

Tim Burger, Usman Mahar, Pascale Schild,
Anna-Maria Walter (eds.)

THE MULTI-SIDED ETHNOGRAPHER

Living the Field beyond Research



[transcript] Culture and Social Practice

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The Multi-Sided Ethnographer

Culture and Social Practice

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The Multi-Sided Ethnographer: Living the Field beyond Research

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Martin Sökefeld in a Pakol cap, skillfully playing the flute at a bazaar in Gilgit city, Gilgit-Baltistan, Pakistan, in August 2013



Photo by Anna-Maria Walter

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Editorial

Introducing the Multi-Sided Ethnographer

Anna-Maria Walter, Pascale Schild, Usman Mahar and Tim Burger

Blurred boundaries

Ethnographic fieldwork and its representation, often in the form of an ethnographic text, are two sides of the same coin. At least since the *Writing Culture* debate in the 1980s, reflexivity in relation to the ethnographer's positionality has been crucial for writing ethnography, thereby making anthropological knowledge more transparent. However, the same cannot be said about anthropologists' everyday life in the field, which is commonly – if not intentionally – ignored in ethnographic texts. While outmoded stereotypes of the lone anthropologist immersed in a distant society, studying mysterious native customs, have now been widely challenged by the reflexive, relational and engaged practices of anthropology, some assumptions around what ethnographers actually *do* in the field remain unquestioned. This volume aims to uncover sides of the anthropologist and their lives invisible in ethnographic publications and hopes to disrupt misleading images that persist of what they do or do not do in the field.

Since fieldwork tends to blur the boundaries between private and professional life, ethnographers appear to be always on duty, eliciting valuable encounters and lying in wait for that next moment of serendipity, revelation, epiphany or insight. Yet what happens when the recorder is off, when the notebook stays in the pocket? What lies in the gaps and the pauses of a busy fieldwork schedule? What concerns and commitments drive ethnographers beyond and amidst, because of – and in spite of – their fieldwork? And crucially, how do these ideas and

activities shape their work, future projects and academic careers? These are all questions we cannot neglect, even if answering them means revealing parts of our lives that may unsettle beliefs and convictions of what ethnographic fieldwork is and what it should be.

Our volume approaches these questions by exploring four dimensions of ethnographic multi-sidedness, pointing to the more-than-fieldwork qualities of (1) *leisure* activities, (2) *kinship* relations, (3) practices of *representation* and (4) *politics* in the field. Leisure comprises the hobbies and personal interests anthropologists take into the field, discover over the course of their fieldwork or utterly fail to find time for. Kinship encompasses modes of accompanied fieldwork as well as the forging of new (fictive) kin relations. Both leisure and kinship make for a valuable analytic with which to think through an ethnographic life in and beyond research and to challenge classic tropes of research methodology. Representation, i.e., written and visual accounts of fieldwork, reflects on the ways and possibilities of acknowledging a life beyond the heroic imagination of an independent ethnographer gathering data. Lastly, politics, not only as everyday micro-negotiations of power, but also as powerful interventions during fieldwork, speaks to matters of agency and surveillance as well as collaboration and activism. These four fields of inquiry all emerge from our contention that anthropology is not made up solely of a body of refined and published ethnographic writings, but also of what remains unsaid or at the edges in such accounts and in anthropology more generally.

Throughout his work, Martin Sökefeld has both pointed out and drawn on the ethnographic potential of what lies beyond an anthropologist's 'main' research questions and activities. Everything we encounter in the field is potentially meaningful and illustrative of social practices and their contexts. However, we simply cannot – or may not want to – write about 'everything', for various reasons. Recognising this notion, Sökefeld also encourages us to write precisely about what we might (want to) miss at first glance.

His article *Ethnologie der Öffentlichkeit und die Öffentlichkeit der Ethnologie* (Anthropology of the Public and Public Anthropology) (2009) teaches us a great lesson here. In the article, Sökefeld traces anthropol-

ogy's complicated relationship with the media and the public in general through his own example as an anthropologist and that of his research partners: Kashmiris who struggle for public, international recognition of their movement for the right to political self-determination and freedom from India (and Pakistan). The article details how he conducted fieldwork in Srinagar, the capital of Indian-held Jammu and Kashmir, for his research on the Kashmiri diaspora's transnational connections, at a time when political violence and protests had broken out. These demonstrations were met with curfews and state repression, and he was mostly unable to pursue his research as planned. Amid these tensions, he was contacted by a German journalist from *Spiegel Online* for an interview. The topic, however, was not Kashmir but rather an upcoming annual commemoration day of the Alevi community, which Sökefeld had previously researched for several years (see Sökefeld 2008). Journalists in Germany, India and elsewhere seemed to take little interest in the political events in Kashmir at the time.

Apart from showing Sökefeld's versatile engagement as an ethnographer, the article explains how other-than-research activities during fieldwork can lead to new research themes and important social and political insights 'beyond research'. Rather than producing tunnel visions, ethnography, as Sökefeld reminds us, must respond to fieldwork encounters with a fundamental, epistemological openness. Therefore, instead of dismissing the interview as simply a frustrating encounter with a journalist, he takes it as an opportunity to reflect on the difficult relationship between anthropology and the public. How can we, as anthropologists, along with our research partners, participate in the public sphere and interact with the media in a more meaningful and politically transformative way? There are no simple answers in this regard, but it is the need and willingness to return to these and related questions of ethical and political engagement that shape, among other more-than-research activities, Sökefeld's multi-sided work as an anthropologist and ethnographer.

Inspired by his engaged scholarship and diverse interests, such as hiking, food, films, photography, politics and music, to say nothing of Pakistani truck art, the contributions to this book address matters of

commensality, various forms of (kin) relations, political activism as well as hobbies – taken to or brought back from the field – and other leisure activities. This Festschrift for Martin's 60th birthday is dedicated to him as an ever-learning student, teacher, colleague and friend. In all these roles, he has been adamant about the fact that 'the field' is not merely a place but a topos that needs to be understood as highly situational and contextual. This volume focuses on the shifting roles of the fieldworker within that social context. Probing the fuzzy boundaries between work and private life, it seeks to explore what anthropology can learn from the mundane doings of ethnographers beyond their immediate research questions and practices.

The fieldworker demystified

Examining the multifaceted lives of researchers in the field means challenging the long-standing imaginary of the highly professional, scientifically determined and emotionally detached ethnographer. This fanciful depiction results from a particular genealogy of the ethnographic method and continues to shape the making of anthropological knowledge. While it has been repeatedly pointed out that stubbornly adhering to classic principles of fieldwork is flawed at best, if not outrightly problematic (Amit 2000; Günel, Varma & Watanabe 2020; Gupta and Ferguson 1997; Stolz et al. 2020), we remain caught in a methodological myth of fieldwork that cannot do without the obligatory reference point of stationary, lonesome and 'data-gathering' ethnography.

Anthropological fieldwork has a lengthy history that can be traced back to the late 19th century (Stocking 1983). Before the emergence of the field as we know it today, anthropology was largely a theoretical discipline based almost exclusively on armchair speculation in which researchers would analyse from afar societies deemed as 'exotic', intentionally omitting any personal interaction. Dissatisfied with the 'unscientific' assumptions and stereotypes this methodology reproduced, a more intersubjective and immersive approach was advocated for in the early 20th century.

While Bronislaw Malinowski, in the early 1900s, was certainly not the first to realise the intellectual potential of spending time with the people one seeks to understand, he nonetheless championed the canonisation of modern fieldwork. His seminal work on the Trobriand Islands established the crucial disciplinary norms of long-term residence, language learning and participant observation (1979 [1922]: 24–49). Against the backdrop of the (natural) scientific context of the time, Malinowski professionalised ethnographic fieldwork as a core rational and objective method of social and cultural anthropology and, as such, also defined a clear separation from the researcher's emotions and private life. The posthumous publication of his diaries (1967) revealed not only this sharp distinction between 'rational' research (ethnography) and 'emotional' life (diaries), but also the presence of a racist and sexist fieldworker who faced a deep personal crisis because he did not live up to the empathetic and professional approach he himself had proclaimed. On the contrary, the diaries showed an academic who was deeply entangled with his research and research partners, but one who nevertheless consistently disregarded personal relationships and feelings in favour of a distanced ethnography. At the same time, the fact that Malinowski wrote diaries, which he had no intention of ever publishing, points to the existence of a private life during fieldwork – one that also involved activities beyond his research, such as reading novels or taking walks.

Malinowski's method of conducting ethnographic fieldwork became the standard for anthropologists and has served as a vocational rite de passage: "[I]t is fieldwork that makes one a 'real anthropologist'" (Gupta & Ferguson 1997: 1). While ethnography still relies heavily on first-hand encounters 'in the field', the originally rational, objective and holistic assumptions have had to make way for self-reflexive attention to the private, political and subjective circumstances of anthropological knowledge-making. This awareness of the researcher's "situatedness" (Haraway 1988), and the partiality of any knowledge produced, went hand in hand with enquiries into the textual strategies of creating ethnographic authority (cf. Clifford and Marcus 1986, Fabian 1983, Kuper 1988). Until the *Writing Culture* debate in the 1980s unsettled the production of anthropological knowledge more broadly, there had been

early interventions (mostly by women or anthropologists of colour) which highlighted the difficulties of ethnographic representation and experimented with alternative ways of researching and writing (cf. Hurston 1928, 1935; Powdermaker 1939, 1950). Feminist contributions, initially often unrecognised, pointed out the male bias distorting fieldwork practices and anthropological models of social life (cf. Ortner 1974, Rosaldo and Lamphere 1974, Strathern 1987). While such a genealogy displays a more complex, multi-faceted approach to fieldwork within the discipline, the predominant Malinowskian conception of ethnography continued to produce influential works and proponents throughout the 20th century. Edward E. Evans-Pritchard, for example, declared fieldwork that was not conducted on one's own and in isolation to be relatively worthless (1973). Similarly, Erving Goffman continued to argue for a rather martial understanding of ethnography with the imperative to "cut your life to the bone" (1989: 127) in order to find out anything meaningful to report about the Other. This single-minded, heroic and self-sacrificial devotion of the lone anthropologist in the pursuit of ethnographic data is what we identify as an enduring disciplinary norm in desperate need of alternatives. In this volume, we make the case for recognising the multi-sidedness and indeterminacy of fieldwork encounters and their value for anthropological knowledge-making.

