. The comparative approach revealed how the ideal master and his relationship with his¹¹ students are imagined and constructed, despite different ascriptions. Mystical insights can be perceived as contradictory to Western rationalized, technologized, materialistic culture, or as complementary to it. The *guru* can be seen as spiritual medium or as facilitator of self-empowerment. In any case, learning *Pencak Silat* means learning about Javanese (and Indonesian) culture, ethics, ways of perceiving the environment, respectful interaction with educators, elders, and other human beings in general. Being a researcher and apprentice, taking and giving, first and foremost entails trust in the methods and aims of the *guru*, and in the embodied experience, reception, and implementation of the imparted knowledge. For the student, testing one’s Inner Power by breaking hard objects and demonstrating the ability of a master to provide protection against sharp objects have similar effects. Both practices are initially disconcerting and frightening, presenting a physical and mental challenge for the apprentice. To face such a challenge, to overcome doubts or even fear, and to experience the relief of physical integrity has several synergetic effects: trust in the *guru* and his methods proves justified, substantiating his leading position; the apprentice develops self-confidence and a sense of belonging, experiencing a personal connection to a higher power (either immediate or mediated by the *guru*), and embraces a cultural identity rooted in *Pencak Silat*. A predicament for the *Pencak Silat* apprentice lies in the sense of commitment or obligation toward a master (and school) for having shared this secret knowledge with him or her. As an apprentice, I experienced this feeling of obligation, the sense of duty and responsibility toward the *perguruan*, especially while experiencing the discomfort of simultaneously studying

¹¹ Female *Pencak Silat* masters do exist, and during my fieldwork, I got in contact with female masters, too. Yet, the headmasters of both *Merpati Putih* and *Sang Cipta Rasa* are male; therefore, the masculine form is used here.

(in) various schools. The sharing of (religious or scientific) knowledge, and the resulting extraordinary abilities an apprentice experiences, constitute a firm bond with the master—a bond that is anchored in affective and relational dimensions. This defines embodied practice as an active constituent in the construction of identity and meaning.

According to my personal experience, at the core of this deep bond between a *Pencak Silat* master and his apprentice lies the sense of duty or obligation on the part of the latter. That is why the feeling of not being able to fulfill the expectations placed in me, not being able to give something back, came close to an emotional impasse. Toward the end of my research year, these experiences and emotions, and preliminary findings from my comparative analysis compelled me to develop a new strategy. A shared common aim of *Pencak Silat* masters and practitioners is to raise awareness for their beloved Indonesian martial art nationally and internationally. Despite competitiveness and dissociation between different schools, *Pencak Silat* is perceived as a nationally shared common cultural asset. My contribution to the promotion of *Pencak Silat* (inter-)nationally was acknowledged and valued by masters and practitioners regardless of affiliation. I was able to make this contribution by attending large public (media) events and participating in public performances without visible affiliation to a certain *perguruan* (i.e., wearing a school's emblem or uniform). After the completion of my fieldwork and my return to Germany, internet research for an article provided unexpected affirmation of this strategy:

The picture of me was taken at a public *Pencak Silat* festival at Malioboro street in Yogyakarta, where practitioners from all over Indonesia represented their schools and skills (Fig. 1). Instead of wearing a particular school uniform, I chose to wear neutral black *Pencak Silat* clothes with the emblem of the superordinate, national *Pencak Silat* Federation sewn on it. The image caption “*Bule aja bangga sama budaya Indonesia*”—“Even a Westerner is proud of Indonesian culture”—indicates that the common aim of promoting *Pencak Silat* as an Indonesian cultural asset prevails over competitiveness. This, at least, is what is portrayed in social media. At a more fundamental level, it indicated to me what a Western apprentice is obliged to achieve or “give back”: to value the culture of Indonesian *Pencak Silat*, one which questions his or her Western worldview, perception, and moral and ethical preconceptions. It shows that, beyond dialogical research, the democratic flavor of martial arts studies essentially lies in the acceptance and exchange of embodied cultural knowledge.

Conclusions: Apprenticeship: From Impasses to Learning Effects

The construction of martial arts must be regarded as a continuous process which is circulated through cross-cultural, mutual exchange, and learning, via media technologies and onsite experience. The discursive formation of the martial artist *par*



Instagram photo by ██████████ - @teministaa
on Sun May 31 2015 at 9:28 - INK361

Bule aja bangga sama budaya Indonesia, masa kamu enggak? (festival pencak silat nasional) #explorejogja #folkindonesia #silat #culture on Sun...

INK361.COM

Fig. 1 “Westerner proud of Indonesian culture”—Instagram photo

excellence, the Oriental Monk (and his general features, absorbed by popular consciousness through mediated culture), shapes the aims and ideals of practitioners everywhere. Both in “the West,” as in Berg & Prohl’s example of the Shaolin Temple Germany (Berg & Prohl, 2014), and in “the East,” that is, in the countries of origin of various martial arts, it influences self-conception and concepts of the (ideal) martial arts master. Yet, neither a master nor a student are “hyperreal” figures in Baudrillard’s terms. Images and discourses in local and global media spheres simply provide an additional reference to embodied learning and intersubjective bodily experience. The martial arts promise of self-improvement, the “work on the self” in Foucault’s sense, requires active action and participation. Bowman’s statement, cited above, referring to the martial arts promise and its requirement of “*nothing more than physical discipline, dedication, devotion and diligent training*” (emphasis added) must sound ironic to anyone who has actually tried it. Moreover, Indonesian *Pencak Silat* combines hard physical training with mystical-spiritual practice. Personal, physical experiences of extraordinary powers and phenomena challenge the rational and intellectual comprehension the “Western” apprentice, and researcher is supposed to be grounded in. This points to the difference between “seeing something in a video” (mediated), “seeing something yourself” (immediate), and actually performing and experiencing it yourself. Performance and bodily experience provide the basis for a more differentiated self-perception and access to the holistic *Pencak Silat* education that incorporates physical, perceptual, intellectual, cultural, ethical, and religious aspects. This represents the primary argument for performance ethnography in martial arts studies, for participant observation that sets embodied,

perceptual experience as the analytical ground for human participation in the world (see Csordas, 1990, 1993). Performance must be regarded as an “ontological device” that renders tangible conceptions of self and other, student and master, and social relations as instantiated in such a close, personal bond of trust.

‘Epistemology,’ how to know, via experience, exposure, then, is intimately tied to ontology. Ontological assumptions concerning the subject, for example, whether societies are fundamentally moulded by economic structures or religious actions, condition epistemology—how to know—with scientific, positivist/realist/Marxist or phenomenological/interpretivist theoretical perspectives. (Farrer, 2015a, p. 1).

In the two Indonesian *Pencak Silat* schools portrayed, either religious or rationalist (positivist) explanatory approaches reinforce a specific epistemology grounded in embodied experience. In reaction to the Indonesian state’s modernization measures, the disenchantment and (re-)enchantment of formerly mystical practices can be identified as the different styles and strategies of *Pencak Silat* schools. Despite the illustrated difficulties of exclusivity and rivalry between schools, my comparative analysis allowed for conclusions regarding a common ground shared by different communities formerly classified as “mystics.” Performance ethnography generated conclusions beyond mediated and even immediate observation. The ontological device of performance, of embodied participation, provided a more thorough appreciation of what is really going on: it disclosed the convergence point of efficacy and entertainment in *Pencak Silat* (or martial arts in general). Beyond the martial arts promise offered to the audience of a performance or in media representations, the practitioners’ persistent belief in the fulfillment of this promise is based on the bodily experience of actually getting closer to the desired ideal. Nonetheless, a martial arts apprenticeship is a relationship of trust. The student is self-confident, but must respect and trust the master and their application of extreme training and testing methods. The master’s primary concern is that his apprentice values the imparted knowledge and uses it respectfully, in a productive way. This means it should not be used to hurt people and not be passed on to others who might use it to hurt people. Instead, “every student is obliged to find trustworthy people to whom the knowledge can be passed on, be it in Indonesia, in Germany, or elsewhere.”¹² Masters like GusMus or MasPoeng accept a Western apprentice on the premise of helping to promote (their) *Pencak Silat* in Indonesia and disseminate their knowledge in “the West.” As a researcher and apprentice, I did not fully meet their expectations. My scientific knowledge was not seen as useful, my contribution to the public promotion of a given school was limited. I have not yet demonstrated *Pencak Silat* publicly or opened a school in Germany. Mas Poeng’s offer, which initially filled me with honor and pride, ultimately evoked feelings of pressure, deficiency, or even guilt for not being able to fulfill his expectations. Yet exactly these emotions helped me to better understand or reflect upon my research topic, *Pencak Silat* education and practices, and my methodology. The relationship of trust between a mas-

¹² GusMus, interviewed in Yogyakarta on September 4, 2016. Translation from Bahasa Indonesia by the author.

ter and his apprentice implies far-reaching obligations and expectations; the gift of knowledge imparted by a *guru* always comes with a sense of responsibility or (ethical) obligation toward the *guru*, his school, and his *Pencak Silat*. My comparative approach restricted the scope for dialogical research, performance, and media engagement during and after my research. Nevertheless, apprenticeship, performance, and embodied participation in different *Pencak Silat* schools offered an opportunity to identify overall common educational principles, common to all *Pencak Silat* schools in Indonesia. My comparative analysis of embodied experience in different schools revealed essential characteristics of the martial arts promise of self-improvement in Indonesia. It made the continuous authority of a *Pencak Silat* master and the trust and respect shown to them by students plausible and comprehensible. My contribution on the ground, what I was expected to “give back,”¹³ was physical involvement, openness and trust, respect toward the master, and assistance in promoting *Pencak Silat*. The shared aim of raising public awareness of Indonesian martial arts culture (as expressed in the Instagram photo) points to the conception of the collective self in *Pencak Silat*, which resonates with the message of the Karate Kid films (those of the 1980s and of 2010). In increasingly globalized, mediatized world, learning martial arts can provide a basis for communication, solve interpersonal and societal problems, and bring initially “different” ontological perspectives into dialog.

Reflecting on participation and apprenticeship as research methods, beyond Indonesian *Pencak Silat* and martial arts, two conclusions can be drawn. First, dialogical research and collaboration with research participants and partners at eye level—approaches that challenge imbalance and the reproduction of colonial structures in academia—are a promising yet challenging endeavor. This chapter indicates ways of dealing with the limitations of these approaches, and the emotional predicaments of reaching one’s own limits while trying to implement them. The researcher must acknowledge that there might be different understandings of “collaboration,” that mutual expectations and commitments might differ fundamentally vis-à-vis local power asymmetries. Second, I am inclined to argue that emotional predicaments related to apprenticeship relations can be considered symptomatic of any ethnographic research. The gift of knowledge always comes with expectations of reciprocal consideration. Researchers and apprentices, I suggest, can transform feelings of discomfort, obligation, guilt, or deficiency into their motivation to take responsibility by seeking a deeper understanding their mentors’ expectations.

¹³ See section “[Reciprocity in Research Relationships](#)” on expectations to “give back.”

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The *Anxieties* of a Changing Sense of Place: A Reflection on Field Encounters at Home



Paul J. Kellner

The goal of this chapter is to reflect on my first experience engaging in long-term ethnographic fieldwork through the lenses of emotion and affect in a way that might prove useful to other first-time ethnographers preparing for their research. Specifically, I will explore how an already well-established sense of place at my fieldwork site became emotionally disruptive for me, leeching more deeply into my person, and engendered affects which I carried with me through the early days of my fieldwork. I will signpost how anxieties about safety at home that were unanticipated in my preparations were reflected upon more systematically in the aftermath of fieldwork. I find that the disruptive emotional experience of anxiety reshaped or impeded the interpretive process I was carrying out between ethnographer and interlocutor; therefore, emotions and affect are aspects of fieldwork about which I wish I had been more reflexively aware. This experience provides support for the growing literature that argues that emotions can broaden and deepen an ethnographer's insight into her and her interlocutor's lives (Stodulka et al. 2019). I will also briefly discuss the important role of privilege (see Twine and Gardener 2013), and its emotional implications (see Kimmel and Ferber 2014) in my responses to a shifting sense of place. Though the chapter will take a confessional tone, this approach is not seen as an end in itself. Rather, I view the reflections as a first step in more systematically engaging with emotions and affects in future fieldwork. Finally, I will posit a few practical ways to prepare for and implement increasingly "relational and affectively-aware" (Stodulka 2015, p. 85) ethnography, in order to help diversify the toolkit of other first-time ethnographers.

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The Fieldwork and My Sense of Place

The experiences described in this chapter took place in a medium-sized Indonesian city, which I will call *Kota Kaki Gunung*,¹ between June 2015 and May 2016, in the course of doctoral research exploring how street-associated youth perform and construct their identities in social and spatial contexts. The research focuses on how youth identify and categorize themselves, how others categorize them, and how these dynamics shape youths' relationships with peers and communities. Several ethnographic methods were used, including participant observation, unstructured and semi-structured interviews, and youth-led tours of their neighborhoods. Most of the core interlocutors were first encountered while residing in a volunteer-run homeless shelter, but the field sites included street-side informal workplaces, places for socializing, and places where youth encountered law enforcement or social services. Before departing for fieldwork, I had prepared myself for the practical and emotional aspects of fieldwork engaging with street-associated youth: I reminded myself of lessons I had learned in my experience conducting applied research with displaced young people in several countries; I reviewed resources on secondary trauma and the ethical clearance process; and I considered how I would respond to a wide range of potentially stressful situations that may arise in the lives of my interlocutors. However, I had not prepared myself as effectively for how emotions in my home life (i.e., the neighborhood or "home" where I resided during fieldwork together with my wife) might shape the day-to-day implementation of the research. Therefore, this chapter focuses primarily on the affective dimensions of fieldwork that are derived from interactions beyond my encounters with street-associated youth, activists, volunteers, or bureaucrats, and how the former shaped my understanding of the latter.

During the work, my existing sense of Kota Kaki Gunung shifted from a place that I thought of as contained, bounded, and singular, to a sense that the city's identity had become multiple and its values no longer monolithic. This shift is reminiscent of a wider change that took place within cultural anthropology in the 1990s, away from sense of place that involved contained, bounded identities towards "boundary erosion" (Feld and Basso 1996, p. 6; see Gupta and Ferguson 1997). Such boundary erosion in my sense of Kota Kaki Gunung engendered anxious feelings and heavy affects that I carried into the field each day. In this reflection, I hope to explore how affective aspects and senses of place intertwine (Aoki and Yoshimizu 2015), and how such a process can shape the practice of fieldwork.

My Kota Kaki Gunung Before Fieldwork

I first came to know Kota Kaki Gunung when I went there in 2010 to learn Indonesian. It was an ideal place to learn a language—a college town with friendly relaxed people. It was clear that it is a place with a strong sense of its history and

¹I have chosen to pseudonymize Kota Kaki Gunung to foreground my emotional and affective experiences as the purpose of this discussion. Kota Kaki Gunung is a well-known city, and readers' own knowledge of or experiences in the city may distract from the purpose of this piece.

culture, and that it seeks to share these things with its many international visitors. At the time, I stayed in a neighborhood full of international, and internationally minded, students. Shortly after my arrival in 2010, I also met my future spouse. We spent many of the formative moments of our relationship sitting in street-side food stalls in the same areas where I would eventually do my fieldwork. It is, and will always be, my partner's home city. Kota Kaki Gunung's culture and mindset are deeply embedded in who she is, and her personality reflects many of the cultural norms synonymous with the city. She is keen on multiculturalism and highly accepting of diverse people and viewpoints, aspects that also reflect my sense of the city. These traits of the city, therefore, constitute a meaningful part of our relationship. Kota Kaki Gunung is also a place where we both learned how to negotiate and build our intercultural relationship. I spent many weekend visits with my future in-laws, getting to know them and their cultural and religious traditions. My new family led me to make several visits to the city over the next several years, and their proximity was a welcome bonus to selecting the city as my fieldwork site. A few months before beginning my 2015 fieldwork, my partner and I had a wedding reception in Kota Kaki Gunung and moved back to the city. We found a house in a quiet neighborhood on the edge of town and began our married life.

If I reflect on how I have described my experiences of Kota Kaki Gunung to others, some key points emerge. At the outset, I want to emphasize that although these impressions have been elaborated upon by undertaking fieldwork, my positive impressions of the city remain and have been reinforced. It is still a place for which I care deeply and have a great affinity. I've often experienced Kota Kaki Gunung as a hub for culture, arts, and education that embodies and encourages the national motto "unity in diversity" (*bhinneka tunggal ika*). For example, I remember bringing a foreign guest to a dance festival that was being held in the center of town in 2011. It featured traditional dancers from all over the country. This type of event was and is commonplace in Kota Kaki Gunung, and articulated the idea of unity in diversity in an everyday circumstance. Although such representations of the idea of unity in diversity have more complicated historical roots in former president Suharto's depoliticizing nationalism (Pemberton 1994), my experience of events like the dance festival created a persistent impression that the city thrived on multiculturalism. I took pleasure in recreating that impression for others.

Second, I had long described it as a very safe and hospitable city. Family and friends who know little about Kota Kaki Gunung have often asked about safety, and I have been quick to explain that violent crime in my home country is likely more common than in Kota Kaki Gunung. For instance, in my home, such events often involve firearms; in Indonesia, that would be a rare and very serious event. As with the emphasis on multiculturalism, there exists a more complicated history, and the absence of violent crime is likely an effect of the colonial, Japanese, and New Order security regimes that were rooted in neighborhood surveillance (Barker 1998; Kusno 2010). However, my experience, nonetheless, created a strong sense that I was safe, and all my time in the city has confirmed this notion. Moreover, Kota Kaki Gunung is exceedingly hospitable. When I first moved to Kota Kaki Gunung for my fieldwork in 2015, I was in dire need of a haircut. I asked a group of young men who often hung out across the street from my house where to go. One fellow, Dino,

immediately said, “I’ll show you; get on my bike.” He drove me to a hip-looking shop about a mile from my house. When I got off the bike, he asked, “do you want me to wait for you?” I thanked him and told him I would walk home. I wanted to get to know the neighborhood. Shortly after he drove off, I realized that I had forgotten to give him a tip, which would be a common thing to do. When I returned later that day, Dino was by himself in the same place I had met him before. I said thank you again, and then apologized for not giving him a bit of money for fuel and offered a few 1000 Rupiah. He replied, “Don’t worry about it; I [did that with] sincerity (*nggak pa pa, saya ikhlas*).” I have had several similar interactions with strangers in Kota Kaki Gunung, which for me demonstrate a high degree of hospitality rooted in local norms of hospitable actions and emotional performance (Stodulka 2017a, p. 62).

In short, spending large amounts of time between 2010 and 2017 in Kota Kaki Gunung helped me to develop a strongly positive, however limited, sense of place. I knew my international neighborhood and my family life. However, there was a great breadth of experience in the city with which I was not well acquainted. I had worked on poverty and health issues with children and youth in several countries, and had collaborated with Indonesian colleagues at NGOs and universities on programs for children and youth in the past. Therefore, I had a sense of how in-depth engagement with street-associated interlocutors might reveal new and challenging ideas about a place of which I was so fond of. I prepared myself for many of these challenges from the perspective of a researcher, reflecting intensely on building rapport with my interlocutors, i.e., by ensuring that I was respectful of their experiences and the time they shared with me, by creating reciprocal relationships, and by responding to emergency situations during the research. However, I was less well prepared for how experiences of anxiety in my home life, and their affective implications, would shape the daily work of my research.

Approach to Emotions, Affect, and *Anxieties*

This chapter employs Stodulka’s (2015, p. 85) definition of emotions as “embodied biocultural processes between and within persons.” This definition is well suited to this analysis, as it places key emphasis on emotions as interactive processes with concrete, bodily implications. The following discussions will center on the processes of interaction that took place between myself and others, between myself and the city, and between my previous sense of place and the new ideas I was confronted with, and will describe the bodily implications of these various interactions. In other words, I will pay “direct attention to how ‘feelings’ are generated in dialogue with the world” (Pelkmans 2013). Moreover, because some feelings yielded a palpable, observable affect in me, I intend to integrate the concept of affect into my theory. Jeremy Gilbert (2004) paraphrases Brian Massumi (1995) to define affect as “organized experience, an experience probably with empowering or disempowering consequences, registered at the level of the physical body, and not necessarily to be understood in linguistic term.” The idea of affect will be employed because theorists

have emphasized the pre-cognitive ways in which affect can shift “bodily intensity” and reshape how one engages in social worlds (Athanasidou et al. 2012). Moreover, there is value in consciously considering the bodily intensity of, energy associated with, and empathy engendered by emotions (McLean 2007). Thus, these issues will provide the backdrop for my argument that taking emotions and affect more seriously in fieldwork may open new opportunities for intersubjective processes.

Finally, this reflection will be loosely framed in terms of Michalowski’s (1996) notion of the anxiety of surveillance during fieldwork. His work puts forth the idea that reflexivity in fieldwork should be sensitive both to the fieldworker’s biography and towards the macro-political processes in the lives of informants and the fieldworker’s post-fieldwork audiences. Although my research site did not have acute political tension hanging over it, like Michalowski’s work in the context of tense US–Cuba relations, this framework proved a useful starting point during my analysis for systematically thinking about the relationships and processes that shaped my emotions and affect. For a first-time ethnographer like me, such tools for building one’s reflexive practice are highly welcome. In brief, Michalowski’s work builds on de Certeau’s (1988) notion that power is expressed in two basic forms, and generates two forms of pleasures, namely disciplinary and discursive pleasures. Through an examination of his reflexive anxieties about undertaking fieldwork in the context of tense US–Cuba relations in the early 1990s, he developed notions of disciplinary and discursive anxieties. He describes *disciplinary anxieties* as akin to the idea that “my behavior is being shaped by others.” He describes two sub-types of anxiety within the concept. *Surveillance*, the sense that “I am being watched,” and *control*, the suspicion that others are exerting agency over one’s experience. *Discursive anxiety* is described as asking the question, “Can I trust my interpretation of others’ motives and underlying meanings?” The sub-type that will be applied in this discussion is *interpersonal*, which he describes as the question, “How are others framing their discourses for my position?” (Michalowski 1996, p. 69). My reflection will solely focus on his notion of *surveillance*, though I found each domain of anxiety he describes useful in post-fieldwork analysis. This said, most salient were my experiences of feeling conspicuously different, being observed but not accepted, learning new social norms, and simply not having as much physical privacy as usual while not conducting fieldwork. These undermined a previously stable sense of place and engendered burdensome affects that I carried to the field.

Feeling self-conscious

Michalowski’s notion of *surveillance* is a good starting point, as this was the most immediately felt, and possibly most potent form of *anxiety* during my fieldwork. Although the sociologist refers to affective experiences when researching in totalitarian regimes, a Javanese *kampung*² can at times feel similarly authoritative with regard to its complex neighborhood obligations and idealized standards of moral

²A small neighborhood, usually set away from a main street.

conduct (Beatty 2010; Newberry 2006; Reddy 2001; Stodulka 2017b). I was observed more than was normal for me, and this led me to be highly self-conscious and vigilant of the ways in which I was being observed. It resulted in emotions of worry, anxiety—or “stage-fright” in Keeler’s (1983) terms—and alienations that slowly reshaped the affect that I carried into the field. It is prudent to mention my background before discussing these experiences. I am from the midwestern United States. My city has a sociable tone, but also affords one a great deal of privacy. Moreover, the time directly preceding this fieldwork was spent living in the UK and Norway, both places in which, generally speaking, one has the ability to place wide boundaries around one’s privacy, personal life, and body. Having become accustomed to such a level of control of personal space and comfort is a highly privileged position. It speaks to privilege in the form of a wealth of control over how and when I am seen in public space. I had spent time reflecting on other aspects of my social position such as ethnicity, nationality, education level, and wealth that would affect my interactions, but I had not unpacked my own privilege regarding feelings of space-related comfort and security before the research.

At Home in a New Neighborhood

My wife and I moved into our new home in a sleepy neighborhood populated primarily by retirees. I was initially pleased. When we moved in, the neighbors barely acknowledged us, and in the following days they mostly stuck to their well-worn routines. One would have hardly known that we had moved into the neighborhood if it wasn’t for a few young people who hung out at the corner shop treating us as a novelty.

As time went on, the subtleties of adapting to my new surrounding put me off balance. The last time when I had lived in Kota Kaki Gunung, I had stayed in an international student neighborhood. I stayed in a large shared house in which I had plenty of private space, and I was physically and socially insulated from the street outside. The leaseholder of the house maintained many neighborhood social obligations that follow from living in many areas of Kota Kaki Gunung, for instance, offering payments and support to the local neighborhood watch (*ronda*) or chatting with the neighborhood head to ensure she/he was informed and happy. I reasoned that even in our new, quietly accepting neighborhood, we would be responsible for these and any other obligations—a social experience that I was looking forward to navigating.

On the whole, the neighborhood, like Kota Kaki Gunung as a whole, was very friendly. However, my previous experiences in Kota Kaki Gunung had often made me feel as though people were keenly and kindly interested in difference; our new neighborhood, however, did not give that impression. Many new acquaintances beyond our housing compound approached initial interactions with a skepticism that I found surprising. In Indonesia, it is common to ask someone new about matters that might be too private or personal in my country of origin. For instance, one’s faith or marital status might be brought up early in a conversation.

From previous experiences in other neighborhoods, I had grown accustomed to religious questions being accompanied by interlocutors kindly teaching me about religious law, discussions of values shared between faiths, or even a warm invitation to further discussion or to join a religious service. However, I had fewer encounters with this warm tone in my new neighborhood, and it made me feel conspicuous as a member of a non-majority faith. Alongside this, there were a few members of the community who outright ignored me as I went about the neighborhood. Based on my previous experience in the city, this could be seen as a negative social gesture. Some neighbors averted their eyes or blankly stared when I said “hello.” In other places, I gained the impression that acknowledging others’ presence in public space was not only a norm, but a social obligation that, if not fulfilled, could lead to one being seen as problematically too proud, aloof, or not sociable enough. At first, I responded to averted eyes and blank stares by re-doubling my efforts to connect with the community in the ways that I knew how, but some new connections were unsuccessful. Through these experiences, I began to feel that many of my neighbors were not merely indifferent to my presence; rather, I was, for some reason, not being included in the niceties of usual neighborhood life. In short, they were aware of us, and in some way disapproved. I can only speculate as to the motivations behind this behavior, but if I were to do so, I would assume that most people simply didn’t feel they knew how to act around me. I began to feel out-of-sorts, and when I discussed these feelings with my wife, she was mostly relieved because she said it meant that we were much less likely to be pulled into aspects of neighborhood life that can be rather time-consuming. I didn’t share that sentiment and began to feel frustrated at home because I had the impression that I understood the social norms for neighborly interactions, but that I, in this place, had either misunderstood or violated them. My wife’s perspective offered the insight that I likely perceived the social norms and forms of obligation correctly, but that our lack of inclusion should not be cause for concern.