Today, anthropologists continue to rely heavily on ethnography as a means of conceptual apprehension, disciplinary practice and critical theory-making. Fieldwork involves more than just 'collecting data', in that it requires building and negotiating relationships, engaging with social contexts and power structures and gaining trust within heterogeneous communities. The knowledge created through fieldwork is essential for anthropologists to comprehend, and provide insights into, human societies and ways of living.

Going beyond simplified understandings of ethnographic fieldwork, whereby we immerse ourselves in the 'culture' and context of our interlocutors, this volume aims to shed light on the complexities and messiness of fieldwork in post-colonial contexts. Instead of sweeping our concerns under the imagined rug of Malinowskian fieldwork, it reflects on the common dilemmas of ethnographic fieldwork in an effort to show

that the lens of multi-sidedness can help us acknowledge aspects that might, at times, be seen as 'limitations' and, in doing so, highlight their analytical and epistemological value. Gregory Bateson's story about Zen Buddhist training to overcome double binds comes to mind and helps illustrate our approach (Bateson et al. 1956 as cited in Gregory 2014; cf. Green 2014). In this anecdote, a Zen master holds a stick above a student's head and lists three options that the student seemingly has: to say that the stick is real, to say that it is not real or to say nothing. Whatever the student says will lead to them being hit. The only way out of such a dilemma is to take away the stick from the master. In our case, the metaphorical stick seems to be the many normative ideas about fieldwork, including the mythical ideal of an isolated lone hero who cuts their own life to the bare bone in order to understand their interlocutors or, better yet, adopts their way of life (cf. Evans-Pritchard 1973; Goffman 1989). Following Bateson's suggestion, in order to solve our double bind, we must challenge the premises of the debate by taking away the 'stick'. Our hope is that a discussion of ethnographers' versatility as multi-sidedness will allow us to take away the colonial, heteropatriarchal 'stick' of fieldwork standards and ideals that many of us try to live up to.

Before beginning his research, on one occasion, Usman Mahar rather unreflectively shared his ideal of fieldwork by mentioning Loïc Wacquant's boxing ethnography, "Body and Soul" (2004). Fascinated with this ethnographic research at a boxing gym in a black neighbourhood of Chicago's South Side, Usman conversed with a few colleagues. Discussing Wacquant's pugilist ethnographic engagement, i.e. his participation in amateur and professional fights to uncover the embodied aspects of masculinity at the gym, Usman exclaimed something along the lines of "Now that's ethnographic research!" Upon hearing this, Martin, who was within earshot, quickly pointed out to Usman that in the case of his PhD research this would mean living as an irregularised migrant or, worse yet, getting himself deported. Martin's often sharp and witty comments have remained with the editors of this volume and remind them of his dynamic and reflexive approach to 'the field' and life. His comments often make him take what Richard Rorty (1989: 74) calls the "ironist" position in opposition to the "commonsensical"

view of those who “unselfconsciously describe everything important in terms of the final vocabularies to which they and those around them are habituated.” On the abovementioned occasion, Martin helped Usman take away the ‘stick’ of an idealised approach to ethnographic fieldwork. Ironically pointing out that total immersion and adopting one’s interlocutors’ way of life could not be a blanket ethnographic maxim applicable to all contexts, nor a maxim by which we have to live, he aided Usman in questioning his ‘common sense’ in the Rortian meaning of the term. On a more abstract level, his remark communicated to Usman how the context of research determines the participation of the observing researcher and how the researcher’s subjectivity plays as much of a role as the lives of interlocutors in the fieldwork context. In most cases, the manifestation of the field takes place through the interaction between the two – not the disappearing of the former into the latter during fieldwork, only to reappear again during the writing-up phase. Thus, while Usman was able to live with his migrant, deportee and returnee interlocutors, Martin’s comment was a constant reminder that ‘living with’ or even ‘living like’ does not equate to ‘living as’.

While Martin would certainly not be against any kind of (full) immersion or adopting the interlocutors’ way of life in the pursuit of ethnographic research, he was critical of conflating immersion with membership (see Cosan Eke’s contribution in this volume, or Khosravi’s auto-ethnography, 2010). Particularly in certain contexts where, despite all our immersion, we are ultimately temporarily present and at best guests engaged in a ‘suspension of disbelief’. Therefore, we think it pertinent that ethnographers discuss in greater detail their own everyday lives in the field and unpick the particularities and subtleties of ethnographic immersion in all its complexities.

All that said, ethnographic engagement certainly still involves long-term contact with a group of people and, in many cases, a political commitment, even if from a distance. In a digitally connected world, we also take our political and social lives into the field and, similarly, remain connected with the politics therein, and our interlocutors, once we are back at “home” (Hughes & Walter 2021, Chua 2021). The mundane doings of

ethnographers beyond their immediate research question, we believe, are imbued with epistemological value and merit reflection.

The value of multi-sidedness

Living up to the methodological debates within anthropology, as well as to changing contours of the field in an increasingly globalised world, George E. Marcus (1995) introduced the multi-sited ethnography approach. What Arjun Appadurai (1996) analytically grasped as cultural *scapes*, Marcus sought to tackle empirically: Since, in the contemporary world, field sites can no longer be defined in the dichotomy of local and global but are more interconnected and mobile, a multi-sited ethnography acknowledges and follows the movements of and connections between people, goods and ideas, the circulation of meanings and identities between and across places and contexts. It would be nothing short of reductionist to limit oneself to a single *site*. Some anthropologists might have feared losing the depth of stationary fieldwork in multi-sited research, but it has shown to be a productive approach that very few modern-day research projects can omit. And with the advent of ubiquitous digital connectivity, ethnographic work has become even more hybrid (Pink et al. 2016, Przybylski 2021) as well as nonlocal (Feldman 2011).

While recent efforts by scholars advocate embracing the patchiness of research activities and anthropologists' private circumstances (Günel, Varma & Watanabe 2020) or work obligations as lecturers and professors (Dürr and Sökefeld 2018), we also notice the potential of an ethnographer's multi-sidedness. Why should we not analytically tap in to the multiple *sides* of the ethnographer instead of focusing only on their professional self? A discussion of the multi-sited ethnographer brings out their biographical, personal and emotional layers. Leaning on Marcus (1995), we may ask ourselves the following: as ethnographers with many sites, how do we traverse our many other sides, and vice versa?

After all, an anthropologist might be a daughter, a partner, a single mother, non-binary, of colour, passionate about music, sports or art,

a religious practitioner, a political activist, in a precarious career situation, an intensely sociable person or rather introvert and many more angles of the self that play out in ethnographic fieldwork. The Merriam-Webster dictionary defines “multi” as “having multiple and usually more than two sides” – just like a dice in a board game. But instead of implying some form of holism, the dice analogy alerts us to the different stakes each side holds, in that all sides make us move, albeit in different ways and directions. Some might immediately lead to more in-depth understanding, others might complicate the matter and only in the long run, if ever, unfurl their creative potential. Consequently, the multi-sided ethnographer is a researcher who draws on different registers of private and professional life to acquire diverse – possibly unconventional and surprising – perspectives on a particular social phenomenon.

The researcher seeks to generate a nuanced understanding of complex cultural contexts by drawing on a range of sources of knowledge and by engaging in sustained and meaningful dialogue with different groups of people. The approach means understanding how interlocutors experience, interpret and make sense of their social worlds, taking into account the diverse cultural and social practices, values and beliefs that shape their experiences by acknowledging the ethnographer’s own human embeddedness in the research context – in the co-creation of knowledge, considering not only their interlocutors’ points of view, but also the complexity of the researcher’s self. With this in mind, how then could any researcher ever attempt to disavow or disregard all that they are and bring with them to their field?

In his seminal article *Debating Self, Identity and Culture in Anthropology*, Sökefeld (1999) identifies a tendency towards othering in the way anthropology used to refer to identity. For groups, the term was often readily associated with “sameness of the self with others” (Sökefeld 1999: 417), i.e., implying sharing many characteristics with others and thus portraying our research partners as part of a collective identity instead of being their own personal (or psychological) selves. In contrast, ‘we’ ethnographers typically do not want to see ourselves reduced to collective markers but instead depict our subjectivity as being actively shaped by us in the process of learning and performing. Sökefeld argues that ‘selfsameness’

as congruency of a singular personality only exists in the Cartesian tradition of thought. As an alternative, we should consider all human beings as rich and competent selves who creatively juggle and enact competing identities; people can discern between their own conscious self and others in the group and use this ability to manoeuvre their own actions and motivations. Depending on the situation and network to which a person relates in a certain moment, the image they project and have of themselves varies. Hence, exposure to and interaction with different frames of reference also restructure their own self-perception. While we combine multiple identities within ourselves that can coexist, overlap and partly contradict at the same time, Sökefeld points us to a person's inner narrative of coherence and continuity that they construct and flexibly adapt to manage diverse identities.

This conceptual approach can be empirically illustrated by recent research which challenges the idea of parallel identities for an update (Chua 2021, Walter 2021). Enquiring into the consequences of constant connectivity afforded by mobile phones and online media demonstrates that different contexts cannot be held separately (anymore) but potentially render themselves present at any time. More than a serial representation of one's selves, these different roles are suddenly (or perhaps always, already) 'there', creating friction with one another, rendering inconsistencies cruelly visible, deflating the careful efforts of self-curation in which everyone strives to perceive themselves as a sensible whole – not a static or a bounded one but one in which splits are reconciled. Direct and immediate connections demand instant positionality, and a compartmentalisation of the self's different roles and emotions à la Malinowski is no longer possible – if it were ever more than an illusion. Consequently, people increasingly work to streamline their persona and realise that any experience recalibrates the whole.

The contributions to this volume arrive at a similar conclusion: ethnographers have many sides, bring different interests and liabilities to the field and adjust themselves to various contexts and situations. However, the field and the home, the private and the professional, the personal and the public are not disconnected but mutually shape each other. Boundaries are extremely blurry, and any interaction affecting

the person of the ethnographer consequently also has an impact on the subject(ivity) of the ethnography. Fieldwork, as James Clifford (1997) observed, is a “disciplining practice” that provides anthropology scientific credibility and distinction from other disciplines while at the same time forcing anthropologists to adhere to certain standards and conventions (see Burger/Burger in this volume), such as the conception of the ‘field’ as a distant place into which the researcher enters and exists. An ‘anthropology at home’ has called this divide into question and broken down the epistemological distance between researcher and interlocutor.

In his piece *Feld ohne Ferne* (Field without Distance), Sökefeld (2002) similarly muses on the artificial separation of a naïve outsider versus an insider’s lack of objectivity and acknowledges the need for (self-)reflection. Whether doing anthropology at home or somewhere else, the ethnographer’s background and circumstances are an intrinsic part of their everyday life during fieldwork. Moreover, the researcher is not a static entity but a responsive and permeable being changing and transforming often over time. While supposedly private aspects of leisure and kin relations or emotions and difficulties encountered during fieldwork (Schild 2021) are often neglected in ethnographies, possibly even muted or remain untapped altogether, they are nevertheless crucial in shaping ethnographers’ works and lives. Since the anthropologist is the prime instrument of ethnography, Ian Pollock (2018), in a blog post about his emotional disturbance after a terror attack, asked himself, “Maybe if I played this tragedy right, I could have turned it into data after all.”

This collection of essays strives to bring to the fore some of the frequently ignored motivations and contingencies of researchers’ interactions, findings and interpretations. How, for example, would Sökefeld ever have published an illustrative photo essay on Atabad Lake (Gilgit-Baltistan) without his passion for hiking? And would he ever have discovered the Alevi movement in Germany had he not lived with his family next to a community centre in Hamburg when his children were still young? Rather than a project of navel-gazing, the multi-sided ethnographer makes the case for recognising the anthropologist as an indivisibly complex human being that is inseparable from their work.

Outline of the book: Ethnography as more than fieldwork

We have identified in this collection four domains of (private) life and multi-sidedness that seem to play out most prominently during fieldwork: leisure or private hobbies; (fictive) kinship relations; practices of documentation and (self-)representation; and politics and ethical engagements.

Section One explores the forms and possibilities of leisure in the field. Drawing on their early fieldwork experiences in Mitla (Mexico) and Nilgiri (India), Eveline Dürr and Frank Heidemann discuss 'leisure' in the field. They unpack the fuzziness of work-life boundaries in our discipline over the years through ideas related to their positionality, specific conditions in their respective fieldwork sites and certain challenges, such as being closely watched and intimately connected with the lives of the interlocutors with whom they worked. Other contributions demonstrate that anthropologists do indeed have and maintain hobbies during fieldwork, which sometimes even turn into important research activities. In her chapter on hiking ethnography, Sabine Strasser traces diverse forms of 'walking-talking' in ethnographers' lives. Relying on the experiences of four ethnographers, as well as her own hiking experience with Martin Sökefeld in the high mountains of Pakistan, her thoughts on walking and conversing open up a path for the reader to reflect on the affective and embodied sides of research practices.

Beatrice Odierna's chapter further underlines such leisure-related connections, showing how sewing, especially patchwork, helped her to (re-)connect with her research partners through a shared hobby during the Covid-19 pandemic and its lockdowns. At the same time, patchwork became an important metaphor for her research practices and a way to rethink her positionality in fieldwork at home. Finally, Alessandro Rippa demonstrates how a side-interest can serendipitously develop into a central research perspective. More specifically, he traces how his search for carpets for friends in Europe, and his interest in carpet craft and design, deepened his understanding of transnational trade relations between China, Pakistan and Afghanistan.

Shifting the perspective from leisure activities and private hobbies to social belonging and relationships, Section Two examines the role of kinship and family and religious ties in fieldwork. Drawing on her experience as both an Alevi member and a researcher of Alevi religious practices in Germany and Turkey, Deniz Cosan Eke shows how the ethnographic field and its constituent research partners not only open up to the 'insider', but also hold special challenges for them. The 'insider' has to prove herself, which in turn makes her an 'outsider' again. The social constitution of the field through the researcher's personal relationships is also the theme of Menahil Tahir's chapter discussing the role of her parents in her fieldwork among Afghan immigrants in Pakistan. The author shows how the presence of the anthropologist's family in the field helps build trust and enables research partners to emotionally draw connections between their lives, experiences and histories and those of the researcher. Sometimes, however, it is not so much the kinship relations that anthropologists bring into the field but rather those that they take with them from the field that continue to shape their future research. Stephen Lyon's chapter shows that an anthropologist's fictive kinship ties can also close down possibilities of research and even lead a researcher to give up their professional work because of the social obligations that come with the assigned role of son and brother in a local family.

While all contributions understand 'the field' as constructed by the researcher's social relationships, Section Three extends this perspective to those practices of living and (visually) representing the field that often do not make it into published ethnographies. Lisa Burger and Tim Burger reflect on their experiences of accompanied fieldwork, showing how couples, in particular couples with children, may differ in terms of their understanding of events in the field and how to respond to them. However, as in their case only one partner was a fully funded researcher, only his version was to eventually find its way into a PhD thesis. Anthropological (writing) conventions tend to produce tunnel visions of both fieldwork and ethnography, marginalising a wide range of experiences and personal circumstances – as Martin Saxer argues in the introduction to his photo essay, which provides us with the visual by-product of

his professional life as a researcher on the move and at home with his family in various parts of the world. In his contribution, Magnus Treiber traces the rarely discussed transformation of field notes into ethnography as a crucial epistemological and analytical process in the making of anthropological knowledge.

The last section introduces anthropologists' complicated political entanglements in and with the field. Pascale Schild draws on her experience with state surveillance during fieldwork in Pakistan-controlled Kashmir. Her contribution points out that suspicion is not merely a sideshow of ethnographic research, but also offers the possibility to think through both the politically intimate and intimately political workings of coloniality and military nationalism in the lives of researchers and the people with whom they live and interact within the field. In his chapter, Azam Chaudhary extends on the conditions of coloniality, discussing state surveillance in Pakistan from the perspective of the 'local' (Pakistani) anthropologist collaborating with foreign researchers. Chaudhary points to the post-colonial researcher's distinctive positionality and their vulnerability to suspicion and mistrust from the state and society, due to their collaboration with suspect but privileged foreigners. Finally, Usman Mahar examines altruistic engagement as not only an ethical requirement, but also a complex and an affective side of the ethnographer. This leads him to probe the politics and ethics of what he terms *qurb\ani* (nearness as a path to altruism) as a way in which researchers can engage with research partners as collaborators despite persistent constraints.

The volume closes with a personal letter to Martin Sökefeld by his former Master's and PhD students Anna Grieser, Anna-Maria Walter, Sohaib Bodla, Jacqueline Wilk and Clarissa Leopold. They reflect on their experiences of falling in love and getting married as a consequence of their fieldwork, challenging one of anthropology's firmest taboos, namely that of 'going native'. Even though the myth of the ethnographer's objectivity has long been disenchanting, the professionalism of anthropologists is still doubted in the face of emotional entanglements.

Drawing on the notion of 'living the field beyond research', the contributions collectively offer diverse and nuanced discussions of fieldwork

as well as ethnographically informed perspectives on the meanings and possibilities of practices and positions 'beyond research'. Some authors point to the embodied experience of fieldwork and their near-complete immersion in a social context, with their private lives and personal relationships often inseparable from research (Dürr/Heidemann; Schild; Cosan Eke; Tahir). Others, in contrast, focus on the way private hobbies and interests discovered during the course of fieldwork inform new research questions (Odierna; Rippa) and on how new personal relationships fundamentally change their role and positionality as professional researchers (Grieser et al.; Lyon; Chaudhary; Mahar). At the same time, some fieldwork experiences will forever remain on the margins of published work (Burger/Burger; Treiber; Saxer). However, they remind us that no anthropological research is possible beyond a researcher's multiple positions, passions and relationships within local and global power structures. It is indeed this recognition that is at the heart of our claim about the multi-sided ethnographer: while there certainly is life beyond research, there can be no ethnographic research beyond life.

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Section One: More-than Leisure

No *Feierabend* after Fieldwork?