I also began to feel self-conscious at home due to mild discomfort engendered by the house itself. Our complex featured six small detached apartments that could see directly into our home when our windows were open. The house was also receptive to sound. Tall ceilings and walls made of concrete ensured that even the footsteps and hushed conversations of those walking on the road more than 25 m away could be heard. Conversely, conversations from inside our house at a normal volume were audible from the street as well. Physically and socially, I felt very exposed. I had not prepared myself for how “public” even minor moments in my day would feel. This again brought my privilege to light in the form of an assumption that I deserve a private, quiet space. These atmospheric discomforts of feeling unwelcome and physically exposed left me with uneasy emotions entirely derived from my own privilege and assumptions about the norms of community in Kota Kaki Gunung.

As my unease in the neighborhood was taking root, I began to notice signage that also made me feel worried. At first, I noticed the flags of the activist wings of a conservative political party. This made sense, as I knew that political party was popular in the area. I also knew that our neighborhood was near a district border, on the other side of which such displays were much more common. However, as time

went on, I noticed other signs, murals, and flags invoking the names of GAM³ and Hamas. For me, it was one thing to see signs for a political party about which I could read in any newspaper, but these newly invoked names made me feel self-conscious. And so, I began to wonder and worry, could this be part of why my neighborhood felt like an unwelcome place for me? How am I being perceived by members of the community who may disapprove of people of my religion or nationality?

When turning these questions over in my mind, I mainly relied on local friends and family to tell me what they knew about these expressions of bold political opinions. They invariably said, “This is a common thing, you don’t have anything to worry about.” It was indeed common. As I reacquainted myself with Kota Kaki Gunung, I could see similar artwork in several conservative neighborhoods. Newspapers indicated very few incidents indicating intolerance, and none directed at foreigners. My under-examined position of privilege created a critical blind spot. In retrospect, placing my anxieties and their origins within a wider view of considering how they compare to the same anxieties experienced by my respondents could have opened the door to greater affective exchanges during fieldwork. Because, indeed, my respondents, and others whom I encountered on the streets, *were* sometimes the targets of the intolerance that caused me my selfish worry. Yet, my worries led me to question whether my neighborhood was the right place for my wife and me to live. This worry created an atmosphere that undermined my previously felt sense of comfort and safety in Kota Kaki Gunung. For the first time in this place, I felt that my difference may not be an object of interest, but a liability.

Effects, Responses, and Reflections

My worries were present during the early days of my fieldwork. They lowered my energy level and slowed me down. I had low morale and brought nervousness and reticence with me into various social settings. Experienced ethnographers had primed me for the possibility that the early months of my research would feel laborious, and that building a rapport with my interlocutors would take patience. Hence, I initially took this heavy feeling to be simply a manifestation of this laboriousness. However, after reviewing my field notes from this period, it is now clear that I carried my anxiety about surveillance with me, in the form of affect, into the rhythms of community life in my field site. This affect was present at the street-side hangouts and volunteer-run shelters. The lazy weight of worry hung over me like low-hanging clouds, and I found it difficult to get up and out the door to spend more time with my interlocutors. I took more opportunities to return home when the opportunity arose. I was reserved and more awkward when working with my interlocutors. One of the volunteers at the shelter observed that I was slow to warm up, and asked me when I would begin more active research. I found myself slow to start my research on many days, finding distraction in more background reading, administrative work, or tidying up.

³A separatist movement active in Aceh province for several years.

I also remember carrying this affect into other moments that were characterized by my being more impulsive and less persistent. In particular, I became impatient with interviews scheduled for late in the evening, in distant, uncomfortable settings. When interviewees were not punctual, I would often attempt to reschedule. A fact of many research projects with street-associated youth is that it is very hard to schedule fixed times at which to do anything. There are always latecomers, “no shows,” and changes. Moreover, Kota Kaki Gunung has some communities in which being an hour or two late to an appointment may be acceptable. Yet, my heavy affect muted my ability to accept this fact, and in that period, I often gave up early on potential moments of intersubjective learning and understanding. My emotions impeded my capacity to be attentive to the highest degree possible at that time. As the research gained momentum, the lazy clouds of worry and discomfort evaporated. I was grateful to the family and friends, colleagues, and interlocutors who had been patient with the results of these emotions. However, it is evident now that, had I taken a more emotionally aware approach to my reflexive practice, I could have been more present and engaged in the early stages of the fieldwork. First, regarding my unfounded worries about safety, my intense emotions at home made processing information from the field more challenging. I believe that systematically taking time while writing my daily field notes to check in with emotions would have helped me recount more effectively the raw data of my experiences as well as the emotions that I was feeling at the time. It now seems clear that applying a critical, analytical lens to my worries about safety would have helped me take a closer, dispassionate approach to these emotions. Additionally, it would have been of value to more fully acknowledge how emotions and affect may have shaped the ways in which I was perceived by others, how they may have fed into my own assumptions or biases about others and my field, and how my lack of energy may have muted many potentially lively, more empathetic interactions. Additionally, regarding my worries about safety, I found myself with lingering doubts even after several reassurances from local friends and family. In hindsight, it feels clear that my emotions prevented me from better engaging with the evidence and experience that my confidants used to provide me with reassurance. Only when an Indonesian mentor who had spent many years in the US reassured me, priming me with language that affirmed my concerns and then communicating evidence in a way that I found comprehensible, did I start to trust this message. In other words, he affirmed my desire feel individually heard, and communicated with great specificity, because he knew how to communicate evidence that I would perceive to be valid. My local friends and family had reassured me with very similar messages, but sought to *show me* that I should not be worried by having the confidence to demonstrate that such worries could be dismissed outright. I find that my strong desire for specificity and the affirmation of my individual feelings was often incompatible with my friends and family’s emphasis on a trust that was built on plausibility and reasonable community consensus. Had I been more reflexive about the ways in which my emotions mediated my uptake of support from local friends and family, I could have more effectively engaged during the early days of fieldwork. This issue again puts my privilege into stark relief. The idea that it took some time for me to feel reassured by dear, trusted friends and

family was presumptuous of me. I had immersed myself in a fieldwork site in which I should always have sought to better understand. I had carried that perspective into my research, but not into my home life adjacent to the fieldwork. I needed to employ a similar approach to communication in my home life, yet I stubbornly adhered to my most comfortable notions of what constituted valid evidence and emotional care. Pre-fieldwork practice in reflecting on individual privilege, position, assumptions, and biases—and how they might be manifest in multiple places, not just the field—might have been valuable, especially if such practices included a component that examined how such issues shape emotions carried into daily fieldwork and analysis. One mode of doing this might be utilizing emotion diaries as described in this volume's introduction. Another method could be practicing journaling techniques borrowed from fields like cognitive behavioral therapy to identify potential emotions that result from such concerns, appraise the potential realistic ramifications of behaviors driven by those emotions, and determine how such behaviors might lead to ineffective engagement in fieldwork.

Conclusion

Despite the fact that my research did not take place under a tense political climate like Michalowski's, my feelings of self-consciousness engendered worries and a heavy affect that meaningfully shaped my fieldwork practices for several weeks. It is likely that such emotions and affects could intensify in contexts where experiences of surveillance are more substantial, and where concerns over security were more real. These worries about insecurity upended my pre-existing sense of place in Kota Kaki Gunung and proved to be disquieting in a way that elucidated my failure to incorporate my personal historical sense of place into my preparations for fieldwork. I had begun my fieldwork with the unacknowledged assumption that the identity of city that I knew was static and bounded within its friendliness and keen support of multiculturalism. Yet, when I arrived, these boundaries eroded, and the city's multiplicity and dynamism were revealed to me through a few everyday experiences. As a first-time ethnographer, I believe that I could have benefitted from additional skills and strategies for better anticipating the ways in which such emotions and affect might appear in my fieldwork practice, means for more reflexively attending to them while in the field, and analytical methods for accounting for such experiences in my writing. Specifically, this could mean more affectively-aware pre-fieldwork pedagogy in the form of keeping a fieldwork diary featuring emotional and bodily awareness as critical aspects. Additionally, pedagogy might also do well to encourage researchers to more thoroughly examine their pre-existing senses of place and people in their fieldwork site, and take more seriously the way that intended fieldwork may complicate or uproot such impressions. Building on this, analyzing the ways in which ethnographers' positions of privilege drive their fieldwork-related emotions and affect may be an exercise that helps researchers both better acknowledge their own limitations and find additional means of

emotional support in the field. Rigorous pre-fieldwork consideration of emotions and affect can be complemented by journaling and note-taking practices in the field, as well as discussions of self-care in challenging field sites. Journaling using techniques based on cognitive behavioral therapy may have helped me separate thoughts and emotions more effectively in my own fieldwork, and therefore may have helped me to be more present in empathizing with my interlocutors' shared emotional experiences. Finally, this notion of being more present and able to empathize with interlocutors is vital to the process of analysis once one returns to the field. The calm, space, and privacy of my own return from the field helped to view my emotions and affect from new vantage points, and only then was I able to better empathize and understand some of the interactions I had in the field. I will carry these lessons with me into future fieldwork.

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Attuning Engagement: Methodological and Affective Dimensions of a Failed Collaborative Research Project in Timor-Leste



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Introduction

At the end [of the first meeting] each of us told the others about the feelings we were going home with. I felt thankful for sitting in this circle, for the fact that it had felt so easy to prepare this session, but I was mostly thankful for all these young people full of enthusiasm, lust for learning and commitment. I was reminded of the fact that this aspect was what had always inspired me so much here. And I don't expect all eleven of them to present a paper in December. Even if there are only four or five, it would already be great!

(Diary entry 11 April 2015, Dili, Timor-Leste)¹

What did I do wrong?! Why could everybody make it in the beginning, and now they are all too busy? Is it really that boring?! I thought I had communicated clearly to them that joining this group meant commitment...maybe they have different standards from me... I don't blame them. I will not allow myself to blame them. I have to accept that they have a lot more responsibilities than I do, more difficult lives. But what drives me almost insane is the idea that it is me who is making the mistakes, that they just do not enjoy it anymore and that they do not tell me, but just, one by one, they have better things to do...

(Diary entry 13 June 2015, Dili, Timor-Leste)

“*Mana*² Sara,” João³ said to me in December 2012 when I was preparing to leave Timor-Leste, a country I had fallen in love with during 5 months of fieldwork for my master's dissertation, “When you come back for your PhD, I want to do research together with you, so that I can learn about doing research.” João was not the only

¹Diary entries were originally Dutch and have been translated by the author.

²Tetun (the Timorese national language) for “sister,” which is the polite way to address an unrelated woman.

³All names are pseudonyms.

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university student who had expressed interest in doing qualitative research together with me over the course of my fieldwork in Timor-Leste. Many students had been intrigued by the fact that I did research without questionnaires and they wanted to know more about it. So when the wheels of the plane lifted away from Timorese soil, I was determined that the next time I touched down on that airstrip, I would arrive with a prepared collaborative research project. During the intervening two-and-a-half years, I finished my master's degree, wrote a PhD proposal, studied the principles of collaborative research, and attended a "participatory action research"⁴ workshop organized by a group of young academics and activists interested in this method. I began to believe that I was well prepared.

Collaborative and participatory forms of research became increasingly appealing to me as I progressed through my studies in cultural anthropology. Several factors convinced me that research should consist of an *exchange* and not an *extraction* of knowledge: my previous experience of being confronted with the shortcomings of the daily practice of conventional ethnographic research; the astonishing willingness of people to participate and invest their time in my research; and the sad reality that almost all the literature about Timor-Leste is written *about* Timorese and not *by* or even *with* Timorese. In advocating a more engaged and reciprocal form of research, I of course do not stand alone. According to Johnston (2010, p. 245), the "call for a socially relevant action anthropology echoes across the generations." The field of active and engaged research approaches is vast and varied, containing a patchwork of sub-fields such as engaged research (Beck and Maida 2013; Low and Merry 2010), collaborative research (Lassiter 2005; Okwaro and Geissler 2015), participatory (action) research (Bastien and Holmarsdottir 2015; Bergold and Thomas 2012; Hemment 2007; Reason and Bradbury 2006), and activist research (Cancian 1993; Hale 2006; Huschke 2015), to name just a few. These sub-fields of course partially overlap and borrow from each other.

The common denominators among them that resonated most with me were, first, that research participants could play an active role in research practice and preferably also in the process of analysis and writing, and, second, that research practice and results could in some way have a direct, beneficial impact on the lives of the research participants. I oppose (with Johnston 2010) the idea that only applied research taking place outside of academia can fulfill this premise. Additionally, I argue that academics do not only have a responsibility to their research participants, but can also profit from a closer collaboration with them. The intense mutual engagement with the research topic can yield a much richer understanding of participants' emic perspectives and life situations, as I will elaborate further below (see also Hale 2006, p. 13; Huschke 2015, p. 61). This, however, does not mean that there is a clear-cut, successful way of conducting engaged academic research. This chapter highlights how challenging this attempt can be, in both methodological and emotional terms.

As various research participants had, over the course of my previous fieldwork, expressed the desire to conduct research together with me, I envisioned a (partially) collaborative research design, which I will present more extensively below.

⁴See for example Kindon et al. (2007).

Furthermore, I saw collaborative research as a method that would put me, as a researcher, among the people I was studying, instead of above them. Engaged and collaborative forms of research are often carried out in an attempt to create less hierarchical relationships between researcher and researched. This attempt is not always successful. Okwaro and Geissler (2015, p. 507), for example, point to the power imbalances and potential for exploitation inherent in “North–South” collaborative research projects in medical science. They argue that in collaborative research, there is a “need to find ways of speaking and acting about inequalities and diverging interests, as well as about one’s shared commitment to equality, to make strides toward an ethic of truly equitable partnerships.” In addition, Lassiter (2005, p. 96) states that the power anthropologists have in the process of collaborative research “must not be underestimated.” It is therefore of crucial importance to describe in detail the nature of the collaborative effort (ibid.). Finally, Bastien and Holmarsdottir (2015, p. 5) refer to the necessity of moving “beyond tokenism” in participatory forms of research and to provide research participants with more substantive forms of participation. These are warnings I did not seriously consider before going into the field, as I naively saw collaborative research mostly as facilitating the deconstruction of hierarchy. Only when confronted with the messy reality of fieldwork relationships and their inherent hierarchies, did I become more aware of the complex interweaving of overt and hidden power dynamics that greatly complicated this collaborative effort.

In reading this chapter, the reader should keep in mind that I am a young, white European woman, characteristics that deeply impact(ed) both my own feelings and reactions to the small Southeast Asian ex-colonial nation-state that Timor-Leste is today, as well as how people in the field perceived, approached, and treated me. More specifically, having both Portuguese and Dutch roots, I was often uncomfortable with my double colonial heritage on the island, a discomfort that my research participants did not in the least seem to share. On top of the status accorded to me as a white person, the fact that I was pursuing a doctoral degree while being under 30 years old (which is rather rare among Timorese) also conferred on me a higher status, which made me feel uncomfortable. Furthermore, I learned to speak the local language, Tetun, relatively fluently within a few months of beginning my fieldwork, an effort many foreigners are unwilling to make. This brought about a level of good will and awarded me a kind of exceptionality I did not feel I deserved, because learning the local language is mostly treated as a *sine qua non* of anthropological training and conducting fieldwork abroad. These factors placed me in an elevated hierarchical position, something that made me feel uncomfortable and further enhanced my wish for a democratized research practice.

Hence, I wanted to join the network of anthropologists who engage in activist and participatory forms of research. I wanted to cut through boundaries of otherness, foreignness, and hierarchy. I naively ignored the admonishments of other researchers (Lassiter 2005; Okwaro and Geissler 2015) about power imbalances in collaborative research, and I was convinced that my research design would automatically solve these issues for me.

This chapter is about “waking up to reality.” Based on diary entries, field notes and reflections written during the process of planning, implementing and finally dismantling a collaborative research group with students from the National University of Timor-Leste (UNTL) in Dili, I will discuss the methodological adjustments I made during the process and the intricately related affective dimensions of this attempt at reciprocity between researcher and research participants.

Excited and Expectant

In the early stages of my project, I planned to set up a research group with about four or five university students. As my research proposal, in applying for a doctoral grant, had to be very specific, I could not let the research group fully define its topic or scope, as a classic collaborative research approach would demand. Hence, the collaborative nature of the project was already limited. Personally, I was interested in researching inter-generational power dynamics in an emergent and post-conflict democracy⁵ and their consequences for how citizenship is interpreted by the post-conflict generation. Furthermore, because the divide in Timor-Leste between the capital city of Dili and the mostly rural remainder of the country is very pronounced, I wanted to further probe the role played by this urban and rural dichotomy in these processes. As the underlying theoretical framework of my research was citizenship, I decided that the research group could also utilize citizenship as an overarching theme. Yet, I would leave it to the individuals in the group to decide how exactly to engage with this topic.

I had chosen to work with students and young graduates as they were an important part of my research population; they also had some basic training in academic work and thus could engage in issues more analytically. Furthermore, some students had explicitly expressed the wish to do research with me. To bring together the research group, I tapped mostly into the contacts I had made during my previous research, i.e., students from the UNTL in Dili. My plan for the group spanned the months from April to December 2015 (the initially planned period of my fieldwork). The idea was to start with some intensive sessions concerning research methods, after which each participant would write a research proposal. Consequently, there would be a 3-month period in which each participant would conduct 1 month of fieldwork (the 3-month period was to allow for personal flexibility) at a fieldwork site of their choice. In September 2015, we would come together again as a group and start writing our individual papers and preparing a symposium (to take place in

⁵The island of Timor was split into two by Portuguese and Dutch colonizers. The eastern half (today Timor-Leste) remained Portuguese territory until 1975. In that year, after a unilateral declaration of independence, Indonesia violently invaded and integrated East Timor. Twenty-four years of occupation and fierce resistance came to an end when, in 1999, the UN hosted a referendum in which 78.5% of the population voted for independence. Timor-Leste became an independent nation-state in 2002 (CAVR 2005).

December) for university staff and students, Ministry of Education staff, and staff from NGOs working with citizenship education. During this symposium, the different papers would be presented. These were the ideas I brought with me when the wheels of my plane touched back down on Timorese soil.

I was very excited about the project. I hoped that the research group would provide a way for me to democratize my own research, giving a few research participants the chance to gain deeper insight into my research practice by letting them decide (to some extent) which direction the research would take, and allow me to put the knowledge I had gathered about research methods and research practice to work for others who wished to know more about it. Also, I was convinced that by observing the ways participants defined what was important and worth investigating in connection with the topic of citizenship, I could gain crucial insights into emic understandings of citizenship and how they were operationalized. Most of all, I thought that the research group would be an opportunity for me to “give something back” (Huschke 2015) to the country and the people I had grown so fond of. It would produce knowledge about Timor-Leste, *in* Timor-Leste and *by* Timorese. It would empower young Timorese academics to start contributing to the body of knowledge about their country, and it would give me the feeling that my research was not just opportunistically extracting knowledge to further my own career, but that it would actually leave something “useful” behind.

Inspired and Energized

The first meeting took place 3 weeks into my fieldwork with 11 young Timorese. Four participants (Vasco, Luis, Eurico, and Alfredo) were young men, and seven were young women (Ana, Helena, Aurora, Maria, Tereza, Rute, and Lucia). Ana, Tereza, Eurico, Rute, Lucia, and Maria had recently finished their bachelor degrees; Aurora, Luis, and Helena were in their last year; and Vasco and Alfredo still had some semesters to go. All were UNTL students or alumni. I was saddened that João, who had so vehemently urged me to come back and involve him in my research, could not participate, as he had just taken up an exceptionally demanding job. Other students who I had hoped would participate were now too occupied by their new jobs or studying abroad. Because all of the participants took part in an extracurricular course I offered on Saturday mornings, we agreed that the easiest approach would be to meet once a week on Saturday afternoons—the extracurricular course focused largely on research methods, and the Saturday morning sessions thus complemented our project and saved some time inside the research group. Participation in both sessions was voluntary and students did not receive university credits for their engagement.

The first meeting, which I had prepared, and during which I acted as the workshop facilitator, was very inspiring. I explained the ideas behind the project and expressed my hopes and expectations. We all wrote down and shared what we thought we could contribute to the project, what we hoped to achieve, and what our

fears were. We also established basic rules for the group through a brainstorm format. Then we split into small groups and wrote down ten concepts we associated with the word citizenship and discussed why we chose them. The results of these discussions were then presented and discussed in plenary form. We concluded the meeting by taking stock of the feelings which people took home. At the end of this first meeting, everybody was slightly euphoric and full of energy and commitment. The diary passage with which I prefaced this chapter illustrates my own state of mind after this first meeting (“I felt thankful...inspired...”).

After the project’s launch, I started making a detailed long-term plan for the weekly sessions, choosing some literature to read and discuss with the group and preparing the methods course for Saturday mornings. In the end, the full Saturday program (both the research group and the extracurricular course) took up a considerable portion of my time, and I felt a certain discomfort or unrest. Not yet collecting much data through conventional methods, I feared that I would start lagging behind with my research. The “emotion diary” proposed by the Researchers’ Affects project included one section that asked the ethnographer to describe herself in the field. I sometimes described myself in the third person singular to assume a detached perspective on my emotions, and the following diary entry illustrates my latent discomfort with engaging in non-conventional research methods:

She is still more the activist than an anthropologist. She writes relatively few field notes, has not planned any interviews yet and also has not yet invested in getting a discussion group together. Nevertheless she doesn’t worry too much. The interviews will come at some point. Everything in its own time. And so activism provides the perfect excuse for procrastination... (18 April 2015)

Although I often felt uncomfortable with the engaged research I was conducting taking up so much of my time in the field, I continued to be convinced of its benefits. However, my doubts as to what it means to conduct engaged research *successfully* grew over the course of the project, especially due to three central problems.

Dispirited and Disappointed

Although the decline of the research group was something very gradual, I will highlight three interrelated and concurrent elements that, in my view, were crucial to its eventual failure: (1) my own role and the utopian disregard for internal power differences; (2) a personal crisis that led me to almost quit my fieldwork; and (3) the conflicting commitments of the research group participants. During the earlier sessions I remained optimistic, although no one was as full of energy as they were in the first meeting. Lucia did not come back after the first meeting, as she had to take care of her sick mother in a rural village. Maria stopped coming after the second meeting because she was selected for a workshop in the Philippines. Although she shortly rejoined the group after coming back, she finally dropped out because she felt she was too far behind. The subsequent week, Rute announced she had been

accepted into vocational training and the training schedule conflicted with our meetings. What I most struggled with in the first weeks was not so much their absence; I had counted on losing some participants to other commitments along the way and had thus set out with more participants than I had initially intended for the project. Rather, what bothered me was my own role. I had envisioned a group in which I would not stand above the others, a group in which we could discuss different theoretical and empirical topics related to my research, a group in which people would not always agree with me, and a group in which new ideas would come up, the ideas I would not have had myself. The reality, however, was different, as my diary entry from this period suggests:

At our meeting's concluding round touching on 'the feelings I go home with,' I expressed that I am searching for my role within this group. I have the feeling I am still way too much a teacher, and that makes it difficult for me to see how we can really become a research group. On the other hand that is maybe also not very realistic because there is in the end quite a big difference in experience and knowledge (at least concerning research practice) and maybe they are not served by me trying to stay in the background all the time. I guess that they do need a certain degree of training and I can provide that. But as it was today, it does not yet feel good enough (...) Maybe I am also too impatient, maybe I should invest in a few sessions before I get anything content-wise back... (Diary entry 18 April 2015, Dili, Timor-Leste)

Although I repeatedly stressed that I wanted to do this *together* with them and that I wanted their input, it became clear that the principle of "everybody is the same," which we had included in our ground rules for the group, was utopian. I wanted it to be true, but it was not. This was for a few reasons: I was further down the road in my academic preparation; I came from an institution that taught (and expected of) me a much higher degree of independence and proactivity than their university did; I came with resources; I introduced the idea and brought them together; I was the one training them in research methods inside a university classroom; and, after all, I was a white western foreigner. Protracted colonialism and, to a similar extent, intensive post-colonial international state-building interventions led most of my interlocutors to view white foreigners as more knowledgeable than themselves. It often genuinely confused people when I asked them questions, and it often genuinely confused me when I was asked to tell my research participants what the political participation of young adults looks like in Timor-Leste, the thing I was actually trying to learn from them. It was only later, after leaving the field, that the absurdity of my self-created role also became clear to me. I became aware that my repeated stressing of a flat hierarchy explicitly neglected the power imbalance that existed within our group and that it was based more on wishful thinking than on the reality we faced.

Another example of my resistance towards the teacher–student dichotomy is that for some weeks I refrained from providing input about theoretical debates on citizenship, as I did not want to "influence" the participants' perspective on this issue. Instead, I wanted to be able to capture the *emic* understandings of the concept. Participants, however, at some point expressed their need for this input in order to get ideas and inspiration as to what to research. I ended up giving an overview of

how ideas of citizenship have developed in academia over the past decades and of the currently held definitions. I also gave them an English academic article about citizenship education in post-conflict situation written by Yuval-Davies (1999). I thought this article was quite easy to digest. As their feedback evinced the considerable difficulties they were encountering in reading this text, we devoted a whole Saturday afternoon session to it, which I thus summarized in my diary:

We went through the text bit by bit, sentence by sentence. Aurora had translated the abstract. Some had read nothing and a few, to my alarm, had translated the textbox with information about Taylor and Francis, the online publisher. I am so blind to these kind of things. The idea to tell them to start reading at page 2 under the official title doesn't even come up! We translated together. In every sentence there were words that I had to explain. After I referred a few times to metaphors, Ana asked me what a metaphor was. I explained about the Gulf War, I told them what the post-conflict situation in South Africa was (Helena was deeply shocked when I explained what Apartheid was, only Tereza had ever heard something about it). I explained that Bosnia was not in Africa and I explained what had happened there, etc. There were so many things I just glossed over, because for me they are normal, they are part of my common knowledge. Wrestling ourselves through the text, I slowly became aware of how difficult this was for them, even for the ones who are a bit further in their studies, the ones who I thought would be able to read the text by Yuval-Davis! But they loved it! Vasco, who actually had to leave at 2:30pm, in the end left at 3pm because he thought it was so interesting. We continued until 3:30pm. We were on page 3 by then. We decided to continue next week with something else because they preferred one of the other options I proposed. But they had learned a lot today, they said. (Diary entry 2 May 2015, Dili, Timor-Leste)

The session on the journal article sobered my view of the epistemological possibilities of this project. It made me realize that it would be unfair to expect from these students an academic paper at the level that I had unconsciously envisioned. I realized that I had conceived of a collaborative academic project within my own epistemological terrain, not theirs. I had expected participants to read academic texts in English (something they very rarely did at university), to write their own academic text (something they did just as rarely), and to engage critically with arguments, both mine and in texts (something they had barely ever been asked to do before).

While reading the article during this pivotal session, I realized I was asking too much. In wanting them to be equal research partners, I had neglected the fact that the reshuffling of curricula and official languages during their school careers, in the transition from occupation to independence, had set them at an educational disadvantage (ten Brinke 2013). This disadvantage has been further exacerbated by the severe underfunding of (tertiary) education in post-independence Timor-Leste. Hence, participants in the project repeatedly stated their wish for me to share my epistemology with them, to teach them the methods I used, and to teach them how to research and write about it. The realizations surrounding our diverging epistemological terrains, the participants' requests that I teach them about conducting research, and perhaps the wish to still be able to make something out of the project, prompted me to increasingly take up the role of the "teacher" within the project and to "call the shots." In retrospect, it seems this was the point at which my research

ceased being collaborative, if it had ever been (Lassiter 2005), and entered the more passive realm of engaged research (Low and Merry 2010).

The session described in the vignette above motivated me to adjust my methodology. I started preparing more content and making handouts that contained the information we discussed at every meeting. I began to prepare specific assignments about choosing a topic, defining research questions, thinking of appropriate research methods, and developing interview questions.⁶ Although this adjustment of expectations and methods yielded more concrete results in the participants' research proposals, it removed the element of collaboration from the project. It also coincided with other events and dynamics that all contributed to the downward spiral that would finally lead to the dismantling of the research group. These events and dynamics were not only related to the research group itself, but also to circumstances in my personal life.

It is reasonable to assume that the eventual failing of the research group was to a significant degree also related to a personal crisis that almost led me to discontinue my doctoral project. In short, I had to choose between continuing my research in Timor-Leste and giving up my research in order to return to Europe to save my personal relationship. Over the course of several weeks, it was unclear how long I would stay in the field, and I eventually cut short my fieldwork by 2 months. This lack of clarity about my ongoing presence in Timor-Leste had negative consequences for the research group, as planning became difficult and the outcomes became uncertain. Deciding to quit my PhD (a decision I later reversed), however, alerted me to the central role the research group played in my project:

There are often moments in which it hurts me to stop, to give this up. For example, when I sit together with the research group and I notice how deeply saddened they are when I announce that I will quit. And I am conscious that the research group was mostly why I was doing this [PhD], to try this out with this group, to write about this, to experiment with it. (Diary entry 27 April 2015, Dili, Timor-Leste)

Even throughout the weeks when I did not know whether I would stay in Timor-Leste or whether I would quit my fieldwork and fly back home to save my relationship, we continued having weekly meetings, both in the extracurricular course and in the research group. As the symposium became unrealistic, we decided that we would finish the preparation of the research proposals together, that the participants would still carry out their research and write about it, that I would provide my feedback via email, and that we would find a way to locally publish the results. It was a very difficult time for me in which I questioned many of the pillars around which I had built my existence: personally (a long-term relationship with my partner), professionally (working in academia), and ethically (doing anthropological research in collaborative and ethically sound ways). All of these seemed to become

⁶This meant, for example, that I explained the process of writing a literature review step by step, outlining the different kinds of sources they could use. It meant preparing matrixes in which they could fill in the texts they read, the sub-topics they found, and the pages on which they could find them. It meant I prepared the basic structure of an outline into which they could enter their topics in order to create their own research outline.

unattainable at the time, at least in combination. And even though I was committed to taking the work with the research group as far as I could, I was also absorbed by my own emotional distress. Undoubtedly, this diverted my attention, engagement, and energy from the research group project.

We often insufficiently acknowledge the profound ways in which our personal situations, relationships, and emotional distress impact our research, perhaps because this does not fit into conventional formats of academic writing. While much has been written on how the experience of fieldwork itself impacts on the researcher's emotional constitution (see for example Davies and Spencer 2010), the ways the emotions we bring into the field from "home" impact on our fieldwork have garnered significantly less attention. By systematically tracking our emotions during fieldwork, we can become more cognizant of the complex ways in which a number of aspects interact: positive and negative emotions; field and epistemic emotions; emotions arising in the field, and emotions brought into it. It can also help us analyze how this complex matrix in turn impacts our production of knowledge. Recognizing this dynamic is a first step, but in discussing it, we have to beware of falling into the navel-gazing psychologization of fieldwork practice. Instead, we must develop productive ways of communicating our emotions.

The problem here is that emotions (and especially negative emotions) are intensely personal and private. Hence, writing about them easily trespasses the line of what we can or want to communicate to a wider, anonymous readership. I do not have a formula for striking this difficult balance. Instead, what I attempt in this chapter is to delineate how the emotions connected to my relationship crisis interacted with my fieldwork, without compromising too much of my own privacy and that of my partner. To put it briefly, my relationship survived and so did my PhD. The research group, however, did not. I think, in part, this was due to the disappearance of a clear aim and time frame and my own waning enthusiasm. This might have led to the research group participants' (re)turning to other commitments, the third problem I want to address here.

Every week at least one of the participants had something else to do, and the group became progressively smaller. I reached the peak of my frustration one day, when only Aurora was present. After that session, I wrote the diary entry with which I preface this chapter ("What did I do wrong?!... Is it really that boring?!"). Most of the time, there was a reasonable cause for the participants' absence, as I also described in my diary:

'What feeling describes you best today?'⁷ Irritated, frustrated, disillusioned, sad (...) Ana wrote me a little note that she had work to do in the afternoon and would maybe (read: *not*) come back in the afternoon for the research group. Maria was ill and had to go to the hospital and would come later (read: *not*). Helena announced that she was tired and still had a report to write for her work and so she was going home. Then Vasco came to tell me that he was preparing a group assignment and that his fellow students had demanded him to be present there. Alfredo was ill and I haven't heard anything from Eurico in ages anyway.

⁷This was one of the questions in the structured "emotion diary" that I kept throughout my fieldwork for the Researchers' Affects project.

That meant that in the end it was just me and Aurora left in the research group. I could cry. And I think she could also see that. (Diary entry 13 June 2015, Dili, Timor-Leste)

The fact that fewer and fewer participants were committed to the project really affected me. I began to see their absence almost as a personal insult. At the same time, I felt that I was personally responsible for this problem, yet I did not know what to do about it. In retrospect, I see that this reaction was quite self-centered and unproductive. However, in the heat of the moment and immersed in the complex emotional landscape of fieldwork, I was confronted with a messy intermingling of expectations, disappointments, uncertainties, hopes, guilt, inspiration, and bewilderment that was, to me, impossible to see through. I repeatedly (and, increasingly, desperately) asked the participants of the research group how they felt about it and what we could do differently. They always assured me that they were very interested, but just really busy. I never managed to elicit a more comprehensive or critical appraisal of the project from them. In the next section, I will describe the process of adjustment that followed the collapse of the research group.

Accepting and Adjusting

With the number of participants dwindling and my own motivation at a new low, I came to accept that maybe my expectations had been too high and that it would be unfair not to adjust them. I came to accept that in the present situation, with an unclear perspective on how my fieldwork would continue, I could not expect the research group to deliver what I had hoped for. It was a difficult and painful process of letting go, but it was also liberating:

After [the morning program] it seemed like everybody was going to go away and that nobody felt like participating in the research group. For one moment I thought, 'Whatever, I do not care about this anymore,' but in the end I managed to convince four people to stay. I thought that in this way I could at least ask them what exactly the problem was. After lunch I quickly walked to the copy shop to print the handouts I had prepared and I thought to myself, 'You know what? I will make today the last session, I will make sure that everybody knows what to do for their research and then I will let go of it. If I end up staying here after all, we can still see what we do about it.' When I came back, Helena, Vasco and Alfredo were involved in a very personal conversation about boyfriends and girlfriends. I entered into the conversation and when Aurora arrived she did so too. And so we ended up having a very interesting conversation mostly about Helena's problems and we all agreed that she is accepting too much shit from her brothers. At some point I expressed my astonishment about the fact that in 2012 nobody had told me about these kinds of problems. Aurora told me it was because at the time I did not speak Tetun so fluently and also because it was at the end of my stay, when we started working together in the library, that we got to know each other better. But at that time, we were mostly very busy. Just hanging out together like we were now, we did not do that back then. And I guess she's right... Eventually I did put an end to our conversation and announced that for the time being, this was going to be the last session of our research group. To my great astonishment, they seemed genuinely surprised and saddened by this fact. Somehow, this brought me a comfort that was greater than I could have anticipated. We spoke about interview questions they had prepared. We spoke about the methods they were going to use and about their time schedule. Of course, they are not nearly as well prepared as I had hoped, but I don't think I will

get much more out of it. And so I think: Let them do their thing and we'll see how this goes on. At 3:30pm we said our goodbyes and I took a microlet⁸ to Lecidere. It felt good to let it all go. My anger had already vanished completely... I was thankful for the people who had been willing to stay and thankful for the fact that my Tetun was now good enough and that I knew the people well enough for them also to share their problems with me. Through the back window of the microlet I saw the figures of Helena and Vasco becoming smaller and smaller, and I felt a great affection towards those two little dots that ended up disappearing behind the bend of the road. (Diary entry 20 June 2015, Dili, Timor-Leste)

The research group cannot simply be considered a failure. The intensive Saturdays we spent together gave my relationship with some of the participants a depth that it would otherwise not have had. Our sessions made me aware of very important elements in my research and helped me better understand other research participants and processes. I became more attuned to how my research participants took responsibility (or not) for the course of their lives; the major role played by health and sickness in the lives of people; the role played by family conflicts in the lives of students; and, in some ways, even what it means to be a citizen. The sessions also provided me with new insights about my role as a researcher. They made me more aware of the intricacies of being a white researcher in a post-colonial context and the challenges this poses for doing participatory and collaborative research. They furthermore prompted me to agree with Lassiter (2005) on the crucial importance of communicating to our academic peers about the challenges and possibilities inherent in collaborative research, in order to further scrutinize its possibilities.

Parallel to the collapse of the research group, I started working with methods that were less collaborative and participatory but more successful in generating data. Solicited by a group of students who very persistently came and asked me to teach them about leadership, I started giving workshops that I later expanded to other groups of interested students (from both the National University and a private university also situated in Dili). I gave a maximum of three to four workshops to a group in order to prevent a process of dissolution similar to that of the research group. I worked with methods I had developed myself based on participatory action research: working in small groups (with posters, post-its, visualizations, etc.) and presenting in the plenary. The topics addressed in the workshops included youths' positions in local hierarchies, the problems youths face in Timor-Leste and the ways these could be addressed by the youths themselves, and the rights and duties youths felt they had at different levels in their communities. From my perspective, these constituted elicitation sessions into which I could integrate my own research questions. To the participants, they constituted training in group work and public speaking, which made collective knowledge visible and solutions feel more attainable. It was not nearly as collaborative and participatory an approach as I had wished for, but participants enjoyed the workshops and took away more from them than they would have from a regular focus group discussion or interview. So did I.

⁸Public transport minivan.

My “letting go” of the research group project was quite absolute. I had decided that I would not force field research onto any of the participants, because that would further contradict the collaborative nature of the project. So I waited to see whether participants seriously wanted to carry out research. I inquired a few times about it, and after most participants expressed they were too busy with other commitments, I did not insist, my disappointment notwithstanding. When I left in October 2015, I could not help but feel a sense of loss—loss of a dream, loss of a certain degree of faith in collaborative research, but most of all, a sense of a missed opportunity. I had learned a lot, I had developed new methods, I had gotten one tiny step closer to understanding the lives of the people I cared for so much. But I had not accomplished what had mattered most to me: the establishment of a collaborative research group. I felt that I had adapted the project to its dissolution. However, I did not really know what I should or could have done differently.

Concluding Remarks

After some time and consideration from a distance—revisiting my “emotion diary” and trying to make sense of the wide array of affective dispositions described in it, and analyzing the process from the comfortable position of an office desk far away from the field—it now seems a little clearer to me what I should or could have done differently.

First, pretending that power imbalances do not exist does not make them disappear. Rather, it discourages us from openly and honestly talking about them, thereby making it impossible “to find ways of speaking and acting about inequalities” (Okwaro and Geissler 2015, p. 507). In my case, my unease with the hierarchical position of a “teacher” that I was ascribed and that I created myself contributed to feelings of guilt, indebtedness, responsibility, and ultimately disappointment toward myself. Had I opened up the conversation and addressed the “elephant in the room” instead of insisting that we were “all the same,” I might have created a new space for participation, or at least a fairer collaboration. Additionally, in collaborative projects, we should be aware of the complications that arise in a post-colonial research context where ideas of superiority and inferiority, domination, power, and inequality linger, sometimes explicitly but mostly under the surface. However, avoiding doing participatory research away from “home” (Huschke 2015) in my view is also not a solution to long-standing global inequalities. Active and reflexive engagement seems to me a more constructive way of going about it. Within a collaborative effort, where different epistemologies meet, it is crucial for both the researcher and the research participants to be aware of their own epistemological frameworks and the inevitable potential for ethnocentrism in order to be able to communicate effectively. Avoiding this in the hope of rendering it unimportant creates frustration, sustains power imbalances, and most probably results in disappointment for the people involved.

Second, engagement and collaboration make little sense if they are built purely on the researcher's wish to soothe her own consciousness by "giving back." In my present view, collaborative research succeeds only when actively solicited by the research participants themselves. Although I had been asked explicitly to run a participatory research project by my research participants during my previous research, I actually commenced the project two and a half years after it had been requested, when most of those who had asked for it had finished studying and were taking up demanding jobs. Consequently, I offered a collaborative project to people who had never actively asked for one, and subsequently I was disappointed when I did not see them taking ownership of a "collaborative" project that I had more or less single-handedly created.

Third, the relationships that are established within engaged research have strongly affective dimensions. The expectations one has of these relationships play an important role in emotional dynamics such as excitement and exhilaration, but also frustration, disappointment, or even anger. Once I came to feel personally hurt by people's disengagement from the project, the alarm bells should have rung. Had I at that point, or preferably even earlier, more actively reflected upon and analyzed what these emotions meant and where they came from, I might have been more aware of the contradictory position I was taking up inside the research group, and I might have been able to more successfully adapt.

Finally, and related to the preceding three points, in hindsight, there are actually interesting parallels between how the personal lives of the research participants and my own impacted the common research project. While their academic system constrained them by putting them at an educational disadvantage (changing educational paradigms and underfunding), my academic system constrained my liberty in doing truly collaborative research (through strict PhD funding regulations). While they had other commitments in the form of university courses or jobs (or both), I had to invest my time primarily in the more conventional research activities that were expected from me. And finally, while they were often constrained by family relations (taking care of sick relatives, being involved in family disputes, having to conform to household duties), I was involved in an emotional relationship crisis that constrained my commitment and availability to the project. Hence, while we were all, in our own ways, navigating the complex terrains of intersection between our private and public lives, I considered my own constraints to be challenges, while I interpreted theirs as signs of declining commitment or enthusiasm. This reveals the importance of open and honest communication within collaborative projects, and especially the importance of communicating on truly equal terms. It also puts emphasis once again on the need for the researcher to reflect not only on their own position but also on the position of the people they work with. Analyzing our own emotional reactions, due to their deeply social nature, may aid us in this endeavor.

In this complex field of power, guilt, excitement, disappointment, cross-cultural encounter, inspiration, disagreement, and commitment, it would only seem logical to keep track of the emotions that, on the one hand, are created in this field of tension, and, on the other hand, are created elsewhere but profoundly impact it. Surprisingly, this is something we hardly ever do. If we want to develop more recip-

rocal and collaborative research, we need honesty, communication, and reflection. This, in turn, requires a frank and systematic documentation and analysis of the affective dimensions of our research efforts. While I documented my emotions systematically in the field, I only analyzed them after coming back. I would argue that developing a methodology of reflection on the affective dimensions of (collaborative) research while submerged in it is still a field open for further exploration. With this chapter, I aimed to take a first step towards a more reflexive approach by showing how my own dreams, expectations, and ideals were put to the test in the field, as well as by dwelling on the methodological and emotional adjustments that I made across my various encounters in the field. In doing so, I have endeavored to contribute to the reflection about and further development of an array of methods in whose potential I still firmly believe.

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Part VI
Unpacking Emotion Regimes in Teaching
and Fieldwork

Unpacking Emotion Regimes in Teaching and Fieldwork: Introduction



Kelvin E. Y. Low and Noorman Abdullah

Introduction

Do emotions matter? Is there an “anti-emotional culture” (Okely, this volume) in research, writing, teaching, and other academic regimes of professional conduct? Is the academic lack of attention to vulnerability an outcome of how these expressions have been posed as a nonproblem in pedagogy and fieldwork? What can we do as fieldworkers to stimulate a more emotionally aware academic and research milieu? Under the influence of institutional emotion regimes that underscore observer detachment and neutrality in fieldwork, emotions have been downplayed, if not disassociated, with expected modes of professional academic conduct. These regimes continue to place a premium on validity, objectivity, and credibility.

The three authors in this section problematize such hegemonic positions that are often sustained in various domains of academia. In their writings, they recognize and reflect upon the centrality of emotions and their associated links to identity, fieldwork conduct, and sociality. In different yet complementary ways, all three contributors consider the role, impact, and valence of “emotional labor” (Hochschild 1983; Lutz 2017; McQueeney and Lavelle 2017) and how this notion can be built into fieldwork and beyond, in pedagogical and other settings in the social sciences. More crucially, research, writing, and teaching methods need to critically engage with emotions and reinsert them into academic emotion regimes as part of the project of developing meaningful scholarship and pedagogy. Scholars who assume different and interconnected roles as researchers, instructors, and academic faculty members must both perform and carefully reflect on the emotional labor that is continually negotiated across a variety of scholarly domains and in relation to different interlocutors within and beyond the field. In this respect, deliberating on these

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issues unveils and acknowledges the epistemological value of emotions (Stodulka et al. 2019) in academia and other social domains.

A range of emotions abound in our profession, both as researchers in the field (Stodulka 2015) and as instructors in the classroom. The question is what we do with them, and how we navigate them. Instead of being displaced in the academe, the three contributors encourage such emotions to be reflexively incorporated in research and pedagogy in variegated ways. This is done to reconcile the analytical weight and sociocultural importance of emotions with what we do in the different spheres of intellectual engagement that we address below. By exercising such reflexivity, the authors here emphasize the pertinence of emotional labor as a resource in generating further analytical insights on researcher positionality, pedagogical training and institutional conduct.

Noorman Abdullah (this volume) calls for researchers to be “emotionally aware” in presenting their ethnographic analyses. Such awareness runs a gamut spanning vulnerability, emotional danger, discomfort, remorse, surprise, fear, and emotional stability. This nonexhaustive spectrum of emotions ought to form a part of the research epistemology of social science endeavors. Managing one’s emotions, as Strauss (this volume) and Abdullah point out, are part of the process of generating as well as analyzing data with the aim of knowledge production. In both chapters, the authors oscillate between emotional discomfort and awareness in the course of conceiving and conducting their respective research projects. Strauss is keenly aware of the gender divide in her research context and how that led to moments of discomfort. For Abdullah, episodes of studying spirit possession opened up emotional subjectivities between him and his respondents which required careful introspection and management of field relations. He also discusses the latent emotional impact that research can carry beyond the field and into the social circles of the researcher. Furthermore, he notes the expectations surrounding male researchers, who are supposed to be emotionally absent if not stoic in their roles as fieldworkers. Gender positionality is both divided and ascribed between the two sexes. In these various contexts, how do researchers maneuver between their emotional states and the extent to which they continue to participate in fieldwork and sociality? How far does one engage with emotional moments in the course of gathering data? What types of self-care strategies (DeLuca and Maddox 2016) do researchers have at their disposal when confronting these moments, so that emotional experiences and labor are not truncated and designated as individual failings on the part of the anthropologist (McQueeney and Lavelle 2017)? Emotional life, after all, is part and parcel of social life and fieldwork itself is a complex emotional undertaking (Lutz 2017). If anything, what is produced alongside the researcher’s range of emotions—fear, anxiety, distress, and vulnerability—is an empathy that lends nuance to our understanding of how others’ emotional lives are both studied and conveyed responsibly (McQueeney and Lavelle 2017).

Fieldwork encounters, as a result, became more dialogic, vicariously felt, and deeply understood as a consequence of our paying closer and more careful attention to emotions. These are the core arguments of the three chapters in this section, demonstrating analytical relevance and resonance across the different emotion regimes

prevailing in pedagogy, research, and academic institutions. Emotions do matter, so long as emotional involvement is carried out in careful concordance and calibration with the exercise of field reflexivity. Data generation and analysis can be further enriched by making the ethnographer and her emotional state of being visible, including feelings of discomfort (DeLuca and Maddox 2016). In sum, these chapters invite a rethinking of emotion regimes that have traditionally tempered parameters of objectivity and detached researcher positioning. Emotional epistemologies matter as they are variable, contextually determined, and impactful.

If emotions matter for fieldwork and the process of research and writing, then how does one harness emotional reflexivity in the classroom and its related pedagogical contexts? In Annika Strauss's attempt to accomplish this, she raises questions of embodiment, research ethics, emotions and self-reflexivity, emphasizing the salience of students' own emotions and subjectivities in the course of fieldwork as well as their post-fieldwork reflexivity. Strauss recruited students into a research project with refugees in Germany, where the former took on the role of volunteers; part of their fieldwork training included taking down notes which were later openly reflected upon. She emphasizes that engaging with such reflections among students required an empathetic approach, especially in order to unpack the sociopolitical aspects of classroom emotions, as well as to carve out a conducive learning environment. In the end, what was imparted to her students is the fact that fieldwork is most transformative when their embodied experiences can be taken into serious consideration. Further attempts are also made to sharpen their sensory and emotional cognizance, in order to connect their volunteer experience to fieldwork analysis more critically and productively. In a similar vein, Abdullah and Okely also recommend a closer look at emotional dispositions and management in teaching contexts and academic relationships, such as those between advisors and students, or among colleagues on a faculty. In order to carry out and fruitfully discuss "emotionally aware ethnography" (Abdullah), the emotional complexities and challenges researchers and teachers experience can be worked out across these different sets of social relations before, in the course of, and after fieldwork. As informal mentoring sessions, they respond to the stoic and positivist expectations of emotion regimes. These endeavors also counter-critically address how emotions, vulnerability, frustration and uncertainty are experienced and confronted in different measures. Recognizing the place of emotional labor, and exercising emotional reflexivity, can be both empowering and illuminating as it provides opportunities and possibilities for strengthening our analytical approaches, as well as complementing our teaching endeavors and helping to resolve the emotional experiences and reactions of both students and teachers. Thus, emotions should form an important part of fieldwork training and reflection.

In addition to these emotive dimensions, Strauss also points out the importance of embodied training for students. Drawing links between the body, the senses, and social status, Strauss engages her students in various embodied exercises comprising multiple calibrations of their walking styles, bodily postures and facial expressions. In so doing, she opens up discussions among her students on whether such embodied behaviors are similarly observed either among their informants or

themselves, and how such behavior resonates with their fieldwork experiences. Further connections are then established between such exercises and the theoretical and methodological literature. Judith Okely, in retrospect, recounts a spectrum of situations in which emotions and subjectivity have been dismissed as irrelevant or private over a range of institutional contexts. These also include authorship and writing style, doctoral supervision, and acts of reciprocity. Beginning with a critique of the Cartesian dualism as disembodied and how the dominant history of the social sciences is one of positivism, Okely makes a case for how the personal is enmeshed with the political. She suggests to researchers the imperative of departing from detachment and disengagement, proposing that “the fieldworker’s experiences stimulate emergent theories.” Emotions are no longer relegated to the private sphere of one’s experience, be it in the classroom, in the field, or in various interactional settings. Instead, they possess pertinent epistemological value. Emotions are reflected upon at the different levels of the self, the interactional, and the institutional. As an example, Okely draws on her own experience at a conference, where the debate centers on the question of whether individual presence is a complement or an obstacle to the scientific objectivity of fieldwork and analysis. The employment of the first person in academic writing is also deliberated upon. The fieldworker’s position and specificity, including her emotions, are subsequently re-inserted into Okely’s analysis. At the same time, she identifies what she calls “emotional drives,” including nostalgia, childhood experiences, and sensory impressions. These refer to how fieldworkers’ varied and individualized connections to the past organize and frame how research is approached.

Drawing upon one’s emotional biography is likewise an important theme for all three authors. They describe how such linkages to the past color and influence fieldwork encounters in the present. Such encounters include being positioned as insider or outsider, or being positioned vis-à-vis discomforting social realities in the field. Other examples involve emotional control or emotional outburst in situations in which researchers constantly have to calibrate their emotional conduct in the process of gathering data and maintaining researcher-respondent relations. Abdullah’s personal biography of experiencing death in the family due to spirit infliction became an impetus for his own research many years later. Such research is entangled with embodied experiences that are emotionally charged, challenging, as well as comprising vulnerable episodes in the course of fieldwork and in the social worlds which we inhabit and study. In Strauss’s case, she was aware of her position as a Western woman in the refugee camp, and understood that this slice of her identity, intersecting with being a female researcher, was foregrounded as a point of curiosity in the field. Paying attention to our own emotional and embodied biography is therefore a way to be sensitized to how such reflexivity adds to a richer comprehension not only of the people whom we study, but also of ourselves and our analyses (McQueeney and Lavelle 2017). The self is embodied, sensorial, positioned, intersubjective, and political. Efforts to carry out in-depth and honest ethnographic work can only be truly successful when one engages with the self, and when one acknowledges and constantly re-negotiates the multiple positionalities that are

taken up, for information is ‘always mediated through the self’ (DeLuca and Maddox 2016, p. 286).

Our emotional capacities—complex and multi-dimensional means of human communication—are the very foundation that makes social relations possible in the first place. In his *Grief and a Headhunter’s Rage* (Rosaldo 1989), Renato Rosaldo writes that he only came to appreciate the meaning of rage that emerged from grief, loss, and bereavement characterizing Ilongot headhunting after the sudden death of his wife and fellow anthropologist Michelle Zimbalist Rosaldo, while both of them were in the field. In his candid and intellectually stimulating account, he recognizes the cultural and communicative force of emotions in fieldwork. This emphasis on emotions also reignited the methodological imperative to incorporate our emotional experiences as fieldworkers into the meaningful interpretation and writing of ‘our others.’