Reflections in Retrospect

Eveline Dürr and Frank Heidemann

Introduction

The German term *Feierabend* combines the words *Feier* (a celebration or party) and *Abend* (evening). It is often used in a rather general way, to describe the time ‘after work’ or the moment when ‘free’ time begins. Instead, the term *Freizeit* (literally translated as ‘free time’) describes a period free of duties, free of work and social obligations. When “Feierabend!” is announced, a work task is declared completed or postponed. Germans are especially known for distinguishing between working time and private life. As Fischer (2014: 108) rightfully states, “Feierabend—that time after the workday is officially over—is taken seriously even by many professionals in a way Americans would find odd”. Leisure time takes place after (or during) *Feierabend*, without the task of formal and informal obligations.

In this essay, we reflect on *Feierabend* and leisure time in our early fieldwork experiences. When we studied anthropology during the 1980s in Germany, we were taught that fieldwork is the discipline’s key method, and that the researcher should plan to spend at least one year away from their home community to study what was framed in this period as ‘the other’ or “the culturally different” (cf. Kohl 1993). However, there was little systematic teaching on how to go about this at the time, although learning by doing seemed to be the most common procedure. In any case, we do not recall much debate about what anthropologists do in the field

when they are not actively researching, although it is well known that Bronisław Malinowski withdrew to read fiction to escape from the Trobriands. Thus, when we prepared for our fieldwork – Eveline Dürr in Mexico and Frank Heidemann in India – we had no plans for ‘leisure’ time. Rather than thinking about ‘leisure’ in terms of ‘time off’, we were excited to join in the daily life of the host societies and – of course – share *their* leisure time. We considered fieldwork an ongoing endeavour with as few interruptions as possible.

Our approach to fieldwork as students was shaped by ethnographies conveying the impression that ‘fieldwork’ was a solitary affair: it was expected that anthropologists should be on their own while participating in other people’s everyday life as much as possible. Particularly in German ethnographies, documenting seemed to prevail over theorising. However, German anthropology was also influenced by international debates. Thus, the ‘writing culture’ debate took shape (Clifford and Marcus 1986), and we discussed issues such as authorship, authority and power in ethnographic work as well as culture as ‘text’ and fieldwork as ‘reading’ (Geertz 1973).

Another key dictum was the notion of holism, insisting that all aspects of society and culture are somehow connected. This did not mean that we worked without any focal point; in fact, we searched for subtle interconnections of ideas or norms which, at first sight, seemed unrelated to one another and to our initial research question. As a first step, we tried to embed our observations in the local setting, and only as a second step did we refer to a specific anthropological sub-discipline. Today, ethnographic fieldwork and publishing focus more on specific topics or sub-fields in anthropology. At times, the everyday cultural knowledge of the ‘host family’ appears less important than that of specialists.

Electronic communication media has enormously transformed the ‘field’ and the rhythm of fieldwork. In this vein, the process today is ‘interrupted’ by a range of professional duties: as professors of anthropology, we address reviewers’ comments on our articles based on previous fieldwork, we review other people’s publications, respond to students’ queries and work on applications for future research projects – and we keep in touch with relatives and friends (Dürr und Sökefeld 2017). We be-

lieve that these developments transform the way we conduct fieldwork, albeit we are not sure whether this transformation creates more opportunities for leisure or 'free time' than some decades ago – maybe quite the opposite.

In what follows, we offer some reflections on our individual situations in the field as postdocs in the 1990s. While we did not reflect on 'leisure' at the time, we identify retrospectively a range of issues that made it difficult for us to find space and time for ourselves or to carry out activities not related to our field(work). We argue that what prevented us from 'withdrawing' from fieldwork was not only the wish to immerse in the respective context as much as possible, but also the entanglement of our positionalities as 'other' or 'different' and with specific conditions in our respective fieldwork sites. As we shall illustrate in the following, while Eveline found it challenging to deal with the sensation of standing out and being observed while conducting fieldwork in a rural Mexican community, Frank could hardly escape close companionship and social embeddedness during his research in rural India. Our reflections recall our experiences of our fieldwork in rural settings and what were called 'face-to-face' communities at the time.

Eveline Dürr: Observing and standing out in Mexico

As a postdoc, I conducted fieldwork in the mid-1990s in Mitla, a touristified small town in the southern Mexican state of Oaxaca (Dürr 1996). I followed early feminist anthropologist Elsie C. Parson's footsteps, who published a comprehensive ethnography on the then Zapotec village, whose economy relied on trade between the central valley and the mountainous hinterland (Parsons 1936). Elsie Parsons, influenced by Franz Boas, conducted fieldwork as a solitary woman in Mexico, which was certainly unusual at the time – both for US-American anthropologists and the village dwellers. As I read more about Elsie Parsons, I discovered that she was not as alone as I had thought during her three fieldwork stays in Mitla from 1929 to 1933 (Parsons 1936: xiv), and she was actually in contact with other US anthropologists working in the Oaxacan region. I suspect

that Parsons joined up with them when she was not ‘researching’. However, not much information exists in her ethnography about what she did when she was not engaged in the field(work).

Arriving in Mitla, my intention was to explore issues of cultural change, and I wanted to find out more about how ‘innovations’ are accepted and implemented – or not – in this specific community. Mitla seemed to be a good choice for a re-study, as I could count on a solid dataset because another US anthropologist, Charles M. Leslie, had also conducted fieldwork there in the 1950s. Unlike Parsons, however, Leslie took his family with him to Mitla, where he stayed with his wife and his son for one year (Leslie 1960: 1). In his book, there are some reflections on his family’s interactions with the village dwellers, but it is hard to find details regarding leisure time. Nonetheless, he mentions the entertainment of another US-American couple living in Mitla, and the hospitality of Mexican colleagues (Leslie 1960: v-vi).

As Mitla receives many tourists, drawn by its famous archaeological site, called *ruinas* by the locals, I thought that as a white German woman I would not stand out that much. Mitla is spread out, and tourists are commonly seen walking along the major road, making their way from the town entrance and bus station up to the archaeological site. I quickly realised that there was not just a spatial but also a temporal dimension to tourists’ presence. They would take a special route up the main road, checking out the artisan stores along the way and wander back the same way, hardly leaving the main tourist path. Very few would consider staying overnight, as the capital city Oaxaca is less than an hour’s bus ride away and offers far more tourist attractions than Mitla. Thus, in the late afternoon, once the archaeological site had closed, hardly any tourists would be seen around – and that made me stand out even more.

From the beginning of my fieldwork, I was fortunate to live close to the town centre with a local family and their three children. As my research focused on social life in the town, I was not really mobile, i.e., I did not travel a lot to other places; rather, I stayed put and spent most of my time in Mitla itself, exploring the town on foot. This mirrored people’s everyday life, as walking to the town centre to attend the market, or walking up to a tourist hotspot to sell artisan goods, was part of their

daily practice. Streets were busy, and town dwellers usually knew who went where, and why. Still, they would ask each other, including me, as a kind of greeting, “Where are you going to?” (*¿A dónde vas?*) and receive the standard answer, “On an errand” (*Al mandado*).

I started my fieldwork through the social network of my host family, who generously acquainted me with their relatives and friends and also advised me to whom it would be best to talk, and when. My daily routine consisted of walking to the homes of my interlocutors, having conversations with them and then returning to my host’s house. Thus, I walked a lot in the streets, was visible in public spaces, attended a range of ceremonies and joined in with other activities. Upon my return from daily rounds, my host mother would inquire where I had been and to whom I had talked. As I was hesitant to give away my full daily schedule, she would easily guess what I had omitted – and to my surprise, I felt that she enjoyed letting me know that there was not much I could hide from her. In fact, she knew where I had been even before I was back home – at times, she would receive me at home by telling me not only whom I had visited, but also which visit I had skipped. When I asked her how she knew, she would smile and respond that this is how things worked in the town – everybody knew what everybody else was doing by watching and being watched.

I realised that observing was not only an ethnographic method I was exercising, but also an essential practice in people’s everyday lives. Nevertheless, a simple parallelisation of these practices would not be appropriate. The ethnographic method includes immersion in the lives of the inhabitants, which makes a mutual observation inevitable – even though the effects of this observation may not be disclosed to the ethnographer (Verdery 2012). In Mitla, I learned that people were fully aware of mutual observation amongst themselves, which served not just as a kind of social control. Rather, rumour and gossip are embedded in a communication network and can normalise transgression, thus making it potentially possible for others to do the same (cf. Hagene 2011). Moreover, there was also talk about who was particularly attentive to other people’s behaviour. For instance, when a speed bump (*tope*) was built to slow down cars in front of a house with big windows facing the street, my interlocu-

tors told me there were rumours the owners had asked for the bump to be built there, as they were located at the intersection of two main streets and would thus be more able to watch who was going where. The members of this household, a grandmother and her daughter, the latter of whom was a single mother of a teenage girl, had the reputation of being extremely gossipy. I took my interlocutors' statement as a warning to be cautious when I went to visit there and to stay alert, as they would listen very carefully to what I would say and what I would not say.

This situation of being watched while being an observing researcher affected me in various ways. I became aware that walking in the streets or just being in a public space were not innocent practices but were rather inherently performative. I also felt that there was always somebody watching, and subsequently commenting on me, regardless of what I would do and to whom I would talk – or have failed to include in my conversation. This put quite some pressure on me, as I wanted to get it 'right'. I wanted to comply with the local protocol and hoped that the town dwellers would comment positively on me. Thus, I felt unhappy when negative remarks about my behaviour were channelled towards me via my host mother. One day, she told me that I had taken the wrong kind of bread to a wedding ceremony – I had placed bread for funerals on the altar of the hosts instead. But she also told me that people took it with good humour and were not really offended by my mistake, which made me very much aware again that I was seen as a stranger, a person who does not really belong but whose behaviour is nevertheless noted and talked about.