Emotions matter because they are resonant throughout and beyond the research and writing processes, as well as across a range of other academically related practices, interactions, and sociopolitical intersections. More crucially, they matter because they reflect and stem from individual behavior, the social, political, and economic, in a whole host of everyday life activities and tensions.

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Vulnerability in the Field: Emotions, Experiences, and Encounters with Ghosts and Spirits



Noorman Abdullah

My interest in studying ghosts and spirits is borne out of my own emotional memories and experiences of loss as I was growing up. My maternal great-grandmother, who migrated to Singapore from Surabaya, Indonesia in the early 1920s, passed away from what was perceived by many among our extended kin to be the outcome of a malevolent spirit affliction. I was barely 6 years old then. Petrified and baffled by the spate of events and emotions that ensued before her demise, I attempted to banish these memories away. While this was successful as the years passed by, her image and the circumstances of her passing continue to linger on. This incident offers a meaningful and profound level of reflection, what I term a “critical moment,” which gradually piqued my intellectual curiosity to better understand this phenomenon. At the same time, I braced myself for what were to be emotionally difficult and vulnerable experiences.

The field of studying ghosts, spirits, and other supernatural entities is filled with numerous emotional encounters: pain, sorrow, feelings of discomfort and fear, uncertainty, and vulnerability. As I engaged with different social actors in these settings afflicted by supernatural entities, I often found myself lodged into unsettling encounters. Increased emphasis has been assigned to the emotional dimensions of research, particularly from feminist ethnographies that have facilitated a deeper understanding of reflexive modes of knowledge (Behar and Gordon 1995; Golde 1986; Wolf 1996). With some important exceptions (Blanes 2006; Favret-Saada 2012; Goulet and

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Miller 2007; Zablocki 2001), most scholars who have studied the supernatural have not taken a more explicitly reflexive, embodied and “emotionally-aware” methodological approach in presenting their ethnographic findings. Lee (1987) further observes that many scholars reject personal accounts of the paranormal in view of its ostensible failure to conform to the regimes of scientific validity, detachment, and credibility that ground research. There is, however, much to gain from what has been variously described as “experience-near” ethnography (Wikan 1991) or an anthropology of “extraordinary experience” (Goulet and Miller 2007). Affective experience, introspection, and emotional dialogue in research comprise complex and multifaceted intersections that concern the vulnerability of researchers on the one hand, and the “others” ethnographers study on the other. The concept of “vulnerability” is further elaborated in the next section.

The arguments of this chapter are threefold. First, this chapter problematizes the relatively neglected issue of “vulnerability” in ethnographic research and for the ethnographer, with particular emphasis on research on ghosts and spirits. Kuiper (this volume) who studies *uchawi* (or witchcraft) in Tanzania similarly argues that “anthropologists need to have the audacity to be vulnerable, in the field as well as in writing.” I present select ethnographic experiences and “critical moments” of spirit possession among Malay-Muslim families in Singapore and Malaysia. This is done to exemplify different vulnerable encounters in the field that ethnographers may have to confront, but rarely address methodologically or in writing, given the emotion regimes of “objectivity” and “emotional stoicism” that prevail in academia. In the process of “researching the researcher” (Campbell 2002) and “writing about the self” (Leibing and McLean 2007), I draw on my own biographically grounded, embodied, and emotional experiences as a means of analytically discussing vulnerability in ethnographic research. Rather than positioning such experiences and reflections of and from the self as a methodological limitation or as antithetical to reason, engaging in experiences that draw on personal commitments does not preclude analytical deliberations in ethnographic writing. Such self-examination, which situates and interrogates the self within the research process and its written product, therefore constitutes a “crucial part of the research setting” (Diphooorn 2013, p. 206). The personal is always “hybrid in character, in that it blends and combines an individual’s personal story with his or her scholarly story” (Burnier 2006, p. 412) and “has never been subordinate in the private world of fieldnotes” (Atkinson et al. 2003, p. 60), or “completely at home” (Danahay-Reed 1997, p. 4).

Second, through the lens of researching ghosts and spirits, I show how these emotional experiences impinge on social relations both within and beyond the field. Ethnographic research is saturated with relations that are not necessarily always documented and referenced in our ethnographies and intellectual oeuvres (Grindal and Salamone 2006; Handler 2004). How do ethnographers competently negotiate and manage the tensions, conflict, and vulnerability that surface in the course of fieldwork, and their effects on both professional and private lives? Could it also possibly be that feelings of vulnerability in academic training and development have been given little attention in the methodological literature because these have been postured as nonproblems? Was the apprehension experienced during encounters in the field with ghosts and

spirits unfounded and thus “irrational”? In this manner, ethnographers are entwined in meaningful and often emotionally intense relationships that frequently spill over the boundary between us and our respondents in the field. Those who welcome ethnographers into their social worlds—respondents, friends, families, and extended kin—do so personally, affectingly, and emotionally, which in turn involve reciprocal loyalties, duties, and obligations. Ethnographers are all too often regarded as the prototypical individual “self” or “person,” or, at best, positioned within academia, disregarding our commitment to the community of kin that we as ethnographers belong to. The dominant emphasis in the ethnographic canon is rather on the immersion among and relations with the informants and interlocutors whom we are studying.

Straddled between what Keane terms “an epistemology of intimacy and estrangement” (Keane 2005, p. 62), I conclude by proposing practical methodological initiatives, both at the institutional and informal levels, which could be potentially undertaken to manage vulnerability in the field and address dominant emotion regimes in academic settings. Through this exercise in working through experiences of vulnerability and other critical moments, the broad aim of this chapter is to help transform ethnographic field research and relationships, to help foster a greater commitment to emotional awareness in ethnography and in our relations within and beyond the field.

Making Sense of Vulnerability

To be sure, the concept of “vulnerability” I use here does not make reference to blanket policy approaches that do not adequately account for “their own life-world, capacities, and strategies of the people as actors, nor on the structural and institutional context and dynamics of their position in society and economy” (Nageeb 2008, p. 245; Lachenmann 1999). Following Behar (1996), “vulnerability” seen through the lens of the supernatural moves beyond the simplified “victimization” of social actors and recognizes instead their potential agency and capacity. Experiences of vulnerability can be both debilitating and meaningful lived experiences for ethnographers and respondents alike. Therefore, embracing and reflecting on the relational and emotive dimensions of vulnerability is not a “problem” which needs to be resolved through recourse to “objective,” “scientific” knowledge but rather through meaningful engagement with and reflection upon such “critical moments.” This enables ethnographers to revisit and redraw the boundaries of research and contemporary ethnographic field methods.

Though this attention to vulnerability is gradually evolving, much of the extant literature continues to reinforce dominant emotion regimes.¹ Ethnographers have

¹These include the emotional conventions that hail the ostensibly fearless status of ethnographers (Fincham 2006; Vail 2001). Often, they privilege the rights and safety of respondents over ethnographers’ (Rosenbaum and Langhinrichsen-Rohling 2006). In many research cultures, ethnographers are assumed to be able and in many ways expected to “rough it out” in the field. These experiences are proudly worn as a badge of academic resilience and hardiness.

addressed “vulnerability” when framed in terms of immediate physical threats to their personal safety (Gwiasda et al. 1997; Williams et al. 1992). With the exception of a few cases, field research is often described as a generally safe endeavor, with “common sense,” or what some term as a “sixth sense,” believed to be all that ethnographers require when entering field settings (Renzetti and Lee 1993; Williams et al. 1992).

While it is important to acknowledge this dimension, vulnerability needs to be re-conceptualized to encompass other forms, which can include physical danger, but also emotional, ethical, and professional dimensions. Lee-Treweek and Linkogle’s (2000) work, *Danger in the Field*, is instructive here. Using these four categories of danger, which are not necessarily mutually distinct but interconnected, they argue that although danger in the field is a distressing experience for ethnographers, it does to a large extent enrich understandings of the field site (Lee-Treweek and Linkogle 2000, p. 2). Of immediate interest is their discussion of the issue of emotional danger, by which they mean a serious impediment to the ethnographer’s emotional stability brought about by negative emotional states induced by the research process (ibid., p. 4). I extend this argument by referring to vulnerability not only as emotional discomfort, but also as distress and feelings of dissonance which can further affect our familial and other personal relationships beyond the ethnographic field site. Researcher vulnerabilities related to isolation, fear, loneliness, and despair in the field can also affect our professional connections with colleagues at work. Paying attention to and reflecting on researcher vulnerabilities enables us to challenge perceptions of ourselves and our relations to others within and beyond field research.

Experiences and Encounters with Ghosts and Spirits

The emotional bearing of undertaking ethnographic research on ghosts and spirits is potentially a crucial source of further insight in re-conceptualizing the vulnerability of the ethnographer. In fact, the discomfort of witnessing tension, violence, illness, or even death as an outcome of perceived spirit affliction and encounters with other supernatural entities needs to be situated in terms of our own involvement in the actions of others in the field. Following Kelvin Low (2005, 2009) and Paul Stoller (1997), ethnographic research is an emotionally embodied and sensuous encounter.

With some exceptions (Birckhead 2004; Blanes 2006; Palmer 2001; Zablocki 2001), methodological quandaries concerning the emotional vulnerability faced by ethnographers studying religious and spiritual experiences in relation to spirit possession, and their exclusion from dominant academic regimes have yet to be given serious attention. Palmer (2001), for instance, problematizes the extent to which researchers should be open to spiritual beliefs and practices. She explores several of her own experiences in which she has been personally affected—both spiritually

and emotionally—by her involvement in religious movement rituals and her encounters with their charismatic leaders. Zablocki similarly recounts a personal journey which made him realize the considerable power of religion in his life. As he puts it, “religion has always terrified me especially when the spirit is moving and has us in its thrall” (Zablocki 2001, p. 227). Here, he reports on his childhood mystical experiences that demonstrated “how easy it would be to slip away entirely from the socially constructed world that parents and teachers called “reality.”” However, the privileging of rituals in studies of the supernatural, often typified as “exciting” and “exotic” in the eyes of ethnographers, have instead steered attention away from these research processes. These involve not only our personalized, emotional experiences of vulnerability when studying such issues, but also our social relations beyond the field site. Through my field notes, diaries, and other modes of data recording, I introduce in this chapter several “critical moments” of ethnographic detail in my study of ghosts, spirits, and possession. These visceral and emotional moments of ethnographic discomfort can be epistemologically informative and are sources of potential insight (Davies 2010; Hume and Mulcock 2004) that challenge the traditional legitimacy and authority ascribed to “objectivity” and “Western rationality.”

In spite of the fact that I conducted research in other vulnerable contexts in 2005 (Abdullah 2005), I found that these situations involving ghosts and spirits have been difficult for me to confront. My interest was in scrutinizing the manner in which Muslim practitioners in Singapore and Malaysia perceive misfortune and debilitation to be ascribed to supernatural agency, and the resulting pursuit of relief from such distress. I aimed to interrogate how such social actors meaningfully managed and negotiated their identities as “good Muslims” during episodes of spirit affliction in their everyday life.

Throughout the research process, I relied upon different methods to gather data concerning the sociality of spirit possession amongst Malay-Muslims: ethnographic fieldwork, informal conversations, in-depth narrative interviews, popular and theological secondary literature, as well as historical archival data that included colonial annual reports and religious documents such as Islamic religious Islamic sermons, texts, and pamphlets/booklets. Other resources included newspapers from both English and Malay dailies in Singapore and Malaysia, ministerial speeches, and government printed material. The heart of this study was nevertheless primary ethnographic data gathered from fieldwork both in highly urbanized Singapore and parts of Malaysia. Participant observation from case studies and narrative interviews spanned a period of 13 months.

My peers and respondents often perceived the environment in which I conducted participant observation as not necessarily “safe.” Neither did I feel entirely at ease as a result of such impressions. This is illustrated in one of the entries in my field diary²:

²Field notes were written on pen and paper in my field diaries. The diaries captured largely emotional and personal experiences, but also included methodological, analytical, and descriptive notes.

It did not help that many people in my personal and professional life, my family and peers especially, habitually advised me to take care of myself before I embarked on my participant observation: ‘Be careful not to get too involved;’ ‘Make sure you don’t trust what they say;’ ‘Try not to believe everything they tell you;’ ‘These are dangerous places, so be careful.’ The inventory went on and on. I found this particularly irksome at first, but then when I observed my first healing ritual and heard Nurul [a respondent] screaming, I saw myself questioning my own beliefs of the uncanny, which was really disconcerting since I always avoided broaching the issue. Now the fear suddenly surged through my body in full force. (Field diary, n.d.)

On the one hand, I found what some peers conveyed about my respondents to be “problematic”; professionally, these are ascriptions that I wanted to critically unpack. On the other hand, there were moments when such thoughts surfaced in my mind, especially when I was confronted with situations in which I felt potentially vulnerable. These emotional experiences allowed me to engage in data generation and interpretation accordingly, particularly in relation to what constituted “data,” that is, how far I was willing to push to emotionally participate in fieldwork and the framing of questions toward my respondents. At the same time, such experiences became a burden to me, because inasmuch as I attempted to brush them aside, the more thoughts about spirit mediums and members perceived to be afflicted by spirits surfaced.

I incessantly asked myself: Would I really get ‘hurt?’ Are they as nice as they looked? Would they expect me to participate in certain rituals which I did not want to engage in, but that would be important in obtaining data? ... Would the food and drink offered by spirit mediums and other members in the families I observed be ‘safe’ to consume, given that food and drink are construed to be potential carriers of magical spells and incantations? Would my rejection of such food then reflect poorly on my position as a researcher? Not only that, but would it also portray me as ill-mannered for not following the norms of reciprocity and respect in consuming your hosts’ food as expected in the field here, and for not repaying their willingness to talk to me, which could then affect the data I collected? (Field diary, n.d.)

My experiences paralleled anthropologist Tessa Diphoom’s own reflexive discussion of her “emotionality of participation” and management of risk in her work on violence and security agencies in South Africa.

I increasingly asked myself why I had selected this topic. Am I a researcher who wants to ‘seek out danger’ and who thrives on the ‘business of thrill seeking?’ (Diphoom 2013, p. 213)

The kind of emotional vulnerability I experienced was difficult for me to describe to others. There were certain episodes during my research where I wavered about venturing further in the field site, especially in its earlier phases. I often assumed that these experiences would subside when I became more at ease in the field. However, there were always periods in the field when I suddenly felt fearful. These “critical moments” that captured this vulnerability made me reflect upon my emotional engagement in the field, and how this engagement affected ethnographic data construction:

I finally managed to secure an interview with a *bomoh* (spirit intermediary), Nek Siyah, who was introduced to me by Delia [one of my relations]. I felt elated! Though I knew by now that I did not want to focus my study on rituals of spirit interference, this was extremely exciting news, since it has been months before a *bomoh* was willing to talk to me about their work, since I was told by most of my respondents that they were often known to be carriers of secret, spiritual knowledge. I nevertheless decided just to go into the interview process with an open mind and heart. When I entered into Nek Siyah's home, I was greeted by an elderly woman who was wearing a simple, nondescript *baju kurung*³ and a *tudung serkup*.⁴ She appeared robust and strong, even though she claimed she was well over seventy years of age and a great-grandmother. She was generally forthcoming about her work and what she did—asserting that she was a “good Muslim” and always insisting she carried out the work of God, but did not go into much detail when I asked further. While the conversation I had with her was extremely engaging, it was often marked by moments of silent uneasiness and discomfiting stares. What struck me as extremely disquieting, however, was at the end of my interview, where she looked at me intensely, clasped my arm, and with a very low and deep voice, said:

- Nek Siyah: There are some things that I shouldn't have told you, *cu*.⁵ But right now, since you are studying this [issue], you better be careful now where you go and walk...not under big trees, or be careful of the food that you eat at some places, ok? Don't offend anyone...What I tell you is private. You know if you let any of this known, you'll know what can happen to you or your family.'
- Author: Eh, what do you mean by that, *Nek*?⁶
- Nek Siyah: (laughs) Why are you afraid, *cu*? You are a good person. You don't have to worry. God is always here to protect you.

This felt extremely uncomfortable and unnerving, even though she laughed at me at the end and claimed that this was uttered only in jest, exclaiming very candidly that she had known this was part of my research and was 'aware' of my fear, even though we had never communicated prior to this meeting! Her later claim was that my close involvement with the 'victim' of a spiritual incursion she was attempting to heal, and the close fictive kinship ties that emerged between the household and me, opened up the possibility of malevolent spirits harming me. Nonetheless, I thanked her for the interview, and walked away, always wary thereafter that 'something' was following me home from behind, especially since it was quite late in the night. I increasingly became paranoid. Was this really happening to me? Could something bad happen to me? I was supposed to talk to her again one of the days next week to talk about her life further. But should I really? I knew I was afraid, I could sense it in my heart, and I felt vulnerable, but my professional side wanted to see through the next interview. Though in this instance I did return to talk to her, since this was part of my professional training, I cannot deny that I was fearful and extremely uneasy during my next meeting with Nek Siyah. (Fieldnotes; Interview, n.d.)

In addition, I often had vivid and lucid dreams, in which I, or one of my family members, was afflicted by a supernatural entity. I felt distressed, vulnerable and emotionally sapped. I also felt that it was difficult for me to continue to carry out research, though I was perennially advised by my respondents during periods of my

³A loose-fitting two-piece long-sleeved dress normally worn by Malay women.

⁴An elastic cap-like bonnet to cover the hair of Muslim women popular in Singapore.

⁵An abbreviation for *cucu* (Malay for grandchild).

⁶Malay for grandmother, or a term of address given to elderly women.

observation to take precautions. Such experiences mirrored other ethnographers who had dreams, illuminations, visions and ineffable, “extraordinary” field encounters (Diphoom 2013; Wilkes 2007) that produced a range of emotions including fear, outrage, anxiety and sadness.

For weeks I had recurring nightmares of the woman’s face floating out of a bathtub and coming toward me, on the verge of vomiting over me. Simply thinking about that incident brings back that atrocious taste into my mouth. (Diphoom 2013, p.210)

Wilkes reflects on the power of dreams in fieldwork and writes on this in the following episode:

It was a lucid dream, one in which the dreamer is aware she or he is dreaming. It was as if I was standing outside of myself at the bedside watching myself sleep. At the same time, I felt as if I was fully conscious. As I slept, I heard a voice, sharp and insistent. I could not grasp the words, or comprehend their meaning...Ultimately it became clear the voice would not leave me alone. So I rolled over in my sleep toward the sound, and there, at the edge of my bed, to my astonishment (even in my sleep), stood a golden eagle. It spoke to me in English and simply said, “Come with me”. I was frightened and tried to roll away... (2007, p. 69)

In many ways, this experience of emotional vulnerability never subsided. I was certain that I had to reflect on and manage these critical moments, and felt that my fieldwork and data collection could not progress effectively if I was not able to confront these issues head-on. What was important through my reflection on these critical moments was that the cumulative pressure from the field, the initial difficulty in gaining access given the sensitivity of spirit possession, and the general shortage of time and resources, further contributed to the distressing character of the endeavor of data generation. However, I came to realize that I needed to slowly experience this vulnerability as an embodied researcher, in view of the fact that “the emotional intensity of experiences with the paranormal can enable anthropology ... to further harness and advance ethnographic knowledge and practices” (Lee 1987, p. 69). These critical moments guided me through my periods of participant observation and data collection. By forcing me to experience such emotional and spiritual vulnerability first-hand, they also allowed me to make sense of and empathetically understand the experiences of my respondents and the prominence of emotions in their everyday lives when confronted with the supernatural.

My experiences not only generated a strong response from respondents. Given that they helped me frame questions during my interviews and in conversation with my respondents, these moments also opened intersubjective and dialogic encounters through which my respondents could interpret my emotions and behavior, as well as the responses these conjured from them. For instance, my presence and vulnerability during a healing session involving a female respondent who was perceived by her family to be afflicted by a malevolent spirit enabled those attending the session to share their own emotional states, after I expressed my emotions and experiences of vulnerability to some of them.

Many times throughout fieldwork however, I felt as though ethnographers were expected to act as semi-robotic, fearless practitioners capable of doing research

seamlessly. These professional expectations to be fearless are especially clear in the case of male researchers, who have often been socialized both personally and professionally to be “emotionally immune” and capable of suppressing their emotions (Pollard 2009). “Maleness” is hence not necessarily seen as advantageous, especially since the emotion regime of masculine emotional stoicism (Kimmel 1996; Sattel 1988) has been strongly entrenched in relation to appropriate gender performance in many academic cultures. While there has been a concerted gendered attention given to the potential dangers encountered by female researchers, such as sexual harassment, assault and hustling (Coffey 2002; Gurney 1985; Warren 1988), there is interestingly a noticeable lack of consideration of male researchers, and the advantages and limitations masculinity affords them. These issues are especially acute for male ethnographers researching in field sites that have been construed as typically “female” domains, and procuring interviews have occasionally been regarded inappropriately as sexual advances or attempts to set up dates (Kenyon and Hawker 1999; Sparke 1996). These observations diverge from the multiple experiences of women in traditionally “masculine” institutions—where gender membership has been shown to be either a “stumbling block” (Huggins and Glebbeek 2003; Okely, this volume) or is rendered as potentially advantageous (Diphooorn 2013; Marks 2004). Sattel (1988) further explains the differences of experiences in the field, particularly the centrality of emotional inexpressiveness and its relationship to the wielding of power and control among men.

In the course of data collection and generation, “maleness” was not necessarily a safeguard or advantageous while doing fieldwork on the supernatural. Sengers (2003) demonstrates in her research on cultic healing among Muslim women in Egypt that her position as a foreigner and woman afforded her advantages at her field site, while for Strauss (this volume), her students’ experiences as women complicated their efforts to make sense of their emotions and their participation in a refugee camp in Germany. In contrast, as a male researcher, some of my respondents and colleagues ascribed me with specific gendered attributes including the ability to be emotionally disengaged and detached from what transpired in the field. This is despite the fact that witnessing such events was initially disconcerting, terrifying and daunting to me. In the early stages of fieldwork, I was expected to be able to convince myself and others of the rightness of the decision I made of engaging in such research and to guard against my emotional involvement in the consequences of that decision. If other ethnographers—both women and men—were able to complete fieldwork on the supernatural, then surely this meant I would be able to accomplish this as well.

However, the oscillation from one intense emotion to another was exhausting, and I often felt debilitated, unable to share my emotions with friends and colleagues, for fear that they would downplay these experiences and doubt my competency as an ethnographer able to “do anthropology” and “masculinity” convincingly. Under such circumstances, I felt that the most difficult experience was facing and managing these critical moments of vulnerability, especially with regard to established, close relationships with afflicted family members in the field and how this in turn affected the ways they revealed their narratives to me. Nevertheless, by reflecting on

such critical moments and after spending more time in the field, I decided that it was immaterial to me whether these fears and emotions emasculated me or were downplayed by peers and colleagues as unfounded and even (problematically) “feminine”; what was at stake was my own emotional well-being, which was an important personal priority during the course of my fieldwork.

Following Goode and Hatt (1981, p. 121), I acknowledged that I did not necessarily need to participate in and attend all the healing rituals I observed, but could instead continue to remain open to other experiences and practices, given that the field researcher “need not carry out exactly the same activities as others in order to be a participant observer.” I found that it was useful during these critical moments to reflect on and record my emotions as candidly as possible in a personal field diary and to intersperse these with my field notes, despite the dominance of the ideal of masculine emotional stoicism in academic settings. In the field, it also helped that I brought along objects such as printed religious scriptures which were sources of comfort and reassurance, despite the fear that some of my colleagues would continue to brand such responses as “irrational,” thereby affecting my professional status. Nevertheless, these strategies ameliorated the tense and lonely periods in the field when I felt helpless and vulnerable, and which reminded me of early childhood experiences surrounding the death of my great-grandmother. These techniques of self-awareness allowed me to generate data more self-consciously as the months passed.

More crucially, and given my attention to personally and professionally critical moments of vulnerability, I realized that the focus of the methodological literature is on respondent-researcher relationships during fieldwork. Notwithstanding several interventions that attempt to reconfigure what the “field” means (Grindal and Salamone 2006; Howell 1990; Yamagishi 2006), this focus does not adequately capture the emotional impact of fieldwork and ethnography on our children, spouses, parents, siblings, friends, lovers and other relations (see chapter “[“Normality” Revisited: Fieldwork and Family](#)”, this volume). As such, the status of the field as a site discrete from our personal emotional life largely persists, while the world of familial and extended kin relations appear as a separate, if not less pertinent methodological category. Correspondingly, the research we venture into and the limits of involvement we set ourselves occasionally depend on interactions with our immediate social relations and support networks in our private sphere. Moreover, family life and ethnographic fieldwork reciprocally and productively inform each other. Reflecting on these critical moments enabled me to appreciate the fact that the entire research agenda is enmeshed in a set of social relations in and beyond the field.

In the research I conducted, there were several persons who were especially concerned for my emotional well-being. An earlier discussion, noted in my field diary, with my mother and grandmother regarding my selection of a topic for fieldwork can illustrate this point further.

Mother: Why do you always have to choose such sites for your research? Why don't you ask any one of your colleagues in the department whether they would allow their son, daughter or their loved ones to conduct research in such 'dangerous' places?

- Author: Don't worry. I know how to take care of myself. I know when to back down.
- Mother: Yes, but look, some of the practices are okay, but many are morally wrong.
- Grandmother: Yes, listen to us. Look at me before, someone sent 'something'⁷ to me after I married your grandfather. It isn't funny, you know. You can't get out from this spiritual trouble once you are involved. And you will make us worried.

Here, the issue of my own personal religious affiliation and experiences of vulnerability became pertinent for them, since this concerned the possibility of me transgressing moral and religious practices, affecting my spiritual well-being. No matter how many times I reassured them that I could manage it and knew my own boundaries, they were often displeased and frustrated with me for my purported moral "irresponsibility," not only toward my own spiritual well-being, but also toward my family, in choosing and participating in such research. The strong local cultural discourse among Malay-Muslim practitioners was that my close involvement with the afflicted "victims" of spirit interference could possibly affect my immediate family members and loved ones due to our close affective bonds, thereby rendering them vulnerable as well. This is illustrated in another entry in my field diary:

Often, I would hear the same oft-repeated issues not only from Mum and Nyayi [grandmother], but also from my other friends: 'Those people [*bomohs*] are good for nothing;' 'Are you sure this is all worth it?' 'What about us?'(...) It's so frustrating and tiring to explain all the time. I know I have to be a so-called 'professional,' distancing myself from the rest. But when I think about this more seriously, we researchers are not only part of the academic community; we also have different positions and are bound with moral obligations to others. I had an obligation and duty to my loved ones, as much as to those who I was studying. (Field diary, n.d.)

Was my relationship with my family members a self-imposed emotional impediment to my need to proceed further with data collection and generation? Were their "fears" and feelings of vulnerability groundless? I eventually decided, after several weeks of fieldwork, that it was more appropriate for me not to inform my family of my progress. I had made the decision to do this research on my own and for myself. As self-interested as this decision may be, I wanted to protect my emotional interests and make sure I was not considered a "burden" to my family and loved ones. As such, I decided to only inform my closest friends and colleagues of my whereabouts during the latter half of my research and depended on them during moments of distress. I realized that by doing so and through such critical moments, I was able to better manage my emotions. I was then able to collect and generate data more effectively and move in and out of the field with greater ease, unburdened by the "bias" and fear of having to hear what my family had to say. This did not mean that I did not consider their interests or advice. On the contrary, it made me reflect more carefully upon how far I would be able to endure and experience these critical moments

⁷This "something" refers broadly to an act of sorcery or malevolent spirit directed to an individual or group.

of vulnerability in the field against the advice and suggestions that I received from them and many concerned others. At the same time, I acquired a more enriched appreciation and understanding of the research setting and my respondents.

Conclusion

This chapter has explored notions of emotional and relational vulnerability, of being affected by others, and of experiencing being “out of control.” It moves beyond the overwhelming focus on physical vulnerability and demonstrates how ethnographic fieldwork influences not only professional, but also personal relationships, and our emotional obligations toward our “subjects” as well as toward our loved ones and ourselves. The turn toward heightened sensitivity to emotion, self-reflexivity, embodied subjectivity, skepticism toward absolute truth claims, and the crisis of representation and authority (Biddle 1993; Clifford and Marcus 1986) that has been shaped in part by the feminist project has reconfigured the contours of ethnographic research and the visibility of the ethnographer in the process of writing. Through embracing and foregrounding the subjectivities of ethnographers, this renders more explicit the blurred nature of the boundaries between “objectivity” and “subjectivity,” “self” and “other,” “personal” and “political,” “mind” and “body,” and, more crucially, “reason” and “emotion.”

The aim is to transcend traditional emotion regimes employed to appraise and interpret ethnographic writing and knowledge construction, and rethink methodological claims made to validity, reliability, and objectivity. The call to what William James calls “radical empiricism” disavows the “epistemological cut between subject and object, that endows transitive and intransitive experiences with equal status, and that investigates phenomena which inductive methods of traditional empiricism were never designed to treat” (Davies 2010, p. 2). The reference to the self and human emotions therefore does not render emotionally reflexive ethnographies epistemologically irrelevant, unscientific or necessarily overindulgent and corrosive to the research process (*ibid.*), insofar as self-reflection is “essential to the argument, not a decorative flourish, not exposure for its own sake” (Behar 1996, p. 14). Far from charges leveled at the recording of subjective experiences as self-absorption, personal emotions, thoughts, feelings and experiences of vulnerability can act as a means by which the social contexts and lifeworlds ethnographers are interrogating and making sense of can be better illuminated. When treated with the same intellectual vigor as demanded of our empirical work, rather than as a peripheral issue, attention to our states of being, in addition to our positionalities as researchers and persons, can positively influence the way we generate, acquire, and understand data (Diphorn 2013; Pickering 2001).

At the same time, the emotional imperatives embedded in the practice of ethnographic research afford us opportunities to reflexively explore vulnerability more closely. These are particularly pertinent when ethnographers interrogate emotionally charged encounters with ghosts and spirits, situations in which our emotions

and those of our respondents weigh heavily in our analysis. Experiences of vulnerability that surface at different points of fieldwork—"critical moments" as it were—are by no means exhaustive, and include moments beyond the study of the supernatural, and those which occur even after fieldwork is completed. Such "critical moments" in the field and beyond involve situations which enable and compel ethnographers to rethink ourselves and reflect upon our relationships with others. My feelings and emotions influenced how I acted in the field, and my actions simultaneously evinced emotional responses from others with whom I interacted. These moments are not necessarily predicaments or problems per se, but are important for the research process in understanding the supernatural, as well as for personal and professional transformation. I realized that these moments further reinforced the call to examine nonrespondent relationships with much more attention. To be sure, these moments may not be at all "obvious." They may involve integral events in everyday life during fieldwork. What makes these moments "critical" is that these make ethnographers consider and reappraise issues and relations in and beyond the field that were taken-for-granted at various junctures in the field. Most significantly, what is important is how these critical moments inadvertently affect data collection, generation, and interpretation. These include moments when we may have felt unsupported professionally and/or personally. They are occasions when we as ethnographers experience feelings of pressure, or when we lack confidence and courage, or when we make what we later perceive to be the wrong decision. In effect, such periods of vulnerability are what critical moments are. Acknowledging these critical moments helped me not only to manage field relations and my own fears and insecurities, but also enhanced the quality of my data through emotional reflexivity (Davies and Stodulka 2019).

How do we then make sense, reflect on, and manage our experiences of vulnerability and emotional challenges and complexities in the field? What are some of the methodological initiatives ethnographers could embrace when confronted with dilemmas surrounding issues of emotions, vulnerability, and other forms of distress? Of course, the intention here is not to prescribe suggestions that stifle the independence of ethnographers in the field. Instead, through such critical moments and ethnographic reflections, these experiences would provide an outlet for ethnographers to explore and possibly employ initiatives and strategies that move beyond mere speculation, "intuition," and the oft-cited but vague conception of "common-sense" when confronted with vulnerable encounters in the field. At the same time, these strategies must also extend beyond the mere exercise of increased reflexivity, but rather should be accompanied by a concept as to how ethnographers can use them concretely and persuasively for data collection and generation.

At the institutional level, ethnographers can potentially profit from informal, open discussions and sharing exercises with advisors and faculty before, during, and after fieldwork. These can establish social support networks and help foster a non-threatening academic community in which ethnographers can collaboratively reflect on critical moments, irrespective of the length of their experience in field research. These informal mentoring sessions have been important resources for ethnographers who feel debilitated and emotionally strained by their fieldwork. Under emo-

tion regimes which posit an emotionally stoic and “objective” researcher, not all ethnographers may be able to endure constant solitude, anguish, confusion and other vulnerable encounters in the field. While some ethnographers may be able to thrive in such environments, others may need to address and process their emotional responses in a safe environment, even if such experiences may be construed by others to be “irrational” modes of thought. Vulnerability and emotion are therefore experienced asymmetrically and differently based on the given context—different ethnographers respond in different ways, insofar as certain situations and feelings of vulnerability count as more pertinent and relevant than others.

My field experience in researching the supernatural is a case in point. Despite the fact that belief in the supernatural is a social fact regarded as meaningful by different social actors, ethnographers who experience moments of vulnerability when researching supernatural entities have sometimes been regarded as “irrational” and ‘unobjective” by colleagues. In the same way, ethnographers may be afraid to candidly express their distress deriving from emotional encounters and experiences of vulnerability, even to those whom they feel comfortable talking to. Through the reflection of different critical moments in the field, I came to realize it was important to cultivate an institutional milieu that enabled me to do this, without the professional fear that I would be regarded as a less capable researcher. Even if these emotional support networks may not have researched on the supernatural, this support—whether one-on-one or collectively coordinated—is necessary in helping us work through difficult issues encountered in the field. Faculty members who have encountered similar experiences should in turn be encouraged to be sensitive to such distress, in order to enhance better collegiality and scholarship.

From these reflections and informal discussion sessions, academic departments can establish flexible and broad guidelines (not didactic or authoritative ones) and (optional) training especially for early-stage researchers to address not only physical dangers in the field, but also the management of emotionally vulnerable periods. These skills, based on the accumulated wisdom and experience of faculty members, could be shared or disseminated openly, and should at all times be conducted in a nonjudgmental and nonevaluative manner. In addition, sessions presided over by senior academic mentors (Pollard 2009) could incorporate issues pertaining to academic publishing in relation to the codification of such methodological concerns, including reiterating the pragmatic dimension of fieldwork, such as the importance of sufficient funding and the need to have adequate “insurance coverage” (whether this be medical, financial, or/and “emotional”) and be assured that institutional help will be rendered if and when necessary.

My research on and experiences of the supernatural is thus aimed at initiating a broader methodological discussion in terms of the potential emotional and spiritual vulnerability researchers may experience when conducting fieldwork. My own vulnerability exists in relation to my multiple positionings not only as a field researcher, but also as a grandchild, son, brother, and colleague. Inasmuch as we attempt to be responsible to our respondents in the field, it is also important that we take seriously the obligations we have to ourselves and to others who are important in our lives. In writing our accounts of the field, and of the intersection between the personal and

professional, we ought to openly recognize the vulnerability and emotions that we may potentially experience, and how these can be drawn upon as a constructive resource both for ourselves, our respondents, and those beyond our field site.

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“How Did It Feel for You?”: Teaching and Learning (by) Emotional Reflexivity in an Undergraduate Fieldwork Training



Annika Strauss

Introduction

This chapter examines how emotional reflexivity can be incorporated into teaching fieldwork. In spring 2016, I led an undergraduate fieldwork training course entitled “Empirical Methods” (*Empirische Übungen*).¹ The aim of the course was to address the various sensory dimensions of fieldwork. I repeatedly drew the students’ attention to “how it feels” to do ethnographic fieldwork by teaching them how to systematically reflect on their fieldwork experiences. In the following, I first outline the background of the engaged anthropology project in which the fieldwork training was embedded. I then delineate the pedagogical and theoretical conceptualization of the course. The empirical part of this chapter is focused on the discussion of two students’ emotional experiences in the field that took place during a class session. It shows how students’ processes of understanding can benefit from a group-based collective reflection of field episodes among peers. I will also investigate the socio-political dimensions of what I call “classroom emotions.” I argue that learning and practicing emotional reflexivity requires a protected space and a self-reflexive, empathic teacher.

By reflecting on academic experiences beyond fieldwork training, I intend to show how the expression and discussion of emotions are shaped by the university context and how we can challenge the “emotional regimes” of the classroom. My contribution is based on detailed field notes taken during the engaged anthropology

¹The practice course “Empirische Übungen” (Empirical Methods) offered in the second semester is part of the curriculum of the BA program “Kultur- und Sozialanthropologie” (Cultural and Social Anthropology).

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project and the fieldwork training. To collect material and address the broader context of emotions in academia, I also organized a graduate student group discussion as part of which two students wrote an essay using a free association approach about emotions in academia. I include some of the information contained in both essays and complement them with anecdotes concerning students' emotional experiences in fieldwork and academia that were collected during encounters with students and colleagues at conferences and informal meetings, over coffees and beers.

Volunteering at a Refugee Shelter

The undergraduate course was embedded in an engaged anthropology project² in a *Notunterkunft* (emergency shelter) (Bogumil et al. 2016). The project was initiated in October 2015, when the German government opened the borders to asylum seekers entering the European Union from Southeastern Europe. The project aimed at establishing a space where refugees and social anthropology students could encounter and interact with each other. In December 2015, my colleague Natalie Gies-Powrozniak³ and I began supervising the project. In the shelter, asylum seekers had to register to be eligible for accommodation and receiving other social welfare benefits. While officially registration is supposed to take anywhere from 2 to 3 weeks, in the winter of 2015 most asylum seekers had to wait up to 10 months for their paperwork to be completed. The shelter was established on a former military ground that had been vacated by the British in November 2012. Although the ground is equipped to house up to 800 persons, about 1200 persons took shelter there. In April 2016, there were still about 300 persons living at the shelter. A remnant of its past, a high wall topped with barbed wire surrounds the ground and called forth the image of a prison in many of us when visiting the camp for the first time. During the first session on a warm October day, we brought several musical instruments like drums, rattles, a guitar, and a keyboard. The children immediately grabbed all the small instruments—the rattles and small drums—while screaming and quarreling. Nobody used the instruments in a coordinated manner as we had imagined. We ended up playing Syrian music from two speakers that a man had brought from his room and danced together. The activity was greeted with enthusiasm and followed by an extended “dance session.” One of the emotional field episodes took place during such a dancing session and will be analyzed in detail further down.

Apart from organizing the dance sessions, the participating 18 anthropology students—who were organized into six groups—set up a slackline at the shelter,

²The project was headed by Prof. Dr. Helene Basu, head of the Institute of Ethnology at Westfälische Wilhelms-Universität Münster.

³Natalie, like me, is pursuing her PhD in social anthropology and carrying out participant observation in grade schools in order to better understand the situation of young refugees. During the fieldwork training, she supervised the activity sessions at the refugee shelter, while I taught the course contents and organized the methodological reflexivity exercises.

arranged a picnic, knotted friendship bracelets, and baked wafers. We concluded each session with a feedback round. During these sessions, the students were encouraged to share their experiences and to address problems and conflicts immediately. As part of their fieldwork training, undergraduate students were asked to complete several assignments. I asked each working group to submit a short research proposal and a research report at the end of the course. Furthermore, students were asked to submit the field notes they took during our initial visit to the refugee shelter. At the end of the course, each student wrote an essay that reflected on a concrete field situation. We had gone to the field seven times and had conducted six activity sessions at the shelter. Eight of the altogether 11 classroom sessions were 2-h sessions in which we discussed texts on ethnographic fieldwork (Madden 2011; DeWalt and DeWalt 2002; Beer 2003; Bernard 2006), the research ethics of engaged anthropology (Huschke 2014a, b), embodiment (Okely 2007), and emotions, and the relevance of self-reflexivity (ibid. 1996). In two 4-h sessions, we discussed the students' field notes and employed the Image Theater Method (Strauss 2017) to encourage collective reflection on fieldwork experiences (Nadig et al. 2009). In the final session, we discussed the students' reflexive essays, providing detailed feedback for each of them.

The Epistemological Significance of Emotions in the Field

The idea behind the course on *Empirische Übungen* aligned with the main concern of this edited volume: using the tools of anthropology and related disciplines to unravel the sociocultural meaning of emotions (Beatty 2010; Röttger-Rössler et al. 2015; von Scheve and Luede 2005), as a means of reflecting on researchers' fieldwork experiences, and thus generating insights into their research contexts (see chapter “Foreword: Pathways of Affective Scholarship” to this volume; also Berger 2010; Davies and Spencer 2010; Linska 2015; Pink 2009; Spencer and Davies 2010; Stodulka 2015). The fieldwork training course was intended to familiarize students with emotional reflexivity and to support students who were engaging both as volunteers and fieldworkers at the shelter for asylum seekers.

Learning and Teaching (Emotional) Reflexivity

Learning during fieldwork means encountering and engaging with ways of knowing, being, and doing that are different from our own. According to Dimitrina Spencer (2011), fieldwork constantly challenges the anthropologist's self and renders it amenable to change. Spencer refers to this process as “transformative learning” in social anthropology, borrowing the term from educational research (Spencer 2011, p. 70; Taylor 1998). Spencer recommends supporting and creating space for transformative learning by including and articulating the embodied experience of

students in the classroom. This helps to develop students' reflexive skills and self-awareness regarding how they learn and how this might be part of anthropological fieldwork (Spencer 2011, p. 81). Furthermore, she proposes certain strategies for including emotional reflexivity in our pedagogy in order to encourage transformative learning, for example, by creating an emotionally articulate learning environment, focusing on skills that enhance emotional reflexivity, drawing attention to the nature of fieldwork research as a process of relational reflection and to the different degrees of emotional reflexivity required by various research questions, and considering the moral, methodological, and theoretical implications of not being sufficiently emotionally reflexive (*ibid.*). Tim Ingold finds that learning takes place as learners develop their own understanding by engaging with their surroundings. Based on this consideration, he conceptualized a course in anthropology at Aberdeen University that supplements traditional lectures with classes applying experiential learning methods that engage students in doing and making (Ingold and Lucas 2007). Taking up the suggestions and ideas of Spencer and Ingold, I included reflexive and experiential exercises throughout the course to supplement exclusively theoretical contents. As I will describe in the following section, restaging fieldwork experiences through the Image Theater Method was one way in which I embraced this approach.

Facilitating Reflexivity by Restaging Fieldwork Experiences

Ethnographic analysis is a process of reinserting ourselves into the reality of the field through the work of memory and imagination. Discussing our findings with fellow researchers in informal talks, colloquiums, or workshops can help us to make sense of fieldwork experiences. The Image Theater Method combines memory and imagination with group-based collective reflection on fieldwork material. Erving Goffman (1990) understands all social interactions as a kind of theatrical performance where we take up the roles both of an actor and of an acting-evaluating spectator. The concept of Augusto Boal's *Theatre of the Oppressed* (2008) is based on the presumption that staging always implies observation and therefore an objectification of social interaction. This performative method encourages nonprofessional "spect-actors"⁴ to reenact and improvise unsettling scenes from their social life. Time, space, and social dimensions all become contained on stage, and thus spect-actors are able to objectify what is happening to them. This objectification comes close to Tedlock's concept of an "observation of participation" (1991). It includes sensory memorization, attention to emotions and relational dynamics, and finally analyzing and potentially influencing the outcome of the enacted situation. The Image Theatre Method opens space for reflexivity by (re)staging a fieldwork

⁴Augusto Boal developed the term "spect-actor" (orig. Portuguese *espect-ator/espect-atriz*) to describe the dual role of those involved in the *Theatre of the Oppressed* as both spectator and actor and as both observing and creating dramatic meaning and action.

situation. This allows a kind of meaning-making through various, multisensory channels, which stands in contrast to conventional verbal and text-based approaches, for example, presentations or analyses of field notes. The performance of a single image serves to physically reembody and condense a situation from the field and reflect on its possible significance for analysis. The use of language is prohibited during the first part of the method. Thus, participants concentrate on the physical aspects of the enacted situation, on what they see and what the image may evoke in them: “In Image Theatre the use of words would interfere with the language of images or superimpose itself on top of that language” (Boal 2003, p. 129). Image Theater focuses on the numerous meanings an image might have: “(...) the meaning of an image is the image itself. Image is a language” (ibid., p. 175).

To prepare the students for the Image Theater Method,⁵ I guided them through a short imaginary journey exercise. I first asked them to recall a situation or scene from the field in which they were moved, scared, unsettled, or confused. I then asked the students to sit down, relax, and close their eyes. I virtually sent them back to the situation in question and guided them in imagining as many details as possible from the scene: sounds, smells, the environment, colors, and tastes. After everyone had mentally left the situation, returned to the classroom, and reopened her or his eyes, I asked the students to write down as many details as possible about the situation they had recalled. Then we looked at one of the student’s situations in more detail by conducting the Image Theater Method. Claudia volunteered to reenact her situation. She acted as a “sculptor” and asked other students to come “on stage” to reenact the scene. Claudia’s peers acted as statues and followed Claudia’s stage directions. She concentrated on the most salient aspects of her experience and thus created a condensed image.

In this application of the Image Theater Method, the sculptor is only allowed to use touch and mirror image to give the statues their postures and facial expressions. Claudia eventually assumed her position within the image (see Fig. 1). At this point, I took over and asked those students who were not involved in Claudia’s image to study the image. I encouraged them to move around the “statues,” to see and observe them from diverse perspectives (see Fig. 2).

I proceeded to ask the students what came to their minds as they were watching the image. They commented on the various positions and bodily postures and mentioned the possible social dynamics, power structures, and hierarchies. The students made the following associations (translation A.S., see Fig. 3). One of the students documented them and wrote them on the blackboard:

vulnerability, dance session, demureness/reservation, girls/boys, joy, shame, courage/bravery, vicarious embarrassment, disapproval, demarcation/distinction, observation/monitoring, two parties, roosting,⁶ discomfort/unease, insecurity, gender, maverick.

⁵The Image Theater Method is a tool to access embodied fieldwork knowledge. I discuss its meaning in detail elsewhere (Strauss 2017).

⁶This refers to a German proverb meaning literally “to roost as chicken do.”



Fig. 1 Claudia's image. Photo: Annika Strauss

After all the comments had been written down, I asked those students who were enacting the situation to share what they felt in their respective positions. Their experiences were also documented on the blackboard.

Julian: skepticism; Constantin: discomfort/unease; Amir: expectation, transformed; Hannah: ogled, judged, devalued, outcast; Claudia: trepidation, constrained, judged, unfree, dominated.

Then, I asked Claudia, the “sculptor,” to disclose and narrate the context of the situation:

The situation took place during our last session at the refugee shelter. We organized a dance session for all people currently staying there. We organized two separate sessions: One that was for women and girls⁷ and another for everybody. I planned the general dance session, where predominately men and children took part. The other students who were there were Amir, Constantin and Hannah. I was particularly looking forward to this session because dancing is important in my life. For me, it signifies the possibility of personal expression and a moment of relaxation. It was a hot and humid day and the session took place in a rather small room. In the beginning, the atmosphere was rather tense and the situation chaotic because there were some problems with the stereo. Also, we hadn't planned which music should be played. When the music started playing we danced mostly with the children. Some men joined the group, but remained in the observing position at first. The songs we played were current chart hits. Dancing with the children was fun and the atmosphere became more and more relaxing and cheerful. But a feeling slowly crept over me, that the men sitting on the sidelines were observing me and I didn't feel as free anymore; I increasingly adopted a cautious posture/stance. Meanwhile, more and more men entered the room,

⁷Almost no women, except some young girls, joined in the general dancing sessions during the winter term. This led me to suggest that the students might organize a separate session for girls and women.



Fig. 2 The spec-actors: Claudia (a), Hannah (b), Julian, Constantin, and (c) Amir. Photo: Annika Strauss

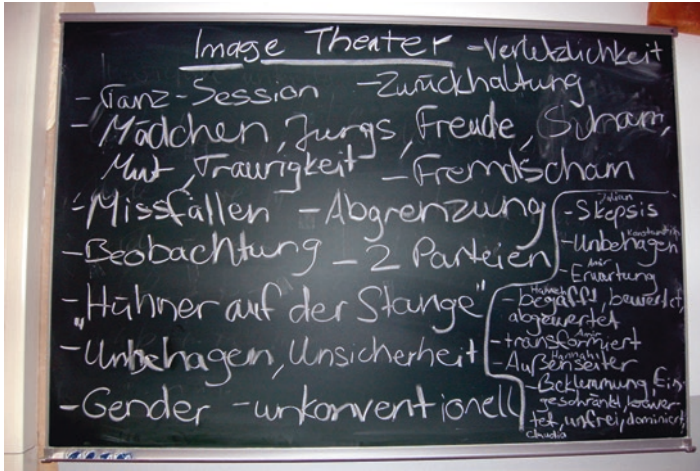


Fig. 3 The associations. Photo: Annika Strauss

and at some point, I felt the urge to share my oppressive/disturbing feelings. I remarked to Hannah: ‘Do you realize?...we are the only women in this room...’ But she just waved my concerns aside by saying: ‘Yes, but what do I care?’ It was very hot and at some point I observed that Hannah got rid of her cardigan and went on dancing only in her spaghetti-strap top. I remember that I was dressed rather inappropriately for the high temperature, too, but nonetheless I was reluctant to cast off any piece of clothing. The situation was later resolved as the men started to play their music; we all danced together and Hannah and I were no longer the center of attention. Other female participants in the seminar joined in as well. (Excerpt from Claudia’s reflexive essay, translation A.S.)

Next, we began a discussion that was designed to disentangle emotions, irritations, and associations. We addressed the following questions: What kind of relationship structures are displayed in the scene? What interpersonal dynamics can be recognized? What role and position did Claudia adopt in the scene? Which anxieties, fears, irritations, conflicts, projections, and defense strategies are evoked? Which sociocultural practices/habitus may play a role? Which stereotypes (e.g., concerning gender, religion, age, or ethnicity) may be at work?

The discussion of the image and of Claudia’s and Hannah’s reflexive essays focused on the diverging behaviors of the two female students. To Claudia’s surprise, though, both equally experienced the male gaze as judging, constraining, and dominating. While Hannah took a stance that she framed as a kind of “resistance” and went on to dance freely, Claudia reacted to the gaze by adopting a reluctant and reserved attitude and body posture. In their essays, both students reflected on how their socialization probably influenced their reactions to the situation. Claudia was brought up in a Catholic family of Polish origin, where women were supposed to adopt rather passive and modest roles. Hannah, on the contrary, described her upbringing as rather emancipative. She was always encouraged by her mother to claim her rights and articulate her opinion freely.

Another aspect brought up by a fellow female student was their age difference, and that this might be reflected in Hannah’s recalcitrant response, who was 20 years old, versus the more mature, reflective, and self-disciplinary behavior of Claudia, who was 30 years old. We also discussed how the feelings of insecurity and disorientation in the situation were the result of being in a sociocultural context in which they could not be sure about others’ norms and expectations, which made it difficult to assess their own behavior as well as the behavior of others.

The next aspect discussed concerned the position and role of the men in the image. The diverse associations (“skepticism,” “discomfort/unease,” “expectation”) reflect Hannah’s and Claudia’s difficulty in grasping the attitudes of the men toward them at that moment. It became clear that Claudia and Hannah interpreted the gazes according to stereotypes of Muslim men as disapproving of their behavior, sexualizing and objectifying them.⁸ Hannah stated in her reflexive essay that she realized her assessment of the situation was informed by stereotypes, and that the men could have also been just curious or surprised, because freely dancing women may not have been part of their social reality before coming to Germany. We further discussed how sexuality is framed differently depending on particular cultural contexts, and addressing Hannah’s act of “undressing” herself, how the exposition of certain body parts can be read as conveying a particular sexualized message: for example, the *décolleté* is perceived as sexy in Western countries, an ankle can be a reason for sexual arousal in Arabic countries, and Japanese Geishas are known for their back *décolleté*. Accordingly, what is perceived as appropriate or offensive behavior varies highly cross-culturally. Nonetheless, it also became clear in the discussion that the body parts in all these cultural contexts were always attached to the female body and objectified by the male gaze. What does the discussion of Claudia’s image teach us about the epistemological value of emotions in the field? A systematic analysis of the emotions involved in the scene attends to Claudia’s and Hannah’s biographies, the structure of their respective narratives, as well as to their socialization and upbringing: For Claudia, seemingly judging gazes prompted her to restrain herself; for Hannah, it was a signal “to woman up” and to insist on her rights to move and dress as she pleases. Claudia and Hannah certainly better understood their reactions and behavior in the field after reflecting upon their emotions, but what does the situation say about other persons present in the field? Can we learn some-

⁸ During the feedback rounds of the volunteer sessions we discussed and reflected on paternalistic and racist discourses that were particularly salient during and after the “Köln event” at turn of the year 2015/2016, when sexual assaults and numerous property offenses took place during the New Year’s Eve celebrations in the city center. Reportedly, the perpetrators were unmarried Muslim men. Conservative and anti-immigration voices quickly built the image of “sexist” Arabic men, and particularly refugees, who threatened the freedom of “modern” women, who served as a symbol of “emancipation” in German society (Dietze 2016). Because we wanted to avoid feeding the paternalistic and racist message that Muslim men were not capable of being integrated and thus threatened the existing social order by means of their “sexist socialization,” we tried to create a space for reflection and self-reflexivity, where such incidents could be discussed, interpreted, and addressed from multifaceted perspectives.

thing about the men's position in the scene? And maybe also about the women's who, as Claudia remarked to Hannah, were not present?