The feeling of being on display deepened my sense of non-belonging, as it made me aware of my own self in an inescapable way, which was not only through not yet knowing sufficiently about how to behave, but through my body. At the time, ideas of embodiment had just begun to unfold as fieldwork techniques. Less of an instrument for a sensory ethnography (Pink 2009), the body was still seen as something that needed to be controlled and overcome, for example by staying awake during long ceremonies, eating and digesting everything the locals do, adjusting to environmental conditions and so on. I became hyper-aware of my European phenotype, and as a relatively tall female, I literally stood

out in many ways, regardless of where I was; in other words, not just in a public space, but also in more private and intimate situations in people's homes. Early on in my fieldwork, I attended a ceremony shortly after a person had passed away in the house of my hosts' relatives. Still not fully familiar with the appropriate behaviour, I realised the formalisation of interacting with the bereaved when visiting their house: entering with a bent posture, crossing yourself toward the east where the house altar was, placing your offering there, then curtsying to the host, followed by a mock hand kiss, then taking a seat and watching more guests arrive, each following the same procedure before being offered a meal. To my surprise, the host asked me if I had my camera with me, which I had. She asked me to take pictures of the scene, including the women who prayed and wailed as a form of ritualised mourning for the deceased. I did what I was told, but I felt particularly uncomfortable. I was extremely present, not only because of my 'otherness', but also because of the flash-light on my camera, which I felt disturbingly heightened this attention. It was impossible for me to just 'be there', let alone to blend in, and as I had anticipated, not everybody present approved of me taking pictures.

However, there were also other instances in which my embodied otherness was a vehicle to engage in conversations, for instance when a teenage girl asked me which kind of shampoo I used, as my hair seemed to be so different from hers. This stirred a conversation about our (different) bodies more generally, and it was precisely the conversation about otherness and our differences that created a bond between us. On other occasions, I was mistaken by a male teenager for a 'gringa' from the US and asked if I could facilitate his migration *al norte*. When I told him that I could not help him in this matter, he still wanted to know if I had been there and how life was in places he thought he had no access to – other than watching them on TV. After our conversation, I bought a map of the world, which we rolled out in front of us, and we talked about place imaginaries, ranging from Jerusalem as a well-known religious site to Madrid as the centre of political power during colonial times.

Being under observation also applied to more private spheres, for instance when I was in my room. In my host household, the rooms were arranged around a patio, and so my host family expressed concerns that

I would read too much, as they considered this unhealthy for my brain – in particular because I occasionally suffered from headaches.

Thus, while my experience of being under observation differs from being surveilled by a state institution while conducting fieldwork, as Sabine Strasser and Martin Sökefeld (2016) discussed, I saw little space where I could act unnoticed or not be commented on. As much as I tried to see myself through my interlocutors' eyes, I was often only guessing and left unsure about the ways people interpreted my behaviour. However, it is important to note that my situation differed substantially from the image of a solitary researcher. While in the field, I frequently received visits from family, friends and sometimes other anthropologists, but I would not consider this an 'interruption' to my fieldwork *per se*. It was somewhat difficult for me to think of 'time off' or leisure in terms of a 'break' from fieldwork while being on site – I would rather withdraw from field(work) by leaving the field site physically. For instance, I went on holiday with friends, during which I visited a Mexican beach. I experienced this 'break' and socio-spatial distance from the immediate field context as a key factor in reflecting on myself and my social relationships, not only directly related to the 'field', but also to re-focus my research. This also points to the interplay between immersion in and distance from the field, which is pivotal in ethnographic work. As an alternative to spatial distancing, I would withdraw from the field, at least to some extent, by engaging with my 'own inner world', e.g., by listening to music or reading novels or letters coming my way, all of which seemed to stem from another world. I conceive of this as more than a simple 'break' from fieldwork and rather as an important time in which I could balance my own wellbeing.

Through these examples, I wish to highlight that observations are always relational and that observers are not detached from what they observe (Dürr 2023). Moreover, what observers observe says at least as much about them as what they overlook, and there is also a spatiality and temporality to watching and being watched. I was not everywhere and constantly exposed to being watched with the same intensity, nor did I conduct participant observation with the same level of intensity all the time. Instead, there were times when I was particularly attentive, for

instance during a ceremony, a ritual, an interview or an informal conversation – and in these situations, town dwellers would also be more attentive toward me. It is important to note that observers are not to be understood as isolated and solitary but as part of a specific observation scenario, for they play an active role in the situations they observe, and, even more so, they not only change them but are themselves transformed by them. It is precisely this mutuality of watching and being watched that lays the ground for common interpretations of others' practices in relation to one's own (cf. Whittaker et al. 2023).

Frank Heidemann: Social embeddedness in India

In the period from 1988 to 1998, the time to which I refer in the following, I worked with the Badaga people, the principal farming community on the Nilgiri Plateau, about six hundred kilometres west of Chennai at the border to Kerala and Karnataka. Historically, the Badaga grew millet, but when the Nilgiris became a 'British' hill station in the nineteenth century, they planted 'European' vegetables for white people. After Indian independence in 1947, they began to cultivate tea, many of them with great success. Many families spent their surplus on the education of their children or in other investments. Badagas won political elections and became the undisputed dominant group. They speak their own language, worship their own gods and live in exclusively Badaga villages (Hockings 1980). I first lived in the eastern part of the district, with my wife Bernadette and our recently born daughter Lena, for 12 months in the small town of Kotagiri. Later, I went alone to the hills for 2–3 months a year, and for some time I stayed in the Badaga village Jackanarai (Heidemann 2006). Since this research, my ethnographic interest and family contacts have continued to the present day.

I am trying to remember what I did when I was not working explicitly on my research questions. I was often invited, fed, accommodated and taken to family parties, weddings or other events. I also enjoyed the landscapes and the never-ending search for new topics and projects. However, I have no memory of what could be called 'leisure time'. I did

not pursue any hobbies or specific passions in India, and I barely moved away from my research on my own initiative. This is not a complaint, though, because I felt extremely comfortable among the Badagas. The short answer to the question regarding what I did as an ethnographer when I wasn't researching is simple: I waited. Waited for a bus, waited for the rain to let up on motorcycle rides or waited for priests and headmen in their villages or for people who wanted to see me in my house. I waited long before the advent of cell phones and a direct-dial system that could make long-distance calls. I often spent half a night in a post office, from where I could be connected manually to Coimbatore, from there to Madras and from there to Germany. Above all, however, I waited for interlocutors. These waiting times were later shortened with the introduction of cell phones; however, the new technology led to last-minute cancellations of many appointments – after I had already reached the meeting point. Fortunately, I was almost never alone while waiting, and new topics of conversation arose – often somehow linked to my research questions.

I know of no ethnography on rural society in India in which the authors reported a lack of companionship. An early example of this friendliness is the classic monograph "The Remembered Village" by M.N. Srinivas, who stayed north of the Nilgiris in a remote village in the 1950s; he was even accompanied when he followed the call of nature (Srinivas 1978). Later, Michael Moffatt (1979) abandoned his first attempt to do fieldwork in Tamil Nadu because he could not bear the lack of privacy. Perhaps they were lonely, but they were hardly alone. In my circle of Indian friends, many had never travelled alone or slept alone in a room, at least in the 1990s. When travelling to metropolitan areas, they always had company or stayed with friends and relatives. Most movements were in groups, and mobility and everyday life were transparent. In the villages, houses were usually unlocked, and neighbours walked in and out. On one occasion, I asked a Badaga friend to look after my cash while I went travelling, but he refused because everyone in the neighbourhood had access to his locked desk drawer, and someone might see the money by accident and spread the news. Consequently, in the case of an emergency, such as a serious illness or accident in the

neighbourhood, he would not be able to resist lending out my cash to pay for treatment.

In Jackanarai and Kotagiri, my movements were accurately registered, and I always had to report to anyone where I was, with whom I talked (and about what) and where I planned to go. When I went to the photographer to get my film rolls developed, he told me that someone had seen me at the bakery earlier, where I had two milk coffees. At the bakery, they knew that I had already bought the daily newspaper. Badagas would see it less as a form of control but more as caring, as many considered it impolite to leave someone alone; as such, when I walked from Jackanarai to the nearby Aravenu bazaar, young men would join me out of courtesy. Once, I had documented a ritual at night-time in the next valley and wanted to ride home on my motorcycle. For whatever reason, a man (I did not know him before) decided to ride pillion with me, following which, after arriving at my destination, he walked back, leaving me confused. The next day, I learned that he was the only one who wasn't afraid of ghosts, so he had been sent to accompany me. In short, I was rarely alone, and neither are Badagas. Two short anecdotes will illustrate this point.

The first incident tells the story of Ravi, who was always late for our appointments. In the late 1990s, my longtime friend and collaborator Mathan could not travel with me to the surrounding villages during a two-month stay. Ravi, his friend, filled in, but he was regularly running extremely late, much more than what I considered to be normal. He replied to my questions about the reasons for his tardiness with invented stories, obviously fictional narratives, always with a dramatic element. The following year, when we talked about the issue, he opened up to me as follows. Every day, when he had to change his bus at a junction, a friend called him over to his store across the street. Each time, the friend said the bus had just left and ordered a tea for Ravi. He missed the next bus, waiting for the tea. When another bus came, Ravi was involved in conversations with honourable persons and had to answer questions about the well-being of his family members. Ravi's friend could not bear Ravi waiting completely alone at the bus station, and so being alone had to be avoided at all costs. Day by day, Ravi faced

this dilemma. However, it would have been too trivial for him to cite this as a reason for his unpunctuality. Thus, 'out of respect', he kept making up new stories. The involvement of an individual in patterns of expectation and politeness also reached out to me when I planned something that could be called 'free time'.

On another occasion, and never thereafter, I planned a hike with a friend. I wanted to go on a three-day hike with a young and energetic Badaga environmentalist. I had already seen much of the countryside from my motorcycle but rarely had the opportunity for longer walks. My friend planned for the two of us to spend two nights in a Forest Department log house, since he had good contacts with the rangers. Our plan spread through our extended circle of friends, and instantly the idea came up to involve more people and to drive most of the way in a jeep, as this would allow us to transport more provisions. In the course of planning the excursion, another friend's friend came into the picture who was a good cook, but whom I had never seen before. The three-day trip was reduced to two days because two people were time-bound due to family rituals. Not everyone in the group was really aiming for a hike, and concerns were expressed in terms of heavy rain possibly making the trail impassable, even for a jeep. My objection that I wanted to walk anyway was not even heard. Another objection cited the size of the log house, which was too small for a group, but I made no protest in this regard because the group size was already set and it would have been impossible to exclude one of them. Another participant had heard from a reliable source that a tiger had been spotted near our destination the previous week. Slowly, it became clear to me that it was not a three-day hike planned by me but an excursion arranged by the extended circle of friends, to which I was cordially invited.

After a few days, the trip was re-scheduled with a new agenda. The small travelling group, men aged thirty to forty years, was now limited to six – as per the passenger occupancy of the rented vehicle. The newly revised destination had the advantage that one could drive up to the doorstep with a car. It was a private house, which was used by its owner only occasionally, nestled in the wilderness north of the Nilgiri plateau. I had seen the natural landscape many times from the Kodanad view-

point, which offers a dramatic view of the wider region. In the years before, I had joined worshippers of the “Seven Mariyamman” as they walked down to the plains and was able to document the rituals for a goddess there (Heidemann 2017).

The planned trip did not correspond to my original wish, but I was still looking forward to joining my friends. I did not have to worry about anything, as everything was organised. No one in the group believed they had gone against my original intention. Their planning was based on the idea that no one should travel alone, and preferably not in a small group of two. From the Badagas point of view, one also ‘shows’ one’s inclusion in social structures by travelling in group sizes of four and up. A second basis for planning this trip was that one should not do too much unnecessary walking. The wilderness experience I envisioned in a log house would be no better than the private house in the wild. In addition, more people could come along and do so comfortably in a passenger car. In their perception, they had made a trip better than I had originally envisioned. Badagas are caring people.

The departure time for the trip was moved from morning to lunchtime, due to fellow travellers’ family obligations, and then delayed for other reasons that I forgot because I never really understood them. In the afternoon, we took a diversion via Coimbatore, where one of our friends had to drop off something and where a newly opened eatery awaited us. The onward journey took place only after the first round of alcohol had been consumed, and so the view during the ride through the darkening landscape was limited to the reach of the car’s headlights. Finding the destination proved difficult, but everyone was in high spirits, and the prospect of not being able to have dinner after midnight was part of the plan. The next day, after a long sleep and a hearty breakfast, we prepared for the return journey. On the way, Badaga songs were played, which I would have enjoyed, too, had the speaker system not been half-broken. The journey was always interrupted when someone wanted a coffee or for any other reason. One friend, for instance, did not want to pass by his brother-in-law’s parental house without a greeting, so we made an additional stop. I learned a lot about group dynamics in this peer group, in which every need was met with great

consideration by every fellow traveller – after all, one has free time and is not in a hurry. Moreover, the subordination noted within a hierarchical structure like a family or a village community was conspicuous by its absence.

At times, I was integrated into the free time activities of my host society. On such occasions, however, I remained a participant observer. On picnics with an extended family, or excursions arranged by the local lawyers' association, I took note of how gender relations change in an extra-village context, how the table order is transformed, how photographs are staged in the private sphere and so on. Even in retrospect, I cannot draw a clear distinction between working time in the sense of empirical data collection and 'free time'. A private invitation, attending a wedding or a funeral, reading the daily newspaper or going to the movies cannot be sharply separated from my research on "Religion and Politics of the Badagas." In wedding halls and at tea shops, politics are discussed, at funerals, the dead are forgiven their sins and human lives are honoured, and at the movies, a dreamworld unfolds before viewers that also acts as a generator of ideas for life plans or an upcoming pilgrimage. In retrospect, 'leisure times' were defined and created by my host society; they were mandatory parts of my research but not 'my leisure time'.

After writing, I asked my wife, Bernadette, to read the text. She had often visited me during the fieldwork periods, and twice we spent a full year together in India. We compared the content with our memories, and it wasn't long before it occurred to her that I had forgotten something! In 1988, I had brought along a high-8 video camera and a postcard-sized monitor to view my audiovisual documentation in Kotagiri. This enabled us to watch video tapes that had been recorded for us of a TV programme in Germany. When we had many sleepless nights with our first-born daughter Lena, the greatest thing for us was to watch the ARD programme "Tatort" on the small black-and-white monitor. Every month, we received a cassette in our mail which we could play on the camera, and often, we received with great joy two or three copies of "Tatort" on one tape, usually a few weeks after the broadcast. Unfortunately – and often – the film lacked the end of the story because the tape had come to an end. Knowing this, there was even more suspense in watching. In that

year, which for me was more exhausting than any other because we were highly concerned about the health of our daughter, we enjoyed watching “Tatort” at night. That was leisure-cum-pleasure.

Conclusion

Thinking through notions of fieldwork and leisure makes us even more aware of the fact that this separation is hard to draw. While we were in the field, we did not actively seek ‘leisure’ time, maybe also because, in our view at the time, this would have been almost impossible to achieve. However, both the experience of being watched (Eveline) as well as being strongly embedded in social life (Frank) had consequences for our ethnographic work. In Eveline’s case, the awareness of being under observation made her more attentive toward her own self and embodied practices while in the field. It also complicated her own observations as an anthropologist, in that she tried to refrain from the colonial gaze and sought rather to engage in co-producing knowledge, challenging categories of the self and other. In this vein, she experienced the relationality of fieldwork in a particular way – as mutually observing each other and drawing conclusions from these observations. In Frank’s case, the host society followed their norms of hospitality and care around the clock. Leisure time, in their view, was always a collective activity, and since their leisure time constituted a social field which was of ethnographic interest, it did not become the ethnographer’s leisure time. This does not mean that Frank did not enjoy such moments or events, but it was not leisure in the sense that he was ‘off work’.

It is important to note that our experiences are drawn from particular settings and shaped by how fieldwork is defined and practiced over a specific period of time. For instance, in urban contexts, these experiences can differ fundamentally, not to speak of today’s approach to fieldwork as a joint cause between the researchers and interlocutors, often dissolving their clearcut roles. Mobile phones and social media networks have opened up other social spaces that are detached from the ‘face-to-face’ offline space, and thus other possibilities for leisure time, but also

for more ‘work’, have emerged – allowing us to switch easily to different worlds. In our fieldwork contexts in the 1980s and 1990s, however, there was not much *Feierabend* after fieldwork – at least not in the strictest sense of the term.

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Hiking Ethnography

A Short Essay on Fieldwork Mobility on Foot

Sabine Strasser

Introduction

As Tim Ingold and Jo Lee Vergunst pointed out in their “Ways of Walking” some 15 years ago, careful ethnographic analysis of walking “can help to rethink what being social actually means” (2016 [orig. 2008]: 2). Walking on two feet and talking to one another are features of our bodies that make us human beings. They allow us to relate and *go along* with others, human or non-human. Walking itself, Rebecca Solnit suggests, “is the intentional act closest to the unwilled rhythms of the body, to breathing and beating of the heart” (2022 [orig. 2001]: 5). It is so much part of our everyday practices that we often do not recognise it as a deliberate activity worth mentioning. Walking and talking has been part of learning and teaching, from ancient times to the present. Walking itself can take on different meanings and lead to religion and pilgrimages, science and expeditions or politics and demonstrations. Furthermore, it is universal and has particular meanings locally, whilst it also plays a major role in ethnographic fieldwork. There is probably no ethnographer who has not had to walk in order to attend activities taking place somewhere else, join research participants on their walking routines and think about past steps and future paths in collaborations. However, for a long time, walking as a technique of the body that can be taught and must be learned was sidelined (except by Mauss 1935), and it was activities upon arrival or the conversations en route that made it into the books and articles writ-

ten by anthropologists. Nonetheless, walking, as Ingold and Lee (2008) have shown, is worth reflecting upon because it allows us to focus on the senses, to compare the movements, sounds, sights and smells around us. Ingold (2018) distinguishes between intentionality – the will to do something – and a habit of simply doing something. In a nutshell, he posits “if the principle of volition renders a form of attention founded in intentionality, the principle of habit gives us a form of intention founded in attentionality” (Ingold 2018: 26). If volition and habit are decisive in rendering the experience of walking intentional and attentional, does hiking (founded in intentionality) then differ from walking, in that it requires volition to set a controlled speed, cover a certain distance or practice a certain technique? Does hiking privilege intention over attention to sensory responsiveness? Do hikes requiring preparation and equipment create intentionality and thus confuse attention that should otherwise be paid to listening, feeling and observing the body and the environment? If this were indeed the case, would we lose the very potentiality of the power of walking (attentionality) through the ambition of hiking (intentionality)? Or could ethnographic experiences of hiking lead us towards a more extensive and determined form of walking and thus increase our ability to be attentive?