During the discussion, some students suggested that Hannah and Claudia, being influenced by the media and by what they heard about "suppressed" women in Muslim societies, may just have "imagined" the gaze. But to dismiss both students' perceptions as mere inner processes and delusions would not do them justice. Instead I would suggest that the situation must be understood as an incidence of "countertransference." This psychoanalytic term originally designated psychoanalysts' emotional reactions toward their patients. In the therapeutic process, the analyst pays attention to his or her own emotions and the "activity of self-interpretation" (Braddock 2010, p. 221) in an attempt to grasp the patient's life and experiential world. Ethnopsychanalysis subsequently adopted the concept and the hermeneutic approach to analyze ethnographic encounters between field researchers and their informants:

What that emotional understanding equips the field worker with is a principled basis for seeing how things are for the other from their point of view, and what it is that they may unconsciously (...) be communicating, through behaviours and actions which elicit emotion. (ibid.)

Let me add another anecdote that may help us to decode the implicit social message sent by the men in the encounter (most likely unconsciously). During the round of feedback that we conducted after the activity session, another graduate student shared that she later spent time with a Syrian girl who did not want to take part in the general dancing session. She stated that her father would disapprove of her dancing openly and that she would not be joining the session. The girl most likely experienced a so-called "socializing emotion." Anxiety, fear, shame, and high self-esteem "are culturally emphasized and elaborated in order to support and mediate the transmission of social norms and values to children" (Röttger-Rössler et al. 2015, p. 188). Culturally specific child-rearing practices like frightening, shaming, praising, and cherishing lead to the construction of cognitive schemas which can resurface as general psychological control mechanisms: "If children have developed a schema based on shaming experiences in consequence of a disapproved behavior, they will start to re-experience shame when intending a similar (mis)behavior" (ibid., p. 191). The girl decided against taking part in the dancing session because she was afraid her father might become angry with her.

Claudia's reaction is similarly caused by an internalized social norm. Accordingly, the socializing emotion "shame" (*Scham*) written on the blackboard refers to Claudia's restrained and modest posture. Hannah, on the contrary, internalized a different appraisal. She elicited the socializing emotion "high self-esteem," which was instilled in her by her mother, whom she experienced as emancipating. When she felt she was being judged and dominated, she enacted a social narrative contrary to Claudia's: "resistance" and "rebellion." In this case, Claudia's and Hannah's experiences of adjusting to a strange social situation exemplify how the regulation of gender appropriate behavior works through internalized norms activated by being subject to a gaze. Here the individual attitude of the "ogling" men is not relevant as

such. Whether their gazes imply judgment, expectation, or just curiosity, the objectifying gaze itself implies a process of categorization and evaluation of the nonconforming “other.” Hannah’s feeling like an “outcast” supports this interpretation. These two students experienced how certain emotions are elicited when they do not conform, and how this leads to self-monitoring and even self-discipline. Because explicit information about what is socially appropriate is not readily available, they fall back on cognitive appraisal schemas formed through their socialization.

Besides the male gaze, we must also consider Claudia’s reaction to Hannah as she rebels against the men’s stares. An association written on the blackboard anticipates a particular relational dynamic: *Fremdschämen* (vicarious embarrassment). This German term, difficult to translate in its literal meaning, describes how Claudia supposedly assesses the behavior of Hannah as inappropriate and feels ashamed on her behalf. Socializing emotions may not only frame an individual’s situational response, but through peer regulation their influence can be extended to other persons as well. Even if Claudia did not experience vicarious embarrassment: Hannah’s action of distancing herself from regulating social norms triggered something in Claudia; a process which led her to include Hannah when restaging the fieldwork experience.

Claudia’s and Hannah’s experience, respectively, can be understood as a “Key Emotional Episode” (KEE) (Berger 2010). Peter Berger coined this term to describe a researcher’s emotional and physical involvement when she or he “has lost control and is subordinated to the flow of events” (ibid., p. 120). As part of the fieldwork process, KEEs can enhance the quality of anthropological insights. The detailed analysis of the students’ KEEs provides a glimpse into how their emotions are linked to social structures. When social situations trigger strong emotional reactions, these always imply certain cognitive appraisal schemas, action tendencies, and social meta-narratives that eventually navigate individual behavior according to internalized social norms (von Scheve and Luede 2005, p. 322). The contextualization of the KEE may enable Claudia and Hannah to empathize with the girl fearing her father or with any other woman in the field. Its methodological value “lies in the fact that it may highlight crucial themes, norms or values of the particular culture” (Berger 2010, p. 138).

At the end of Hannah’s essay, she mentioned that, based on her field experience, she would like to further explore the position of women, the attitude of men, and the existent gender hierarchies at the shelter. She further states that she would not have paid attention to the scene and would have suppressed the thoughts it evoked if she hadn’t been encouraged to take a closer look at it through the guided reflection. Since the quality of reflexive engagement varied among students, this approach must be practiced. As my detailed analysis of Claudia’s image shows, a hermeneutic approach includes the evaluation and combination of several observations alongside contextual information. Regardless of the level of reflexive analysis reached, most students finally embraced the idea that their own experiences, sentiments, and emotions are relevant in participant observation, and that they should reflect on them rather than ignoring and excluding them from their field notes.

The Sociopolitical Dimension of Classroom Emotions

The classroom and academia are neither free of emotions nor of social norms. Learning processes do not take place independently from social relations or students' dispositions. Claudia mentioned in her essay that she felt that her fellow female students could better empathize with her than her male peers. I myself noted down a similar observation after the session. Two of the male students implicitly suggested that Claudia and Hannah might have "misinterpreted" the gazes of the men, and that they might have actually been looking somewhere else. I do not deny the possibility of misinterpretation, particularly in transcultural contexts. But for a self-reflective person who is monitoring her senses consciously, which we can assume is the case during participant observation, it would be highly unlikely for somebody to feel stared at if she were actually just glanced at. As may have become clear above, the social and communicative function of emotions mainly work implicitly, which makes it easy to ignore or render social messages that are sent nonverbally as nonexistent. Furthermore, arguing that a perceived gaze may be "just imagined" is part of a paternalistic strategy that purposefully devalues certain—particularly female—voices. In the following, I discuss the importance of cultivating awareness of the interpersonal and political significance of classroom emotions and how social and power structures may be reinforced through emotional practices.

Self-Reflexive and Empathic Teaching

When we organized the dancing sessions at the refugee shelter, I took part in the session for women and girls only, where one of the students taught us a hip-hop choreography. Later we joined the general dancing session. In my field notes I describe how I entered the room right after Hannah had just taken off her cardigan and was dancing with a young man. I remember how an uncomfortable feeling (a kind of *Fremdschämen*) was aroused in me. But I also immediately rebuked myself for judging her behavior. As we were leaving the shelter, Claudia shared with me how dancing with the young men and being watched caused her to feel awkward. We discussed our position as Western women, and I shared my experiences from my fieldwork in India with her, how I never escaped being marked as "the moral other" and how this categorization always left me with a sentiment of being indecent.

Reflecting once again on how I felt uncomfortable on behalf of Hannah, I recognize how this was probably elicited by an appraisal schema which I internalized during my "field socialization" in India. There, while carrying out my graduate and doctoral research, I could literally observe my behavior and posture changing under the male gaze. One day I walked to a friend's apartment one street away from my hostel in a suburban neighborhood in Delhi. I walked self-consciously, under the gazes of numerous shopkeepers and men loitering on the street. Aware of the fact that as a "good girl" I was not supposed to loiter (Phadke et al. 2011), I headed

straight toward my destination; my eyes did not wander but were fixed on the ground in front of me. Later that day my friend told me that his landlord observed me coming to the apartment and had praised me for being a “good girl,” one who does not stroll around or make eye contact with random men on the street. His comment aroused mixed feelings in me. On the one hand, I was proud that I was able to accomplish the task of walking in a gender appropriate way in this particular socio-cultural environment. It was satisfying to “pass the test,” and it was one of the rare times I did not feel marked as the Western girl who would never be as modest and innocent as an Indian one. At the same time, I felt suppressed and dominated. I abandoned a part of myself by subordinating my body and adopting a locally expected posture.

I think that my familiarity with these ambivalent emotions let me empathize both with Claudia, who tried to conform with the expectations of the monitoring male gaze, *and* with Hannah, who took a stance and did not let anybody regulate her body—something I failed to accomplish many times in India. By resisting my own initial intuition to take sides with Claudia, I managed to empathize with both students, and thus avoided “silencing” Hannah’s negotiation of the objectification she was experiencing. By sharing my fieldwork experiences during an informal conversation with Claudia after the dancing session, I hoped to deepen her understanding of her own experiences. I encouraged her to reenact and discuss the scene during the class session, because I saw it as an experience that could teach us something about “gender roles.”

Being empathic toward our students, similar to being empathic toward our informants in the field, helps us understand what they are going through and the challenges they face (Golub 2015). Accordingly, Angela Jenks (2016) suggests it would be better to try to understand students’ feelings, concerns, and opinions than to dismiss them as rebellious or offensive, or to complain about their inability and lack of discipline. Teaching methods do not function as abstract, theoretical didactic models; they work because we implement them attentively and adapt them to the group and environment in which we are working. We also have to be aware of our defense mechanisms as lecturers, in order to attend to and understand what really happens in student–teacher interactions.

Unfortunately, at least in my experience, lecturers are advised to keep a distance and avoid empathic interactions (and therefore also emotional reflexivity) with students. Many argue that this distance is important in order to “safeguard objectivity” in evaluating students’ academic performances. These unwritten rules of (emotional) conduct in university teaching often seem to serve the function George Devereux (1998) ascribed to “objective” methods in the social sciences: they control anxieties, fears, and insecurities. At least when I discussed teaching methods and student–teacher relationships in more detail with university colleagues, an oft-mentioned reason for avoiding “friendship-like” relations with students was to prevent inappropriate feelings—such as anger, insecurity, attachment, or even falling in love—from developing in “professional” settings.

Accessing Our “Fieldwork Selves,” and the Meaning of Transformed Emotions

Rules of conduct in academic spaces predominantly expect emotions to be handled “professionally” and relationships to be framed by “formality.” Affects are ideally dealt with by means of a logical distance, so as not to interfere with “objectivity” or “professionalism.” Strong emotions are nevertheless prevalent: competing academics may envy each other’s success; a young scholar might admire the work of a senior academic in her field; a scientist may feel joy from a scientific discovery; and a PhD student is likely disappointed and angry when a research project is not allotted funding. Billy Ehn and Orvar Löfgren characterize the university as an “emotion-controlled” but “contradictory organization” (Ehn and Löfgren 2007, p. 103). “The taboo of certain feelings at the same time results in a tendency to indirectness, the appearance of emotions in disguised forms or in surprising contexts” (ibid.). They describe how the specific political and cultural organization of everyday life forms particular “structures of feeling” (ibid., p. 114), and how these, anchored in certain routines and frameworks, give rise to an institutionalized “emotional habitus” (ibid., p. 115). It is this academic emotional habitus that also supports “defensive professional practices” and in this way interferes with an emotional, reflexive pedagogy: “‘Managing’ and ‘controlling’ is a way to keep the defenses intact and prevents us from understanding emotions and their role in producing knowledge and from articulating how we feel” (Spencer 2011, p. 76). Dimitrina Spencer investigates the “compartmentalization of self” as an example. The term refers to “cleavages between different states of being” and correlates with “feeling and acting one way in one place, and rather differently in another, with no evident reason for such disparity” (ibid.). In social anthropology, this corresponds to the embodiment of a “professional” versus a “personal” self, and feeling and acting in the classroom differently vis-à-vis the field. Fieldwork emotions are often allocated to informal spaces such as coffee breaks or dinner parties, while the theoretical-analytical accounts of fieldwork experience are allocated to classes and conferences (ibid., p. 77).

Similarly, the kinds of relationships we allow to evolve in these spaces—namely, open, empathic relationships with our informants versus supposedly controlled and formal relationships between students and supervisors—may originate here. While Spencer discusses this compartmentalization of self as embodied by academic scholars, I found it equally present in students’ attitudes, practices, and expectations. When, in the context of a group discussion among graduate students on the topic of emotions in academia, I asked two students to write an essay using free association, they wrote that emotions and academia are in fact two spheres which should be kept distinct from each other. One called the distinction “personal” vs. “institutional,” whereas the other labeled it “private” vs. “business.” Both described how they experience it as irritating and inappropriate when the “personal” or “private” are addressed or enacted in the university sphere. This illustrates how the breach of self-related categories may be experienced as fear invoking and as some

form of danger. This fear is exemplified in the narrative of one of my students, whom I had offered to address myself informally on a first name basis.⁹ After we had already developed a productive student–teacher relationship, she disclosed how my proposal to switch to the informal “you” (*Du*) had completely jumbled her concept of a “formal” teacher–student relationship. She had racked her brain thinking about how to resolve the situation in a convenient way without rudely rejecting my offer, and had consequently suffered from a headache. Reframing an unquestioned, conventional relationship model can entail a transformative learning experience that challenges former conceptions of the student’s self. Practicing emotional reflexivity and accessing the “personal fieldwork self” often requires us to transgress the academic compartmentalization of self.

Indeed, the refugee project enabled us to breach the “academic habitus” in subtle ways. Natalie and I decided at the beginning of the course to introduce ourselves to the students by our first names. In this way we adopted the rather informal relationship structures and forms of addressing that marked the interactions between volunteers and staff at the shelter. Supervisors and students went into the field together, all embodying the social role of volunteers. As one student commented, “It was indeed always clear that you were our lecturer. But you still took part in everything and you did exactly the same things as we did in the field.” In the classroom, our different institutional roles as students and teachers are also marked by certain professional practices, such as standing versus sitting, lecturing versus hearing, and guiding versus following. By contrast, at the shelter, we engaged in similar practices and embodied the same institutional role as volunteers. The experiences we shared

⁹We can understand forms of addressing in the context of classroom interactions as contributing to a compartmentalization of the self. The German language provides two pronouns to address the other: The first is the respectful, polite, formal, and distant *Sie* (third person plural pronoun) or *Herr/Frau* plus the person’s last name. And the second is the informal and more intimate *Du* (second person singular pronoun) or first name or alternated forms of these two. Of relevance are age (difference), status, gender, and possibly social category. Additionally, rules of conduct vary regionally. During most of the twentieth century, persons outside the family circle were addressed with the third person plural pronoun (except very close [childhood] friends). Nowadays the social contexts in which people encounter each other seem to increasingly determine how people address each other. Particularly hierarchical, business, and administrative interactions are marked by a formal, distant addressing, while the informal addressing predominates in informal contexts like sports clubs or parties. In some contexts, addressing informally indicates an equal status and/or belonging to the same social category (e.g., students or university colleagues of same hierarchy level) or membership in a certain social group (e.g., labor party). In other contexts, addressing informally can be read as marking antihierarchical, antibureaucratic, or anarchical spaces (e.g., politically left milieus or alternative subcultures who construct themselves as antipodes of prevalent social norms). Once switched to the informal way of addressing in a relationship going back to the formal “*Sie*” is normally perceived as an indicator for a major conflict and/or cease of trust. Because social rules that guide how to address somebody else are partly inconsistent, and because how to address somebody else “adequately” depends on the social context, on the social group one belongs to, on social status, as well as on age, gender, and the relation to the other person, the decision on how to address somebody else holds manifold pitfalls (Besch 1998). I normally offer to address students by first names when the relationship transforms from a punctual, mere administrative-driven interaction into a more or less long-term supervising or learning relationship.

as “personal fieldwork selves” breached the often-experienced gap between “here,” that is, the classroom, and “there,” that is, the field, between the experiencing student and the theorizing supervisor.

Relationships formed within academia have the power to transform those involved, including our emotions and attitudes toward our fields. Heaton Shresta (2010) shows in an autobiographic account how interactions with her supervisor and colleagues brought her to reformulate her emotional experiences in her doctoral thesis. Referring to William R. Reddy, she argues that “academic emotional regimes” change, build, hide, or intensify emotions¹⁰ by “the complex of practices that establish a set of emotional norms and that sanction those who break them” (Reddy 2001, in Heaton Shresta 2010, p. 45). The “power of ambivalence” in scientific settings is likely to lead to a situation in which “[s]ome emotions are rapidly changed into other moods” (Ehn and Löfgren 2007, p. 114).

In one essay, a graduate student described her anger, desperation, and disappointment at the seminars, teaching methods, and supervision, which she felt actually hindered her learning. The following passage demonstrates how her disappointment is repeatedly transformed into self-disciplining acceptance and resignation: “Of course I am annoyed when my expectations regarding a seminar are dashed, but acceptance and the desire to learn from these kind of experiences is stronger here. In some things we just have to acquiesce (student essay, translation A.S.)” Sue Wright (2004) argues that applying personal–political reflexivity to our academic environment helps us identify and work out ways to act upon sociopolitical structures, institutional discourses, and unfavorable policies. Wright designates those students who are able “to analyze daily encounters, their reactions to them, and signs of how the actors seemed to expect them to act” and “to uncover the detailed ways in which boundaries, hierarchies and power relations operated in the institution” as “politically reflexive practitioners” (ibid., p. 40). The student in the example above might learn to understand her resignation as molded by a context of institutional power structures, which prefer a student who is self-disciplined and adaptive to one who is angry, rebellious, or transformative. Such a reflexive understanding would ideally enable us to figure out subtle ways to negotiate and pursue learning processes. Similarly, university teachers who “problematize the institutional environment, and question how national policies, the university’s procedures, classroom interactions and their own values interact” (ibid, p. 47) are able to encourage and support their students’ learning careers in sustainable ways.

To be able to measure the historical–political implications of “academic emotional regimes,” we have to take into consideration their sociocultural conditionality. Politically reflexive teachers scrutinize how classroom emotions and their

¹⁰According to William R. Reddy statements about emotions are “an effort by the speaker to offer an interpretation of something that is observable to no other actor” and as such they are “neither descriptive (constative) nor performative—they neither adequately represent nor construct (perform) emotions” (1997, p. 331). Reddy argues against a strong constructivistic approach that he renders as viewing the individual as fully plastic, and therefore, as not able to grasp the politics or a meaningful history of emotions. According to him emotions and emotional expression interact in a dynamic way, with varied outcomes.

“repressive transformation” (Reddy 1997, p. 333) are induced and also molded by certain discourses, sociopolitical structures, and hierarchies (Lutz and Abu-Lughod 1990; Heatherington and Zerilli 2016; Handelman 2007; Denich 1980). While it seems self-evident that social anthropologists have to critically reflect on their positionality in the field in order to be able to establish ethical and unambiguous relationships, in academic interactions this is far from being the norm.

Conclusion: Becoming Politically Reflexive Practitioners

Emotions are epistemologically valuable ethnographic material. In social anthropological analyses, they are equally as important as field notes or transcripts of informal interviews. I have illustrated how reading about, discussing, and practicing emotional reflexivity in ethnographic fieldwork training contributes to an enhanced understanding of ourselves and the people we encounter in the field. Becoming aware of our emotions in a university teaching environment and decoding their meaning helps us to become more sensitive toward our students and to address emphatically their concerns, fears, and insecurities. Moreover, we can overcome “anthropology’s blind spot” (Wright 2004, p. 48) by bringing sociopolitical reflexivity from the anthropological field into the classroom, in order to critically evaluate our teaching.

Critically reflexive teaching methods can challenge the rational–bureaucratic logic that renders the personal sphere and emotions as “private,” and therefore abolishes the “personal self” from “official” spheres like the classroom. Particularly young scholars have recently made a vehement call for an inclusion of emotional reflexivity in social anthropology and academic teaching (Curran 2010; Heaton Shresta 2010). This book itself is an endeavor in this direction. Besides the students who plan to pursue doctoral research and carry out long-term fieldwork, the many students leaving academia to work in other professions (e.g., in pedagogic, therapeutic, or health-related professions) can equally benefit from emotional awareness and self-reflexivity (Dauber and Zwiebel 2006). Only if we support our students in becoming “politically reflexive practitioners” will they be able to critically reflect on, engage with, and transform institutional and policy contexts.

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Fieldwork Emotions: Embedded Across Cultures, Shared, Repressed, or Subconscious



Judith Okely

Binary Oppositions and Wider Context

Anthropological participant observation should confront binary oppositions and extreme dichotomies. Western intellectual traditions have illuminated dichotomies in definitions, especially Descartes' Mind/Body division. The disembodied "I think therefore I am" prioritizes the intellect over other aspects of human experience such as the senses. Nevertheless, another French intellectual, Marcel Mauss, centuries later, published an outstanding essay on *Les Techniques du Corps* (1936). He explored the multiple ways in which bodily movement differs according to culture. Internal cultural differences are also affected by age and gender, which he did not confront at that pioneering stage. Notwithstanding, Mauss is relevant to the practice of anthropological fieldwork across cultures. The anthropologist learns not only with her/his mind, but crucially, in combination with the body (Okely 2007).

Subconscious Emotions

In the same era as Mauss, Freud (1954) explored the hitherto less researched subconscious core of human experience. He pioneered theories regarding the long-term influence exerted by infancy and childhood on an individual's emergent personality and life trajectory. Here emotions, molded through early relationships, general

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socialization, and various events—whether predictable or unexpected—are crucial ingredients both as cause and consequence. Their impact and resonance would be explored through psychoanalysis, a format involving one-to-one hourly encounters between professional and analysand. In this process, there is full recognition and exploration of the subconscious or even unconscious. The analysand is encouraged to free associate, to voice rather than repress any stray remarks, and recount narratives, alongside seemingly irrational dreams or nightmares. Contrary to any dominant hegemony of rationality and ideals of self-control, letting go releases core insights, new understandings, and more complete knowledge.

Embracing psychoanalysis and its process exposes repressed, almost unknown experiences, whether trauma or forgotten joy—all lived or hidden emotions. Hitherto, surface reasonings have taken control of emotions, the latter too often dismissed as irrational and to be suppressed. The practice involved an asymmetric exchange where the “patient” free associates and the analyst offers interpretations. Eventually, the outsider professional has to make sense for and with the patient, who ideally can eventually confront the past and be transformed. Psychoanalysis of the Freudian genre privileged the subconscious, which the process aims to make conscious. All this entails extensive narrative and seemingly disconnected images, recollections, outbursts or even denials, and unexpected unburdening of long-hidden horrors and trauma.

In the social sciences there has, by contrast, been a long-held history of positivism, namely, an imagined imitation of science where the researcher/scientist is detached and disengaged, apparently in order to ensure that they have no influence on the object(s) of study. Such detachment has been transposed to what some claim is the core of social research. Additionally, the quantitative has increasingly taken precedent over the qualitative, seemingly in response to governmental policy demands and the strictures of neoliberal economies. Here voters, citizens, and consumers are judged and valued as mere samples and numbers, the more the better for capturing profit for those with power and control. This fetishization has occasionally been exposed as misleading: for example, the opinion polls which failed to predict the Conservatives winning the 2015 UK General Election and now the 2016 Brexit referendum. As Edmund Leach (1967) convincingly argued, intensive fieldwork in one location, through extended time, can reveal the *system* rather than superficial mass surveys conducted in multiple locations. The questionnaire based on a passing encounter between strangers may, as Leach reveals, merely repeat the inbuilt errors of both question and answer, disguised by intoxicating numbers. By contrast, intensive fieldwork involves day-to-day, in-depth interaction in every possible context: verbal, bodily, group, or personal interchange among a wide range of connected persons. Inevitably, emotions emerge and become entangled, bringing creative consequences.

Given the current regrettable personal distancing in a sociology that forgets the celebrated Chicago School of Sociology which first invented the term “participant observation,” there are bizarre responses in some contemporary UK sociologists. Strangely, emotional rapport in one-to-one dialogue can be seen by some as mere distraction. One recent example will suffice. Invited by another university to join a

PhD committee consisting of social scientists, I was the only anthropologist. The postgraduate, named Rachel, with outstanding degrees in social anthropology, was researching identity and domestic history among aged women. Rachel suggested exploring, during semi-structured interviews, the emotional significance of personal objects in the home space to each individual, as a route to reconstructing past experience and values. Immediately, one of the quantitative sociologists declared, "That's just psychobabble!" Thus, emotions and their analysis were caricatured and triumphantly trashed.

Then the postgraduate, whose first language was not English, in a highly original retort, suggested that treasured objects around the home were "good to think with." Thus, she creatively resonated with Lévi-Strauss on Totemism (1958/1962), where he brilliantly explains that animals are "good to think with." But the social scientists declared that the anthropologist should correct her English grammar. No matter that she was citing the English translation by a distinguished Oxford anthropologist.

The Personal as Political, Theoretical, and Revelation of Emotions

There has also been a deletion of the use of the first person, again in the name of science. Some decades ago, I proposed the theme of autobiography for the Association of Social Anthropologists' (ASA) bi-Annual conference. I had been inspired by themes emerging from the now much-cited *Writing Culture* (Clifford and Marcus 1986). One article (Rosaldo 1986) had highlighted and analyzed the appearance of the word "I" in the text. It was not inevitably autobiographical, nor self-revelatory; instead, the article argued that the word "I" was merely there to assert authority at key junctures. It informed the reader that *only* the anthropologist was there as fieldworker witness, in contrast to the reader who was *not there* (ibid, p. 90). The individual presence, seemingly, added scientific proof of presence and in-depth fieldwork. This analysis triggered reconsideration of the varying uses of the first person in publications. My co-convenor, Helen Callaway, and I had to argue the case at the preceding conference. We insisted that the gender, age, ethnicity, and personality had huge influence on the specific insiders who welcomed the outsider (see 'Doc' in Whyte 1955), the anthropologist's varied responses, choices and in-depth interpretations. Shockingly, opponents of our theme argued this was "navel gazing," "narcissism," and "California speak." But, we won the vote by show of hands, thrilled that "elders" such as Leach and Raymond Firth supported us. The ensuing volume, *Anthropology and Autobiography* (Okely and Callaway 1992), triumphantly emerged from that huge controversy. Some now downgrade so-called reflexivity as under-investigating "the researcher's *states of being*" (Davies 2010, p. 1). Yet, we were reinserting the fieldworker's specificity, including his/her emotions.