In any case, the walking and hiking ethnographer is still under-represented in anthropology. The reason for this is that for most of its history, ethnography was constructed as a sedentary field of research until globalisation, transnationalism and mobility were introduced in the 1980s. Additionally, ethnographers prefer narratives about the people they walk with and the events they walk for. When anthropologists look at the learning of different modes of walking in different social contexts, they rarely think about their own involvement in this important matter. Nevertheless, the invitation to think about more than fieldwork, or about what ethnographers do when they live in the field, allows me to focus on their responsiveness and attentiveness when walking and hiking as part of and beyond their fieldwork. And since steps leave traces and create memories, we might also ask how walking and hiking connect ethnographers with the social, environmental and political dimensions of their field.

In the following, I first present some examples of hiking undertaken in the field by colleagues¹ at the University of Bern, Switzerland. I then describe my own physical and bodily experiences of walking and hiking in Turkey and in northern Pakistan. A tracking tour in the Karakorum contributed to a particular understanding of the country through the power of water and rivers, stones and mountains, ice and glaciers. Finally, I reflect on the potentialities of hiking for ethnography and ethnographers. Although hiking, as well as walking, “is a subject that is always straying” (Solnit 2022 [orig. 2001]: 8), it creates traces, memories and connectedness.

Walking in the field, hiking in the mountains

Walking, hiking and running are all considered (similar to drinking water and eating apples) cure-all practices in preventive medicine, helping achieve “a state in which the mind, the body, and the world are aligned” (Solnit 2022 [orig. 2001]: 7). Walking, as expected, is relevant in all of my colleagues’ fields, and depending on the research it is a means to take ethnographers to places where they want to meet someone, participate in an event, share experiences or follow in the footsteps of others. Hiking is not always differentiated from walking, and there is little to discern if it is either part of fieldwork – and even a prerequisite for respect, recognition and exchange – or detached from it. Walking and hiking can help wake up the body in the morning, reflect on experiences from a mountain peak or recover from the noise of a mega-city. Hiking, moreover, allows the ethnographer to appear busy and to escape expectations. In cities there are neighbourhoods in which you can walk or run without being observed and investigated, but in camps, villages or small towns

1 I would like to thank my colleagues Moslem, Manon, Manuel, Isabel and Maya for sharing their experiences, expertise and thoughts with me. They also commented on my account of their hiking experiences. It was wonderful to be able to perceive them as multi-sided ethnographers in this context.

you usually have to adapt to the rhythms of mobility along the lines of gender and age, and you have to justify any deviating steps.

For his research on gendered mobility and stuckedness on the Iranian border with Iraq, Moslem Ghomashlouyan studied a Kurdish mountain village (Ghomashlouyan 2023) where many of the younger men worked as Kolbars (porters). For them, their job required them to walk across the spectacular border mountains to Iraq to pick up heavy loads of contraband, take these packages back over the pass and then load them onto trucks close to the border for onward transport to cities living under various US- and EU-imposed economic and financial sanctions. Time and again, when Moslem asked to be taken to the border, the young men in the village mocked him for being a 'city boy' not strong enough to walk up the mountain. Without doubt, he had to get used to making his way around a village nestled on a steep mountain, which involved running up and down staircases and steep tracks for every little task. However, even after Moslem got used to walking in the village, as villagers do, he was still not allowed for a long time to join the men on the mountains. When he finally had the chance, he returned halfway from the border (not least for safety reasons and the PhD supervisor's urgent request). 'Going to the border' involved clandestine work and being threatened by border guards, police and intelligence services. For the young men, this walking business was an expression of pride, strength and masculinity. Later in life, they would often complain about back pain, which, as they told Moslem, in turn affected their sex lives. What was crucial for their male pride and economic success was also exactly what was destroying their bodies, their potency and thus their masculinity. 'Going to the mountains' was not a recreational activity in the village but (dangerous) work.

Moslem also avoided hiking in the mountains surrounding the village because of rumours he was a treasure hunter, i.e., someone who explores the mountains in order to dig for gold. Although there was no evidence that anyone had ever actually found hidden gold near the village, foreigners hiking in the mountains were nevertheless seen as suspicious in that respect.

Moslem hiked only once, towards the end of his fieldwork, and this was a dangerous adventure. He met a young woman who insisted on talking to him alone. In order to make this possible, they had to find a place where no one would interrupt them, and so she suggested the grave of a saint high above the village. She was the only person in the village who actually knew how to trek mountains. She even planned to climb Mount Ararat in Turkey: amazing, considering the fact that women live under strict gender segregation and surveillance. For this same reason, hiking to the holy sepulchre and meeting her there was a very real threat to this young woman and to Moslem's fieldwork. Every single step taken in this highly mobile village was controlled and had to be justified. Moslem learned to walk in this mountainous region, but neither intentional nor attentional hiking became part of his fieldwork, since it was either considered as 'going to the mountains' to work or not appreciated in the village.

Manon Borel makes a clear distinction between hiking and walking. For her, hiking since her childhood has meant going up a mountain and wearing hiking boots and a windbreaker. Hiking is at once emotional, relaxing, exhausting and allows one to experience nature from a different perspective. She hiked a mountain every weekend during her fieldwork in Gyumri, Armenia, but it also supported her ethnographic work, as it helped her align with her environment – geographically and psychologically. Therefore, hiking in the Armenian Highlands, in her view, was and was not part of her fieldwork examining the infrastructure of Gyumri, a town in the Armenian-Turkish borderland. She was following the remains of layers of destruction and reconstruction, ruins and rubble as well as refurbished buildings and sites in the city to understand their historical and political reverberations (Borel 2023). Climbing a mountain in Armenia – for her – was different to doing so in Switzerland, as it was both harder *and* easier. There are high hills in Armenia for which you do not need a map, as you can see the whole mountain and decide on a route to the top without trail markings. In Switzerland, everything is already marked and hiking is part of leisure activities and normal for almost everyone. In Armenia, Manon only noticed tourists hiking in the mountains guided by Armenians who had spent a long time in Europe. In

this region, people were surprised, sometimes stunned, when they saw someone in the mountains they did not know, but they also showed respect for the person's efforts. Locals would always use a vehicle, a car or a horse to get up a mountain. Even in the city, she observed that only people who could not afford private transport would walk, and everyone else would take the car for the smallest task, even in the immediate vicinity. On one of her visits to a border village, she wanted to combine a walk to a ruin with an interview about the history of the building. Nonetheless, before she could even say anything, her hosts had taken the car out of the garage to drive her just a few hundred metres. Local hospitality would not have allowed them to let her go on foot. While locals obviously avoided walking and had no concept of hiking, Manon would benefit from her hikes, which afforded her attentionality towards the landscape and people during her otherwise challenging ethnographic fieldwork.

Manuel Insberg grew up walking and cross-country skiing, and so he took his passion for walking, hiking and jogging very clearly and consciously into his fieldwork on refugees in Norway. At the beginning of his fieldwork, he would hike and run in the mornings, when his research participants attended their integration programmes – to which he still did not have access. Manuel is an experienced mountaineer and emphasises the difference between an emotional goal and a destination, between walking along or hiking alone. He describes different ways of walking, with and without a destination, sometimes going straight to the library and sometimes attentively exploring the city, strolling and roaming. After a while, he also met with research participants for walking interviews (Kusenbach 2003), learning about their views of the city and their favourite places, which revealed their (sometimes shattered) dreams for the future. He went with them when they met their integration officers and observed how the boundaries between leisure and research – not only in terms of walking – began to blur. This way, Manuel learned that the Norwegian way of hiking, '*Gå på tur*' (Go on tour), is part of the integration programme offered to refugees by civil society associations on a regular basis. *Go on tour* can also refer to a relaxed walk through the city while practising the Norwegian language.

During these walks and hikes, and in summer camps, Norwegians teach people how to hike, what to wear and how to develop skills for hiking and language – in short, how to become a proper Norwegian.

Like Manuel and Manon, who are explicit about their own interest in walking, hiking and jogging, and the routines related with these pursuits, Isabel Käser considers hiking an utmost ordinary practice, albeit a very useful one, in her fieldwork on the PKK women's movement (Käser 2021). What first came to Isabel's mind, when I asked her about walking and hiking in the field, was these women's pacing: they were always pacing, walking back and forth, up and down, in pairs – when they met journalists and strategic partners walking in a garden – up and down. They got used to this rhythm of walking in prison when they had very little space and their movement patterns protected secret information exchanges. Marching drills, military training, powerful singing of battle songs while walking – all these actions were also inscribed in their mobile bodies.

Isabel grew up in the Appenzellerland, a region in Switzerland known for its beautiful hikes. She has been in the mountains and at scout camps ('pretend' military camps without weapons, as she calls them) since her early childhood. Thus, it was no particular challenge for her when she had to walk to the training and education camps of her research participants, accessible only on foot. The walks were only about two hours, and she always felt quite protected. Guerrilla forces were observing the area from the mountaintops and would warn people in the valleys of attacks by the Turkish army. Although she did not find any of the walks challenging, commanders kept teasing her, expressing doubts about her tenacity and strength, due to her being a European civilian. In Isabel's opinion, this teasing was partly concern for her well-being but also an expression of the expected alignment to a life in the mountains. She explained: "Even the casual banter evolves a lot around bodily appearance (e.g. how skinny you are or that you recently gained weight), which links to how ready you are for 'mountain life'." Straight bodies perfectly trained for exercising, with particular rhythms always ready to get up and go, day and night – particularly in pitch darkness at new moon – this is the basis for survival. For this reason, female com-

manders, like their male counterparts, go through gruelling physical and ideological training, but women often feel like they have to outperform their male comrades. Those who survive the first years in the brutal war against the Turkish army become excellent mountaineers and knowledgeable about food, weather, routes, fire and water. But almost all of them have worn-out bodies and serious problems from carrying heavy equipment, a lack of healthy nutrition, camp beds, torture and prison. But they have to move on if they want to continue the struggle.