Already, in the mid-1980s, I had controversially used personal experience, both my own and others, as examples of changing historical positionality in a book on Simone de Beauvoir (Okely 1986). There I confronted emotions in the acts of reading, writing, and choosing a subject. This publication did not directly involve fieldwork. But the past leaves ethnography. Retrospectively, I recognize that the project was indeed integrating personal, lived, and emotional experience with what initially seemed a detached re-reading of a celebrated feminist. By the mid-1970s, the Women's Liberation Movement had rejuvenated feminism with the rallying cry: "The personal is political."¹ Later, I was to argue in the ASA volume (ibid. 1992, p. 9) that "The personal is theoretical." The specific ethnography from the fieldworker's experiences stimulates emergent theories.

Signing a contract for Virago Pioneers, a series celebrating key women authors, I had initially thought an opening chapter on the historical context of the writing of de Beauvoir's (1949) *Le Deuxieme Sexe* would be apt. Soon I recognized that, my being no historian, this chapter would take months of amateur research. The challenge was solved when re-reading my French Gallimard copy, first read when studying at the Sorbonne, Paris. I was transformed by the text: having escaped a prison-like English boarding school where intellectual quests and achievement were discouraged, let alone feminism, and where most emotions, not just personal grief, but also masculinized rage and anger were to be repressed (ibid. 1996, Chaps. 7 and 8).

Here are the emotional and indeed personal ingredients to my de Beauvoir writing. The original French text, read by this English teenager in the 1960s, had been a much treasured object, among other key books, for example, by Breton, Camus, and Sartre. The pages were marked by excited (yes, emotional), ink scribbles and key underlinings from two decades back. Later, in the mid-1980s, now an anthropologist and Essex University lecturer, this author began preparing the Virago publication. In the aftermath of the impact of 1970s feminism on gendered research and cross-disciplinary texts, there was a dramatic contrast between my once naïve teenage comments and what most women of multiple disciplines would take for granted by the mid-1980s. Fancy, in the early 1960s, the then young student had felt it illuminating to underline and appreciatively tick de Beauvoir's assertion that women were of *equal* intelligence as men. My school never taught me that. The teenage ink marks, exclamations and comments in the margins were a record of past emotional, as well as intellectual enlightenment. I had gradually unlearned the hegemony of the culture of the "stiff-upper-lip" (see below), and the female subordination born of 9 years of boarding school indoctrination, disguised as privilege (Okely 1996, Chaps. 7 and 8).

For the proposed book, the strategy was to summarize key arguments by de Beauvoir and then juxtapose them with early readings and now taken-for-granted

¹The phrase was popularized by Carol Hanisch's 1969 essay title "The Personal is Political" in Firestone and Koedt (1970), although Hanisch disavows authorship of the phrase. Others have also declined authorship but cite millions of women as the collective authors.

assumptions. Additionally and crucially, past personal readings were inserted from as wide a range of *other* women's perspectives, concerning the book's impact on them and the historical context of their textual readings of de Beauvoir. These included my Bengali, mature doctoral student who read it far from Europe and an English postgraduate of white working class origins. Women of varied ethnicities and ages, all celebrated the impact of de Beauvoir, often from very different cultural perspectives, linked to specific biographies. Here then were examples of what, sadly in 2014, the university social scientist mentioned above might have dismissed as "psychobabble."

Alternatively, the approach was legitimated, but misinterpreted as "reader response theory." Any critique of this alleged perspective is valid, because this author *never* claimed there is NO inherent meaning in a text, but instead, that its interpretation *varies* according to cultural, gendered, and historical perspective. By the 1980s, in contrast to understanding the world as recent school leaver at the Sorbonne, now having acquired an Oxford BA and doctorate, this author no longer needed reassurance that women's intelligence was *not* inferior to that of men.

Strangely, the American publishers, Pantheon, demanded from Virago, the UK publishers, that all such personal interjections were to be deleted because Okely was "not a celebrity." I refused, arguing that the personal was an historic case study for many others. Pantheon did succeed in excluding the photograph of de Beauvoir buying a daily newspaper from a working class Parisienne. I suggested this was one of the rare instances when de Beauvoir moved beyond the bourgeois elite. Pantheon insisted: "In America we don't do class." Central to my analysis of *Le Deuxieme Sexe* (1949) was that its apparently powerful universalism actually drew on submerged autobiography, confirmed in de Beauvoir's *Memoires* (1958). Pantheon excluded all US publicity. But the Virago edition, appearing, dramatically, the week de Beauvoir died, became an Oxford best seller.²

The Range of Emotions

Thanks to the invitation to the 2015 Berlin workshop "The Researchers' Affects" (see chapter "[Introduction: Affective Dimensions of Fieldwork and Ethnography](#)", this volume) I had to consider in depth the full range of emotions in fieldwork and academe. In social anthropology, we are forever confronted by the tension between the universal and the culturally specific, especially concerning the acculturation of the body. As Mauss (1936) noted, there are different ways of learning to act in and through bodily movement. Some become ingrained from childhood. There are differences across cultures. It is therefore no surprise that expressions of emotion will vary across culture, and within cultures across gender and age. One of my students,

²At the 2016 AAA conference, a leading anthropologist, of Indian descent, confided how influential my Virago publication had been to her.

Roger, when an undergraduate at Durham University, was brilliantly astute in acknowledging the cultural effects of gender on biology in a tutorial for my course “Race and Gender.” When discussing the classic difference between sex as biological category and gender as cultural (Oakley 1972), he reversed the dichotomy in a highly original way, declaring: “I have tear ducts in my eyes. But, after being sent to an all male boarding school, I have not used them since I was aged 11.” At such elite British boarding schools, boys were ordered not to cry. Here macho culture overrode “natural” biology. Emotions of grief, sadness and pain, perceived as signs of weakness, were banned. There was no equivalent censorship of aggression and properly directed anger in such places. The psychoanalyst, Schaverien, has now published her long-term study of this “stiff-upper-lip culture,” namely *Boarding School Syndrome: The Psychological Trauma of the “Privileged” Child* (2015). When Roger produced such insights, he was 19 years old. He is now an Oxford Professor and soon to be head of a prestigious college. Thirty years later, I reminded him of his insights. He exclaimed: “Did I say that? Goodness I was brilliant. It’s been downhill ever since.” Here we have insights into British ideals of masculinity. Men, let alone boys, are stigmatized as weak if they cry, revealing emotion. Seemingly, they will also be stigmatized as like girls, for whom tearful emotions are reserved. No matter both boys and girls have tear ducts.

Emotions in Fieldwork

Here is a partial range of emotions: anger, aggression, envy, jealousy, fear, terror, hate, disgust, boredom, joy, love, grief, sadness, remorse, surprise, interest, awe, admiration, guilt, trust, and vigilance. I have selected some from a Wikipedia chart. There are others especially relevant for anthropological fieldwork; for example, there are key emotions linked to cross-cultural choice: curiosity, longing for the elsewhere. This may be a fantasy of escape and the search for difference. There are also emotions linked to political engagement; something I learned from Sartre and de Beauvoir.

Another complex of emotions in fieldwork is giving, as part of reciprocity. Here we are back with Mauss, but this time with his brilliant book *The Gift* (1925/1954) revealing an obligation to give, to receive and to return or reciprocate. Again, I argue there are specific obligations in anthropological fieldwork. We depend on our hosts’ welcome and hospitality. Eventually, we are not there just to take but also to return. We can give back in different ways (see section “[Reciprocity in Research Relationships](#),” this volume). The acts of reciprocity on either side are driven by emotions, both at the start and completion. Sometimes that continues for years, even decades.

One example draws on my own fieldwork, elaborated in an article in press where I explore the key individuals who befriended and guided me through the inevitable

mistakes anyone makes as outsider entering another culture. They were all literate, in contrast to the majority of the community of Traveller-Gypsies³ with whom I lived. They were two men and two women who had lived the other side of ethnicity, having spent time in houses and in school. One, whom I here call Geoff, had been wonderful company in extended discussions. Here was a fellow intellectual full of wit, detailed observations and insights into the traveling way of life. Sometime after fieldwork, he was

³The author explicates on the histories and politics of designations in Okely and Houtman (2011, pp. 24–25): “Different groups choose different self-ascriptions, and preferred titles change over time as labels become stigmatized. Fredrik Barth (1969) pioneered the emphasis on self-ascription among ethnic groups. “Traveller,” used by the Scottish and Irish, has replaced the once neutral descriptive label “Tinker.” The latter term originally referred to skills in improvised metal work, but hostile use by the dominant society led to its abandonment (Helleiner 2000). “Gypsy” is a shortening of “Egyptian,” long-used from the fifteenth century onwards to denote any so-called foreigners. It was once a capital offense to be a Gypsy. Nevertheless, Gypsies in England and Wales have embraced the title. An alternative is “Romany.” In hegemonic media discourse, “Romany” has been used as a means of constructing “real” Gypsies, as distinct from alleged “half-castes,” or “drop-outs” (see Acton 1974). When engaging with racist outsiders, Gypsies may prefer to neutralize their identity by calling themselves Travellers. Another current term is “Roma.” This label’s increased profile is connected in part to the practice, widespread in the former communist countries of the EU, of appointing state-salaried ethnic “leaders,” who were often in effect professional lobbyists, and frequently considered unrepresentative—and sometimes ritually polluted—by the groups they supposedly stood for (Kaminski 1980). This form of representation stands in contrast to the decentralized experience and political strategies of Gypsies in other countries, especially the UK. It was through such figures that the label “Roma” gained its wide currency. In 2004, the EU, apparently ignorant of these multiple histories and preferred terminologies, deferred to those who saw themselves as representatives of Roma in the ex-communist countries, who lobbied for the banning of the label “Gypsy.” In so doing, the EU showed its apparent ignorance of the existence of the long-established British Gypsy Council (Acton 1974). The label “Roma,” although certainly embraced by many, is not universal in the formerly communist-ruled parts of Europe. Czech anthropologists (Budilová and Jakoubek 2009) confirm that Slovak Gypsies refuse to call themselves Roma, because the high-profile “leaders” who embrace the term do not represent their interests. Furthermore, the use of “Roma” can emphasize a mono Indian origin and an unadulterated Hindu culture, at the expense of recognizing the variegated histories and adaptations that have emerged over centuries. This implicit privileging of a single territory of origin is consistent with the Stalin-era policy of affording greater recognition to minorities with claims to specific territorial connections. The pre-Sanskrit linguistic connections that can be discerned in the many forms of Romanes meant that for many communist-governed Roma, it made sense to designate India as their original territory. The single Indian origin is reiterated by a British-born linguist (Hancock 2002). These issues have been fully debated elsewhere (Willems 1997; Liebich 2007; Jacobs and Ries 2008). My early questioning of the narrative of a single origin for Gypsy culture was generally taken to be anthropologically convincing (Okely 1983, pp. 1–27). Regarding the question of “ethnicity,” for each of these minorities, the principle of descent is crucial. This principle has gradually replaced the problematic notion of biological “race” in political and legal debate. When the Irish Travellers were recognized as an ethnic minority, followed by the Scottish Travellers in 2008, it was people’s shared history and shared ancestry that was felt to be crucial. The question of Indian origin was not deemed relevant. Scottish Traveller Colin Clark and I provided expert witness for the recognition of Scottish Travellers (see also Clark and Greenfields 2006). As recently as the 1990s, only the English Gypsies were legally recognized as an ethnic group in the UK, on the grounds that they had originated in India and were therefore a distinct “race.” All these groups distinguish themselves from nonmembers. Roma and Gypsies call outsiders “gaje.” The Irish Travellers call non-Travellers “country people.” The Scottish Travellers use the label “flatties.”

wrongly charged with kidnap and attempted murder. A subtext was that he was closet gay and avoided by other Traveller-Gypsies who then stigmatized non-heterosexuality.

I visited Geoff in Wandsworth Prison, although the director at my research center, a seconded civil servant, insisted I should be working in the office during afternoons. Here emotion intruded. I declared that the Traveller-Gypsies had given us so much, the least I could do was to reciprocate. Emotions of defiance overtook me in confrontation with power. My boss demanded I take the “free afternoon” out of my limited annual leave.

I was Geoff’s only visitor. Later, he described how much the visit meant to him. An outsider found him a solicitor who picked up my cultural capital as Oxford graduate, with ‘posh’ accent. I acted as a character witness in the Central Court of the land. It helped. Geoff was found not guilty of most charges. Here is a wonderful memory, full of multiple emotions. It also challenges the lie that anthropologists only use the ‘other’ for selfish gain.

Emotions Among Varied Anthropologists

Thanks to Thomas Stodulka, who must have responded to the latent emotions in my *Anthropological Practice: Fieldwork and the Ethnographic Method* (2012b), I was inspired to re-examine various incidents in terms of emotions in the field. I recorded dialogues with some 20 anthropologists of 16 nationalities who had done fieldwork around the globe, some as long ago as the late 1960s, others in recent years. Unpredicted were the emergent commonalities. At the outset, partly due to my opening broad questions, I was focusing on seemingly more grounded aspects of fieldwork practice and a less formulaic means of understanding fieldwork. I regret not having asked whether and in what way they were changed by the experience.

In this article, I have brought to the fore the implicit emotional aspects of key incidents, encounters or the very choice of locality and topic. Gradually, I recognized some key aspects linked to crossing cultures. There are emotional drives which push us to seek the elsewhere, the different, beyond what we may have been socialized to see as normal. It may be, by contrast, that the search for the elsewhere is a conscious or subconscious reconnection with some lost, past experience or even fantasy. A “longing for the elsewhere” extends to choice of location or topic.

The Elsewhere: Emotional Drive

Akira Okazaki, like others, was driven to the elsewhere, in his case from his home country of Japan to Africa and then to nonliterate communities, namely the Maasai. But once in place, he changed to something very different:

The first time I went to Africa I wasn’t an anthropologist. I was there as a hitchhiker and a traveller. I found myself living among the Maasai. I started to be attracted by their way of life, centered on age groups. But I didn’t have any way of knowing or understanding their

way of life. (...) I had been studying French symbolist poems, critique, and philosophy, always dealing with what is writing, like in France at the time, the way we were talking “*Qu’est que c’est l’écriture?*” (What is writing?) This question was common.

Okazaki had believed that people’s perception and ways of experiencing things might depend on whether or not people have a written form of language.

I was interested in Africa because I thought that some people there are not disturbed by that written form. My interest was in how they could see the world, landscape and other people without being disturbed by *écriture* (writing).

But after I arrived, and several months living among the Maasai, I completely forgot the initial reason of going to Africa. I found something totally different and another new question coming from that experience. That is, how can I understand?

In French poetry, people are talking about what is truth in poems, I was a master in that discourse, unable to think about any alternative way of looking at the world.

Many times I returned to Maasai land. From 1981 I decided to find another place for my fieldwork, to try new ways of learning the stimulation from a new world. I was unemployed for 10 years or even 20 years, doing fieldwork with my own money, gained by manual labour, because I wanted to be among the Africans and because I can learn and get interesting things. I found one community in the Gamk area.

Here, in his own words, is evidence of Okazaki’s continuing passion in pursuing difference and its understanding. He self-financed his fieldwork through the years and changed the people and locality, but always far from Japan. Among a community of Gamk, on first arrival, he was treated as outsider with seeming advantages, but later, he discovered disadvantages. He did not realize that he was perpetually treated as a foreigner in a culture used to welcoming outsiders, but kept at a distance. Then, unexpectedly, he returned to the same community after a long absence through illness. The people were impressed he had chosen to return and invited him to join them, albeit to build his own house. Crucially, his hosts were emotionally touched by this Japanese outsider’s commitment, respect, and drive. His curiosity was always satisfied with the unexpected. Akira was captivated by the fact that he could not predict anything happening in this culture:

During my fieldwork always there is something new. Amazing things happen. Not in terms of spectacular things. Just tiny things like the way a ritual is so different from what I know - some new word which they use when they are doing rituals.

Emotional Nostalgia: The Past Revisited

Another emotion involves the opposite to the unknown afar: not searching for the new and unpredictable, as Okazaki did, but instead for a known but lost past. H el ene Neveu Kringelbach was drawn to a fieldwork site initially by her childhood memories, a Proustian *  la recherche du temps perdu* (1913–1927). Franco-Senegalese, she returned to Senegal, where she had spent her childhood with her Senegalese father and French mother. After parental separation, she was brought up in France with her mother. She reconstructed that highly charged past, which resonated with Proust’s emphasis on bodily triggers, in his now famous case of the taste of a

biscuit. For H el ene, here also is the link with experiences which reconstruct emotions of innocent happiness:

I'd been to Dakar about eight times as a child. I found myself gliding into the place much easier than I would have done otherwise. I remembered the feel of the city, the sounds and smells. It helped me getting started(...) I could still find my way around a little bit. The bulk of my memories were from the age of five playing in the street with other children, going to the beach and smelling the rotten fish. All these things were still there. Of course, the city had expanded(...)It had changed. But some of the sensory impressions were still the same.

Escaping Association with Violence

Another choice of field location was also governed by the anthropologist's ethnicity and earlier formative experiences, but again in different ways from the ones above. Paul Clough, with an American father, an English mother and American citizenship, had lived through childhood in militarily defined locations, because his father had been in the US army. Electing to avoid any country with a US military connection, he chose Nigeria; far from Vietnam, Cambodia or Korea, marked by US warfare. Less significant for him, personally, was the British legacy of colonialism in Nigeria, given his Americanized upbringing:

It was all pure idealism. I was living in Malta. I had a Peace Corps application. They wanted to send me to Melanesia. I didn't want to go because it looked like I was moving into an American colonial situation – American protected territories. I wanted something genuinely different. Out of idealism, I started looking for jobs in Africa. There was an American Jesuit priest at a Maltese school where I taught who said, "Write to the Vice Chancellor of Ahmadu Bello University." I sent him this letter saying I wanted to be a volunteer teacher. That's how I got to Nigeria – another strange accident.

Escaping Mono-Cultural Bodily Control

Felicia Hughes-Freeland was drawn to dance in Southeast Asia. It was an escape from conventions, especially her cultural bodily controls. There are parallels with the yearning for difference, as with Akira.

I let things happen. That's when things went well. I'd seen a lot of dancing. Everywhere I went there'd be some sort of dancing. It struck me that there was something going on (...) that seemed different from what I was used to. When I saw the court dancing in Yogyakarta, I thought it extraordinary and beautiful. I wondered how it kept going. I couldn't relate it to this highly materialistic society I'd been living in, in East Java, after becoming an anthropologist.

Hughes-Freeland reveals the then shockingly ethnocentric, if not racist, beliefs of the Voluntary Services Overseas (VSO) which:

made you come home at the end of your second year to stop you “going native.” They had this funny idea that, if you’d stayed longer than two years, you would be irredeemably lost.

After some years of acquaintance, Felicia and I discovered we had attended the same constricting boarding school in the Isle of Wight, albeit at different times (Okely 2012a). Predictably, we had suppressed an emotionally brutal past and hitherto never revealed it. She attended the school at a later age so was less vulnerable. But, inevitably, she found the island internment as ever shocking. After just a few terms’ attendance, she fled this “total institution” (Goffman 1968) which acted as both bodily constraint (Okely 1996, Chap. 7), and to popular incredulity, discouraged intellectual pursuit, let alone university entrance (ibid, Chap. 8). Here forms of socialization, however brief, may have emotional legacies of defiance, not necessarily nostalgia.

Anthropologists are often outsiders. They’ve had experiences in their childhood or they’ve lived in other countries. Something’s happened or they’ve come from outside. Often one doesn’t feel one belongs. So in the field experience, you passionately want to be accepted. Belonging is very important.

By contrast, that same boarding school had a different emotional legacy for another former inmate: Mimi Khalvati, of Iranian origin, was, like a number of other girls, sent by global elites to this outpost. Dispatched, at primary school age, Mimi did not revisit Iran, her home and family, for over a decade. Years later, now a celebrated poet, she returned to her former school grounds in search of the only continuity she could find in her childhood, the nearest thing to having a “home.” Here, as with H el ene Neveu Kringelbach, the return was made out of emotional nostalgia but, additionally in Mimi’s case, to make sense of a relatively brutal past where she came to realize she had been abandoned. Again, there are Proustian sensory resonances. Key buildings had been bulldozed, but her emergent book of poetry reconstructed the poignant attachments to remaining vegetation and landscape (Khalvati 2001). When I showed her select footage of my filmed return to the school, a few years earlier (Okely 2003), she begged for a copy. She could now visually revisit memories of the now bulldozed Chapel and Gymnasium, once places of joyful *communitas* (Turner 1969).

Paradoxically, although this time-warped institution was supposed to indoctrinate us into becoming “upper-class ladies” in an imagined English monoculture, this future anthropologist befriended girls of different cultures sent from around the world. These included girls from Iran, Pakistan, Malaysia, the Caribbean, Canada, Hong Kong, France, Sweden, and South America. We exchanged cross-cultural contrasts. I learned Persian phrases and enjoyed narratives of the Caspian Sea, all in contrast to our Alcatraz in the English Channel. Thus, like the varied anthropologists in *Anthropological Practice* (Okely 2012b), children, not just adult researchers, may be open to what intrigues them, especially contrasts with the familiar. Advance agendas, whether imposed by research committees or by authoritarian regimes for children, are invariably transformed or subverted.

Interpreting and Conveying the Unspoken

The long-term emotional impact of varied childhood experiences is again to be found in another anthropologist's future fieldwork and research. The examples of anthropologists I have mentioned thus far are of Japanese, Franco-Senegalese, Anglo American, and English descent. The next example is an anthropologist of Polish descent. Marek Kaminski was, as was the custom for families facing poverty under communism, out of necessity placed by his widowed mother in an orphanage. He had only infrequent visits from his mother. His father had been a distinguished war hero fighting fascism. But after the postwar communist take-over, he was "accidentally" killed when visiting a factory. Kaminski grew up to hate the regime that murdered his father. He explained how he learned that, as a child among many in the orphanage, he had to observe the passing adults to understand, then appeal to them. Learning to be unthreatening through body language, he became fearless when entering groups and localities where others might have been terrified by prior stereotypes. He elaborated on how bodily movement may convey inner feelings and attributed his approach to his experience as a once abandoned child:

We project patronizing, uncertain feelings with our movements. Because of my life experience, (I had to leave when I was five years old for the orphanage), I had in some way gathered non-verbal communication signals which I projected onto other people, because in some cases, I entered the most dangerous situation. I should have been knifed, or killed, or raped, or whatever, and nothing happened to me. I believe in some way we project that innocence, or ignorance sometimes. It could also be interpreted as the core of humanity – we trust that other person.

So my contact with Gypsies was always the same. I believe they could interpret my reaction as always friendly, as nothing hidden. Like a dog meeting the more powerful beast, they will lie on their back and show four legs. I think my entire social situation was of that dog with the four legs, so I was left un-attacked and allowed to stay.

I project without knowing it, because it was the same when I was in Greenland and meeting the guys with the guns when we were shooting, or in Lapland when I was left in the forest suddenly alone. Then the Laplanders brought me back to their things, and allowed me to meet criminal groups – always in some way this non-verbal communication. Somebody told me that dogs attack us because, when we are scared, we have some kind of smell, which they can feel. I believe there are social situations, it could be non-verbal, but the way we look, the way we approach the person, brings hostility or welcoming. Gypsies, so many times, are trained through generations to perceive the signals, not only Gypsies, but all marginal groups.

Kaminski attributed the trust, he elicited, to his deferential, visibly harmless, near vulnerable approach to strangers and stigmatized minorities. "Never, ever in my involvement with Gypsies has anyone stolen from me."

Kaminski's experience took a new turn when he escaped Poland as a refugee to Sweden in the early 1970s. Stateless for years, that position added to his emotional and general identification with other vulnerable persons and groups. This can extend to empathy with the cause of humanitarian justice against the hegemonic racism of the majority population, and enhance the engagement of the anthropologist with a persecuted minority.

Years earlier, Kaminski revealed a complete contrast with his self-description as deferential, when, before migrating, he stood up to the fearsomely powerful:

In Slovakia we were waiting for the bus and suddenly Slovak hooligans came and attacked the Gypsies. I was with them and waiting. It was an automatic response: "Why the hell are they beating these guys who are waiting for the bus?" That event brought me into their community. I took their side in this struggle. The area I was studying was not far from the Slovak/Polish border. There were districts where the majority were Gypsy inhabitants. You are waiting for the bus from this district town, as it was the only access to these Gypsy settlements. Then there came some drunkards, non-Gypsy Slovaks, who are, just for the sport, attacking waiting Gypsies.

Sometimes the Gypsies would defend themselves, but usually, they were extremely passive. They were beaten, but no reaction. I was from Poland, an outsider, and a privileged position, because in Eastern Europe a foreigner, even from a neighboring socialist country, was a privileged person, because the community couldn't travel easily. We didn't have passports, so being an outsider and taking their side and shouting at Slovaks in Polish, I was suddenly projecting that fight into some international affair at the local level. I was told that this was frequent. They were beaten at that time, all the time. We talk about Jewish pogroms in East Europe in the twentieth century. But I am talking about Slovakia in 1968/69/70 – regular pogroms against the Gypsies by non-Gypsies.

You had to have a residence permit to stay in one area. You had to have a work permit to move to another. Within Eastern Europe you had domestic divisions. The Gypsies had no chance, if they gained criminal records, through fighting back. The entire police force had no Gypsy who admitted to being a Gypsy. Whenever they were attacked and beaten up, they had no chance of defense. That I recognized.

Such 'pogroms' continued even after Slovakia joined the European Union. Meanwhile, a new generation of anthropologists is emerging with important studies of Gypsies and Roma in the Czech and Slovak Republic (Jakoubek and Budilová 2006; Budilová and Jakoubek 2009).

The individual may, in fearless protest, draw on his or her emotionally painful, transgressive biography, which becomes empathy toward the vulnerable. Consciously or subconsciously, the individual psyche is enhanced or even exploited in fieldwork encounters. The anthropologist may be both rebel and compliant—a 'critic at home and a conformist abroad' (Lévi-Strauss 1955/1973, p. 386).

The Passion for Difference

The anthropologist may not only seek the elsewhere or past nostalgia, but also difference, wherever encountered, both nearby or faraway. Okely, having been inspired by difference in the West of Ireland, when accompanying her then partner (2009), embraced an escape from a monoculture to anthropology, which celebrated the full range of human possibilities. By chance, after a postgraduate conversion course, she answered an advert for a research project on Gypsies and other Travellers. The successful appointment led to a continuing, multi-faceted engagement. Here was difference and ingenuity, in the very 'Home Counties' and on 'Gypsy Sites' sites, midway between Oxford and Cambridge (Okely 1987).

By contrast, as assistant to Evans-Pritchard, Malcolm McLeod became interested in witchcraft in Africa. An accident led him to Ghana and an Asante community. Although there was witchcraft, Malcolm responded passionately to what was different before his eyes, all without advance planning. He was captivated by the material culture: tremendous textiles, woodcarvings, and especially goldsmithery. Here was curiosity, openness, and spontaneous enthusiasm for difference in a feast of cultural artistry. This unanticipated response to material culture, rather than direct human interaction, led McLeod to be Curator at the Museum of Mankind at the British Museum and the Glasgow Hunterian Museum. Eventually, McLeod returned that gift of creativity by assisting in establishing a Museum in Ghana to celebrate Asante culture in their home place.

Suzette Heald, planning to examine bilingualism in Africa, had to change topic just before her departure. Her husband, accompanying her as teacher, was suddenly assigned a different locality where bilingualism was marginal, if nonexistent. But, spontaneously driven by immediate, albeit naïve, curiosity as to difference, she became immersed in the very opposite to her sheltered, middle-class Anglican upbringing, namely violence and the threat of murder:

I'd had a very middle-class, English upbringing in the fifties, where personal security was absolutely taken for granted. There were two events that focused me. One was coming across a party of men one morning armed with knobkerries and pangas. Knobkerries are sticks with rounded heads, and pangas are called machetes in other places. These men were quite heavily armed, and me saying: "Where are you off to and what are you doing?" and them saying: "We're on the tracks of a thief." Me: "What are you going to do with those implements?" "Oh we will kill him of course" – just absolutely straight. That shocked me. Then very soon afterwards, a witch was killed, an old man, fairly near where I was living. I was immediately challenged by this violent side of Gisu life.

Like Heald, anthropologists, such as Zulaika (1995) and Okely (2005), may not specifically have set out to study violence, but if its use and management are integral to the society or group, the anthropologist cannot ignore it. Violence may be the outcome of cool calculation and the repression of emotion. In other contexts, it may be intermingled with emotion, for example, individualistic anger and rage or group identity, fired by ethnic, nationalist, racist identity, and loyalty.

The Outsider Anthropologist as Therapist, Confidante, or Rival to Other Academics

In England, from my first week living on a site for Gypsies or Travellers was treated as an unwitting therapist. A young mother, 'Gemma,' still traumatized by the accidental death of one of her children in the previous year, invited this non-Traveller-Gypsy (*gorgio*) to join her for a chat in an abandoned van, every late evening. Gemma's husband and other children, like the other residents on the site, were asleep in their caravans. Gemma talked for hours, re-living the drama of her tragic loss. The outsider acted as supportive listener and silent therapist. The bereaved

mother projected her grief outwards, explaining how her neighbors no longer wanted to listen. The stranger anthropologist was honored to be so incorporated as co-resident.

Another emotional outlet for the site residents were the dramatic details of a feud triggered by jealousy, betrayal, and murder. Again, the outsider/incomer was treated as the listener of endless details surrounding the conflict and ensuing horror, resulting in a court case and prison sentence. In fact, the person who had technically caused the death was not exposed in court. The anthropologist was informed of the entire context and the name of the individual whom the two feuding parties had jointly agreed should be named as the guilty party. Details of this were never published in my 1983 monograph but concealed in the 1977 doctorate, banned for decades from public circulation.

The anthropologist did not fear about the knowledge circulation by the Traveller-Gypsies, but 'fellow' academics, driven by macho jealousy toward a young female invading 'their' patch. Within weeks of my moving onto the site, an established male linguist, ignorant of fieldwork methods, hearing of my existence on the activist circuit, asked what this female Oxford graduate looked like and her exact fieldwork site. He, Donald Kenrick, declared his intention to visit the locality and inform the residents that Okely was a 'government spy.' Ironically, in direct contradiction to this macho rival, the government had tried to block the research project, financed by the charitable Rowntree Memorial Trust, at an independent center. The project director, Barbara Adams had seconded herself from the Ministry because the latter had already attempted to censor public circulation of her relatively timid (sometimes ethnocentric) Census report, *Gypsies and Other Travellers* (M.H.L.G. 1967). It appeared 'too sympathetic' toward the persecuted, nomadic minority. But Adams had also judged that the ensuing 1968 Caravan Sites Act had not fully considered the wishes of traveling communities. Thus, the research project was proceeding *despite* government disapproval.

No matter that this insecure man's provocative plan could have been life-threatening to the *gorgio* researcher, given the previous feud, emerging from one member of the Traveller community reporting another Traveller-Gypsy to the police. Fortunately, the anthropologist was warned by a sympathetic social worker and avoided all activist London meetings attended by self-styled 'rival' *gorgios*. Years later, the same man, red with anger, at a university workshop, yelled at this anthropologist that her monograph should be burnt (Okely 1997a). Such is the brutal venom among some scholars protecting 'their' patch.

As for the murderous feud which the site residents individually detailed to me, according to each perspective, only after key parties were themselves deceased was the still disguised ethnography published. This was carefully placed in a journal rarely consulted by 'rival' specialists (ibid. 2005). Fortunately, younger international scholars, and now Traveller graduates, have expressed appreciation of the insights into alternative conflict resolution after an emotional catastrophe and murderous feud.

The Outsider Normalized by Unexpected Anger

Paul Clough's actions were misunderstood while measuring land as a favor for his host, something which aroused unanticipated complaints. His uncontrolled angry outburst aroused astonishing admiration. Suddenly this white man was seen to be like them:

'He is human:' I was trying to be a detached sociologist, to keep my emotions under control, to keep smiling even when my leg was being furiously pulled. I was not developing friendships with older men, apart from two. I was not in a family situation where I could get to know the wives.

Then something happened. I began mapping the farms of the village, measuring acres. One man said would I please come to measure his fields. I was very busy. I had some research plans. I said: "OK when and where do we meet? What field do we measure?"

I went to the field to measure with my tape. There he was farming with his mother and she began grumbling in a way I'd heard before. She said to me in Hausa: "You are here to exploit us. You are here to eat our villages." It's a well-known expression in other African languages. This 'eat' idea is popular in West Africa, a word for exploitation. She said: "You are eating us. You are going to go back to England to make money out of your notebooks." I'd heard this before. I was fatigued. Since her son had called me to do this for his benefit, I blew my top. I lost control. I started shouting: "Damn you, I don't want to be out in this bloody field", all in Hausa. "I only came because your son invited me. I'm happy to leave this bloody field this moment."

Their mouths dropped. For the first time, after a year, they had seen this white man show emotion. He was a human being. From that moment she became my firm friend.

Here, superbly, the loss of control and emotional outburst, all vividly expressed in the local language, was totally appreciated. If Clough, the white man, had merely sworn in English, his bodily image might have confirmed his research subjects' initial stereotyping of the outsider, stranger. As his opening lines reveal here, he had believed that the control of emotions was a fieldwork requirement.

The Outsider May Trigger Fear of Competition, or be Invented as Scapegoat

For H el ene Neveu Kringsbach, arriving as a mother proved not to be a technical handicap, as some university supervisors have presumed, (see Morris in Okely 2012b, p. 38), but evidence of commitment and shared humanity. Moreover, another unexpected advantage for mutual trust in Dakar, with a practice of polygamy, was being a married mother:

Having a child with me made me much less susceptible to being a competitor to other married women. There is huge competition amongst women, given that there is polygamy. There is always a threat of a second or third wife coming. When you come as a single woman you're very likely to be perceived as a potential competitor.

After becoming well established as a co-resident on a Traveller-Gypsy site, this author was exploited as outsider-object to resolve an internal problem. However, the

anthropologist resolved it through a comic riposte. Returning one day from visiting my partner, a philosopher at Kings College, Cambridge, I confided in a *gorgio* resident that I was suffering from stomach upset after a huge college banquet. But the *gorgio* lady, whose marital fidelity was under scrutiny while her husband was in prison, mischievously spread a false, distracting rumor that I was suffering from ‘early morning sickness.’ Soon the story was embellished. Apparently, I was pregnant by a married resident of the Traveller community, dubbed a womanizer. This was the Traveller-Gypsies’ way of getting at him via a *gorgio* target, without offending any Traveller woman. The outsider was the ideal object of projection, as non-Traveller women were deemed promiscuous: ‘They go behind the hedge with anyone.’ The narrative spread to other sites. My neighbors joked they would move the man’s clothes into my caravan.

The womanizer’s wife, who had been my close friend, no longer saw the joke. Finally, waiting until a number of residents had gathered in or near the alleged womanizer’s trailer/caravan, I grabbed a doll from the collection of toys given by a charity to the ‘poor Gypsy children.’ Swaddling it in a blanket, I joined the crowd, proclaiming I had just given birth to this ‘baby’ and asked for paternal support from the ‘father,’ the alleged womanizer. Everyone shrieked with laughter. The rumor disintegrated. My co-residents declared: “You can muck in!” Thus shared humor across boundaries can override stress and projected disapproval. Meanwhile, the youngest child of the ‘father’ seized the doll, as perceived rival, and smashed it to pieces, enacting sibling jealousy in dramatic form. The original article (Okely 2005) was part of a *Festschrift* for Ronnie Frankenberg. He in turn composed a thank-you poem about a broken doll.

Unconscious Emotions Driving Analysis and Interpretation

Just as social anthropology avoids formalized number-crunching and arguments dependent solely on quantification, so its alternative analysis draws on material gathered from intensive fieldwork in limited localities. The anthropologist is involved at every stage: from initial research designer to fieldworker, analyst and author. This contrasts with an often scientific format where the data is gathered via questionnaires, administered by multiple ‘slave’ assistants. Subject to managerial, top-down control, they are devoid of all influence, whether in the initial research questions or in the writing up. The final publication may be credited alone to the grant holder (see Okely 2015).

The anthropologist, by contrast, if multi-engaged and open to an holistic approach, may focus on themes never predicted before entering the field. Here the example from my classically organized monograph. *The Traveller-Gypsies* (Okely 1983) also hides a theme, driven by personal emotions and childhood trauma. Granted, I had been congratulated on the pioneering methods chapter. This was partly explained by a long struggle in insisting on the method of participant observation, whereas my initial director had privileged mass questionnaires.



Fig. 1 Fathers and daughters. Photo: Judith Okely (1983)

Additionally, people have reacted with continuing astonishment that I lived with a community stereotyped and demonized. The chapter answered some of the outsiders' near racist astonishment.

However, at the time of writing, I thought the final chapter, 'Ghosts and *Gorgios*,' was seen as routine for a monograph. Later, the French anthropologist, Patrick Williams, lauded it as one of the few concerned with mortuary rituals among Traveller minorities throughout Europe. He had then been inspired to complete an entire monograph on French Manouche mortuary rituals (2003), succeeding his earlier publications on this traveling minority. In turn, I questioned my focus. Gradually, I recognized the unconscious, psychoanalytical drive.

The photograph labeled 'Fathers and Daughters,' (Okely 1983, p. 160) which I had lovingly selected from many, reveals two men and three little girls whom I had done research with. I knew them as neighbors, but never revealed any identities. These were photographs by professionals with the subjects' consent (Fig. 1).

Privately, I knew the seated father was dying of a brain tumor. Only recently, I noticed the daughter's foot poignantly placed on his. She knew of his illness. He did indeed die. The last image of the monograph (*ibid.* p. 221) is his burial. Children scatter earth into his grave. I attended that funeral and was recorded in the crowd.

Here this chapter's early section on subconscious emotions is most apt, and links up with the anthropologist Williams' response to my work. But only decades later, did I experience the Eureka moment. I had unconsciously identified with that little girl. I also lost my father at a similar age, but was wrongly told: "Daddy has flu." Actually, he was paralyzed from the neck down with polio, encased in an iron lung. In crisis, my sister and I were dispatched to boarding school, far from what seems

an idyllic childhood in rural Lincolnshire.⁴ Months later, our mother appeared, dressed entirely in black, wearing Daddy's signet ring. Just before leaving, she found the courage to tell us that God had taken Daddy to heaven. In bewilderment, we returned to the dormitory. The matron asked me why I was crying. I explained my daddy had died. She replied: "I know. But you are to *stop* crying *at once*. It's against the rules to make a noise in the dormitory."

Here we see the dramatic and brutal contrast between a British 'stiff-upper-lip,' anti-emotional culture and that prevalent among Traveller-Gypsies, who are themselves the victims of emotive, racist persecution. The children I lived alongside were fully 'educated' in their way of life, but had never been 'schooled' in authoritarian institutions (ibid. 1997b). That little girl, consoling her fragile father, was never 'sheltered' from tragedy. The children co-experienced, alongside adults, both the banal and melodramatic; they were allowed to release emotions, not forced to repress them as in 'privileged' institutions, which even separate children for months from parents. Traveller-Gypsy children can bid farewell to the dead displayed in open coffins. They participate in funerals, sharing grief among kin. Of all this, my sister and I were deprived. We never knew our father was dying, nor the day of his death, let alone being able to attend his funeral. On being suddenly informed, weeks after our tragic loss, we were forbidden from expressing the slightest feeling.

The ultimate political revelation of that dominant culture is the contrast between the personal and public, state hegemony. Many months after our loss, the teacher ran screaming into our classroom and tearing her hair. We were terrified. She repeatedly shrieked: "The King has died!" Expected to enact mass hysteria, we covered our faces, some smothering giggles. Days of mourning ensued, with special Bible readings in the Chapel. Girls had to demonstrate sacred reverence to the deceased Head of Empire and Anglican faith. Only then was emotion demanded: public and performed, but never personalized for lost loved ones, most close.

Thus, decades later, the externally lauded ethnography and analysis of mortuary rites among Traveller-Gypsies can be explained in finer detail. My attendance at the young father's funeral and the choice of that photograph subconsciously expressed and resolved comparisons and contrasts with the anthropologist's brutal childhood. Aspects of the closing chapter, especially its images, can be interpreted as wishful thinking; something psychoanalysts recognize in dreams.

To Conclude

Individual biography, even trauma, may find emotional resolution elsewhere through participating and finding empathy in other cultures. It has been argued that the personal is both political and theoretical while social science has struggled to retain the detached observer. The alternative is too threatening. This explains both the furore

⁴My father lectured in German and French at Royal Air Force Cranwell. My parents first met in Berlin as fellow Anglos. This chapter celebrates that emotional connection.

when the ASA conference theme was proposed and the publisher's resistance to auto/ethnography in re-reading de Beauvoir. In Europe, if not beyond, binary oppositions have included male/female divisions: transposed as reason versus emotion. The profoundest fear is that 'The Personal is Emotional.' Yet, this need not contradict the political and theoretical. All are intertwined.

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Afterword: A Return to the Story



Paul Stoller

Kumba hinka ga charotarey nyumey
It takes two hands to nourish a friendship
Songhay Proverb

Several years ago, I returned to the Republic of Niger after a long hiatus. Indeed, for a long period of time, I had concentrated my ethnographic efforts on writing about West African immigrants in New York City and had put my research in Niger on the back burner. When I returned to Niamey in February 2009, I did so with great expectation. I planned to visit the gravesite of my mentor, Jean Rouch and wanted to begin research on how the introduction of digital technologies had altered urban and rural social life in Western Niger.

February in Niger can be blisteringly hot. For me the searing heat of Niamey had never compelled me to take taxis or rent a vehicle. I had always preferred to walk the dusty traffic-clogged streets of the capital city. On my 2009 trip I ventured out in the early morning and late afternoon, stopping to chat with street merchants or to visit friends at the University of Niamey's Institute de Recherches en Sciences Humaines. One late afternoon as I was debating American politics with a group of people just outside a cell phone charging station, an older man waved at me as he ran across the street. He came up close and pointed his finger in the air as if he was trying to remember something.

"Aren't you that white man who speaks Songhay with a northern accent?"

Onlookers inched closer to hear a potentially interesting conversation.

"I do speak Songhay with a Gao (northern) accent," I admitted.

"Then it is you!"

I looked perplexed.

"Don't you remember me?"

I didn't recognize the man.

"Didn't you used to sit with Diop?"

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"I did." Diop, a Senegalese man from Dakar, owned a small market stall from which he sold African art. "Yes, we used to talk for hours. We were good friends," I said, "but I haven't heard from him in a long time. How is he?"

"I am sad to say that Diop died several years ago."

"I am so sorry to hear that."

The man smiled. "You know what I remember about you?"

"What?"

"One day in the late afternoon, you and Diop were sitting next to one another outside of his shop. As usual, Diop was telling a story."

"And?"

"You laughed so hard you fell off your chair."

I remembered that moment.

"Afterwards, Diop said that most white men laughed only with their heads, but you, having lived so long among us, had learned to laugh with your body. Diop said that people who couldn't laugh with their bodies would never understand life in Africa."

Until that moment I hadn't fully realized the importance of laughter in the interpersonal dynamics that constitute anthropological fieldwork. I also realized why I had gotten on so well with Nigerien colleagues and friends. I understood why I connected with my mentor Jean Rouch, for whom laughter created the bonds of long friendships from which emerged some of his greatest films, *Jaguar* (1955), *Petit a Petit* (1971), *Cocorico* (1975), and *Madame l'eau* (1992). I then grasped that it had been laughter—and stories—that had nourished the relationships I had slowly and gradually developed in Niger and New York City. Put another way, there is no separation of the personal and the professional and no Cartesian disconnect between emotion and analysis. As anthropologists we are personally implicated in networks of social relations the depth and quality of which shape the depth and quality of our work. "It takes two hands," as the Songhay elders like to say, "to nourish a friendship."

Despite these ever-present field realities, which constitute the here and now of social life, there is ongoing academic resistance to the emotional presence of ethnographers in their ethnographic works. There is, of course, a very long history of the intellectual separation of head and heart. It began with Plato's *The Republic* (2016) in which the sage warns of the danger of poets and dramatists whose works connect to the heart and evoke emotion. Throughout the centuries that followed scholars continuously reinforced those deeply classical principles. Stories are not serious science and have little or no place in academic discourse. Humor indicates a lightness of thought and must be reserved for the margins of scholarly representation. These conventions have resulted in what has been called plain style—the bloodless prose of (social) scientific reports. Indeed, in their Introduction to this engagingly important collection, *Affective Dimensions of Fieldwork and Ethnography*, Thajib, Stodulka, and Dinkelaker write:

Readers may find that in some of the contributions the writing style and the personal presence of the authors are jarring at times. This impression may be attributed to the impetus among many of the authors to address affective challenges in doing fieldwork that contributed to what they consider substantial anthropological insights of their projects. This is not an easy task considering that genres which engage in affective scholarship when it

comes to fieldwork and ethnography have so far positioned themselves as ‘*anthro-poetics*’ (Behar 1996; Rosaldo 2014) or ‘*auto-ethnography*’ (Ellis 2004), and hence if not at the margins, at least distinctive from mainstream ‘academic anthropology’ (this volume, pp. 8–9).

But can we comprehend the human condition without confronting our emotions?

As Songhay elders like to say: “You can’t follow two paths with one foot,” which is an oblique way of suggesting that any description of social relations that avoids human emotions is an incomplete, if not empty, endeavor. It is, after all, such core emotional sets as love and hate, fidelity and betrayal, and courage and fear—to name only a few—that shape the human condition. Put another way, the 27 ethnographically rich and conceptually provocative essays that comprise this volume demonstrate powerfully the need for the inclusion of the affective elements of fieldwork and ethnography.

The essays in this volume defy simple and specific categorization, which means that in this brief Afterword space precludes a detailed summarization of each contribution. Even so, there are some general topics that weave their way through all of the chapters. Here are some of the important themes that the contributors consider in their essays:

- the relationship between the personal and the professional;
- the link between emotions and personal relations in the field;
- personal implication in field relations;
- reciprocity (or giving back) in the dynamics in field relationships;
- the space between emotion and rationality;
- personal transformation in the field;
- fieldwork and family dynamics;
- the messiness of social relations compromised by disappointment and betrayal;
- embodiment and human emotions;
- the sexuality and the human emotions of “being there” and “being here”;
- the presence of existential uncertainty and anger in field settings;
- the problem of failure;
- the emotional whys and wherefores of apprenticeship; and
- human vulnerability and affect.

The powerful exploration of these under-explored issues in this volume enriches immeasurably the ethnographic record and deepens significantly anthropological insights about the nature of human being.

But how can we explore these essential human emotions that undergird our life in the world? How personal should our personal implications be reflected in a text or a film? How can emotions guide us to rational insight? Should we express our vulnerabilities in our representations? What are the limits of authorial presence in an anthropological text? How much is too much? How little is too little? These are questions we should ponder when writing about human emotions in fieldwork and ethnography.

It is one thing to analyze affective dimensions in a typical academic essay. It is quite another thing to evoke the affective dimensions of human being in a work of narrative non-fiction, a poem, a documentary film, fiction, a media installation or a blog. In the aforementioned academic essay, scholars attempt to ‘tell’ their readers about a subject. In more narrative forms of expression, scholars-artists (say, anthropologists who communicate their scholarship through narrative non-fiction, poetry, film, or media installations) attempt to ‘show’ their readers the texture of the subject. One tack is denotative; the other tack is evocative. One is a tack works well for the analysis of data and the construction of theory; the other tack works well for the sensuous description of human emotions that can evoke theory. The denotative approach to scholarly representation is a necessarily limited path to social description. The evocative orientation to scholarly representation is a necessarily full-bodied approach to understanding more completely the human condition. If you use narrative to evoke a world, you understand fully that “one foot cannot follow two paths” (see Stoller 2014, 2018).

So how do we confront the personal and professional perils routinely encountered on the ethnographic path? How can we take the detour that explores the emotions in order to convey deep anthropological insights about human being in the world? The answer is deceptively simple: we embrace the story and construct narratives that capture the ineffable complexity of life. As the late Edith Turner wrote: the ineffability of “(...) *communitas* can only be conveyed through stories” (2012, p. 1).

Although I found the essays in this book to be ethnographically rich and conceptually forceful, I often found in them an uneasy alliance between narrative and exposition, between evocation and denotation. Such textual imbalance is not at all surprising given the aforementioned institutional constraints that shape academic discourses that foreground explication (telling) and background narrative (showing).

If we look to narrative ethnography, there are concrete moves, borrowed from fiction, that scholars can make to overcome institutional obstacles that obstruct the powerful evocation of human emotion. Writers who want to emotionally awaken their readers tend to produce works that feature short sentences in active voice. They write relatively short paragraphs and craft short-chaptered books. Texts that ‘show’ rather than ‘tell’ tend to sensuously evoke space/place as if those spaces and places are alive with memory and feeling. Texts that ‘show’ also feature direct—as opposed to indirect dialogue—in which characters speak idiosyncratically. In these kinds of texts informants become characters who move, stand, or gesture in particular ways. In the end, these textual moves breathe life onto the page and sensuously evoke human emotions.

It is a profound scholarly challenge to write for, about, and with emotion. If you choose to employ narrative techniques to evoke ethnographic emotions, it doesn’t mean that you have to abandon traditional academic discourse or, for that matter, theory development. It does mean, though, that you foreground story and storytelling through the evocation of space/place, the articulation of direct dialogue and the development of character. As in the story I recounted at the outset of this Afterword,

narrative foregrounding can set the stage for blending story, analysis and theory into a seamless text that presents the human emotions in all their vexing glory.

The challenge, however, is far more than a textual move. Writing about personal implication, sexuality, fear, courage, love, and hate creates in the anthropologist a deep source of vulnerability. It is a choice that is existentially transformative. What does such a transformation imply? For me, it suggests a fundamental challenge. That challenge is for the next generation of anthropologists—and to the contributors to this volume—to approach the world as would an ethnographic painter for whom there is no absolute Cartesian divide between mind and body, between ‘subjective’ experience and ‘objective’ analysis. By taking an embodied painterly approach to the world, we follow a path toward the ‘there is’ on which we see-think-feel from the inside. As the great painter Paul Klee wrote:

In a forest I have felt many times over that it was not I who looked at the forest. Some days I felt that the trees were looking at me. I as there, listening...I think the painter must be penetrated by the universe and not penetrate it (...). I expect to be inwardly submerged, buried. I paint to break out (Charbonnier 1959 cited in Merleau-Ponty 1964a, p. 31).

Such an artistic move takes you to edge of ethnographic expression. Consider the insightful words of Maurice Merleau-Ponty (1964b, pp. 122–123):

Given the experience, which may be banal but for the writer, captures a particular savor of life, given, in addition, words forms, phrasing, syntax, even literary genres, modes of narrative that through custom are always endowed with a common meaning – the writer’s task is to choose, assemble, wield and torment those instruments in such a way that they induce the same sentiment of life that dwells in the writer at every moment, deployed henceforth in an imaginary world and in the transparent body of language.

If ethnographers choose to explore the human emotions from the inside, they need to approach the forest like Paul Klee an open themselves to experience, an opening to that brings into relief the considerable existential risks of vulnerability.

Vulnerability, of course, brings personal risk and discomfort, but it also opens a space for narratives—stories—through which is established a powerful connection between the ethnographic and her or his audience.

In this way, the (...) [ethnographer] (...)“using evocative language, brings life to the field and beckons (...) [audiences] (...) to discover something new – a new theoretical insight, a new thought, a new feeling or appreciation (...). And just as writers need to spend many years searching for their own voices, so we anthropologists need to find a ‘voice’ and create works which bring readers to dwell within us and we walk along our solitary paths in the field, exposing our hearts so full of excitement, fear and doubt (Stoller 1989, pp. 54–55).

The contributors to this volume present us a provocative challenge. Reading between the lines of these bold essays, are we not asked to take a leap of faith into the nether world of ethnographic narratives through which ethnographers reconnect fully with human emotions? That leap takes us into the spaces between things, where we find the story and know it, to paraphrase T. S. Eliot, for the first time.

When we embrace the story, we rediscover the field, reclaim our human emotions, and remember ourselves. That is a challenge worthy of our full attention.

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Appendix

1.1 Emotion Diary Template

Emotion Diary

Date: _____ Time: _____

Where are you now? _____

Who else is here: _____

1. What do I have to write down here and now?

2. What feeling describes me best today?
Can you describe why?

1

3. Is that feeling related to a particular situation or person?
When did that happen? Can you describe it briefly?

4. Who or what has surprised or impressed me today?

2

5. This is me in the field...
Try to describe or sketch yourself.

6. What am I longing for?

3

7. What I will do tomorrow is...

Further comments:

4

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