In the mega-city of Buenos Aires, with about 15.6 million inhabitants, Maya Ober often walked long distances from the bus stop with either design students or teachers who also aimed to reach the Faculty of Architecture, Design and Urbanism located in the University Campus of Buenos Aires (Ciudad Universitaria) on the northern outskirts of the city. Maya's research focused on feminist practices in design, and she often discussed feminist epistemologies and their application in education and practice with her research participants along this shared path. She exchanged – walking and talking – thoughts about design classes and about feminist interventions in the school. In her perception, she hiked or walked (she used them interchangeably) with the feminist activists in a natural reserve close to the campus and enjoyed breaks there – moments of slow breathing and escaping from the lively – sometimes strenuous – city and from intense institutional life at the design faculty. There, Maya often had discussions while walking in nature that made it into her notebook. Although she did not particularly reflect on the practice of walking or hiking in the nature reserve, these walks or hikes offered intimacy and privacy, which in turn promoted intense conversations. Similar to the meetings in the natural reserve, she also joined these activists on demonstrations organised by feminist or leftist movements in the city. Maya participated in these demonstrations for interconnected political, personal and professional reasons – as a friend, co-activist and ethnographer, and sometimes as a mother pushing a pram along the crowded marches and bumpy roads. These walks in the city were anything but an alignment of mind, body and world, but Maya enjoyed these strolls that helped establish and deepen relationships with activists outside the faculty. The crowds

offered protection from unwanted encounters, and the vibrant sound of the city allowed for intimate conversations. She was also not only walking and talking, but she was also there to align with her thoughts and her feet, to express a political will. Maya did not clearly differentiate between walking and hiking but rather between relaxation and political intervention on foot. For her, unlike everyone else, hiking without altitude was possible because she was making an effort and had a goal. Identifying very much as a ‘city person’, responsiveness to nature was quite unfamiliar, or at least irrelevant, to her.

In my own fieldwork on vernacular humanitarianism in the Turkish Aegean, I focused on experiences of the so-called ‘long summer of migration’ (2015/16) and explored the local impact of the deal between the EU and the Turkish government on activists and migrants on the move (Strasser 2022). Like them, I walked regularly over the hills and along the shorelines to understand the networks of solidarities, note the movements of Turkish and Greek coastguards and to identify spots where collapsed dinghies could be found. I walked with my research participants from the associations to collect data on the distribution of food, clothing or hygiene items. Walking also meant joining people in their forced mobility and embracing their experiences with my own body.

“Before we talk, you should first go up to the old, rotten houses on the hills of Agora [a neighbourhood in Izmir, where the poorest of the Syrian, mainly Kurdish refugees live], experience how steep these roads are. And see the despair when you get back home and realise you forgot milk and have to walk the same hill again. Go up there five times, embrace the people who live without running water, electricity, a refrigerator, sometimes even without shoes. Go up there, learn your lesson and we can talk!” (*Hakların Köprüsü* (Bridging People) activist).

For members of the refugee association *Hakların Köprüsü* in Izmir, working with refugees mainly meant *solidarity*, including strenuous walking in order to develop affective and attentive relationships. They rejected charity, which, in their opinion, creates inequality and superiority over locals. On the other hand, women on the frontline of the charity organisation *Bodrum Isanca Yasam – Bodrum Humanity* – also walked long hours through the hills of Bodrum to find people who were

hiding from the border patrol. They aimed to treat their wounds, show them the way to places where food was being distributed and provide them with shoes and clothes for the next stage of their journey.

Since my childhood, I had been used to Austrian Alpine hikes, but long walks in the Aegean did not mean hiking to me. Contrary of the contemplative hikes along marked paths in the mountains, walking in the Aegean hills had a highly politicised meaning that included hiding from police and border patrols, finding people in need and supplying them with vital provisions whilst on the move. I never longed to hike in the Aegean in order to enjoy the beautiful Mediterranean scenery and amazing views. Hiking for me involves relaxation, reflection and rhythm on a prepared several-hour tour, all of which was obviously not possible at that time when hundreds of people had died in the surrounding glistening sea. Combining research with hiking, however, was something I had experienced the year before. I had never been to Pakistan, and had never started hiking above 3,000 metres, when I decided to accompany Martin on his trekking tour to the high pastures of Shimshal (on Taghm in Shimshal, see Sökefeld 2018) in the Karakoram Mountains of Gilgit-Baltistan. In the next section, I focus on hiking experiences and bodily sensations in the north of Pakistan. Particularly, I discuss affects and effects that are rarely mentioned in ethnographic encounters because they expose personal anxieties, hierarchies and emotions. Used to travelling independently to and moving around in my own research fields, extensive hiking in a new and breathtaking environment with an experienced colleague pushed me (unexpectedly) into a role similar to representations of 'wives' in early anthropological encounters.

Walking a glacier in the Karakorum

This story begins with walks in the humid heat of the Islamabad summer, with which Martin was familiar. I also had to adjust quickly to the sheer density of bodies in Rawalpindi, which forced me to negotiate my inflexible body clumsily through an astonishingly flexible environment. The first really stressful experience was caused by the huge amounts of

Pakistani traffic, which really made me feel and act like a stranger on the drive from Islamabad to Abbottabad. The driver of the car immediately recognised my anxieties and confirmed my feeling of alienation by asking Martin with a smile if I was German. *No, I am from Austria*, a child inside me protested, but she was silenced immediately by the traffic that demanded my full attention. Martin seemed to be stronger than usual, masculine and supported by other men, among whom I felt quite awkward. Gender relations supported him and made my way of fearing for our life seem strange and cowardly.

Adventure did not quite match the experience, though, when – a couple of hours later, shortly after the Babusar Pass at an altitude of more than 4,000 metres (13700ft) – the car's engine failed and thus could no longer support the braking manoeuvres required for the steep serpentine-like road that twisted down to the Indus valley. I would definitely have preferred to walk down this mountain road, but I knew it was way too far. At least we didn't need an engine when rolling downhill, and so we kept going until the car stopped in the middle of a sandy nowhere on a small hill on the way to Chilas. I could see towers dotted across the landscape which, according to Martin, had been set there to protect people from blood feuds. I saw men who seemingly appeared from nowhere and started to form a circle around the car, curious to see who had got stranded close to their village. I began thinking about Ruth Behar's "The Vulnerable Observer" (1996) in a different way, trying to be invisible by not looking around and keeping my eyes lowered. After a while, Martin took me to a construction shack close by, in order to protect me from the men's gaze, and, without anyone asking me, I covered my face. Walking to this shack was a relief after hours in the car, and yet the mind, the body and the world were all but in alignment at that very moment. Although the men from the village proved to be extremely friendly and pushed the car to the next ridge, from where we could roll further down, I felt exhausted from the immobility and the waiting. I had learned my first lesson about the power of altitude and a local variation of gender segregation, and shortly thereafter I made acquaintance with a nasty virus in Chilas. Broken brakes and traffic were no longer my main concerns.

As we drove through the Indus Valley the next day, I saw the remarkably powerful river with huge waves and whirls in the muddy water. I began to understand a line from a book in which a man who had fled the Indian Partition looks sadly at the beautiful Bosphorus Strait in Istanbul and describes the quiet water there as a prisoner compared to the Indus. The power of the rivers in Gilgit-Baltistan made me feel fear and excitement simultaneously – a bodily sensation I got used to over the next few weeks when walking across them. We caught our first glimpse of Nanga Parbat and had breakfast near a sign indicating that the huge mountain ranges of the Karakoram, the Hindu Kush and the Himalayas met there. Later, at the Karakoram Highway, we had tea in the face of the almost 7,800 m high Rakaposhi, with no mountain between this magnificent beauty and us. Before I start sounding like a tour operator, in describing my arrival in the land of Hunza and Nagar, I would like to return to my growing understanding of mountains, landslides, rubble and water while preparing for our hike in the Karakoram.

In Hunza, we drove along the highway from Karimabad to the small town of Gulmit, for which we had to cross the Attabat reservoir in a small boat. We had to share this boat not only with many people, but also with motorbikes and our car, which stood on two wooden planks and protruded over the boat's edge. The reservoir, which I knew from Martin's work (Sökefeld 2020), had been created by massive landslides that destroyed parts of Attabat village and the Karakoram highway and also dammed the Hunza River. I understood well after a couple of days why Martin spent so much time there and was captivated by the power of mountains and rivers in the region. Personally, I was rather fascinated by the manifold but always noisy interactions between water and stones. While Martin was talking to people and conducting interviews, I had time to look at myself arriving somewhere and getting bodily acquainted with new rhythms and routines, sounds and views. After a couple of days of adaptation to the altitude, facilitated by walks along the steep hills and through the lovely apricot gardens of Gulmit, we planned to move on to Shimshal the next morning, and so we organised a car and went to bed early. In the evening, we were surprised by drizzle, a rather rare occurrence in that stone desert at the time. Since Martin

didn't seem to be worried, I had almost forgotten about the rain the next morning when I woke to a splendidly sunny day. We were therefore quite nonplussed when we found out that this tiny amount of rain had actually caused the closure of the highway to China and had covered the small road to Shimshal with about 15 landslides. Determined to carry out our plans, however, we decided to climb over the hills created by these landslides, accompanied by some villagers who had the same goal. After having crossed four of the huge gravel mounds, we arrived at a bridge that had been destroyed by a rockfall and had fallen into the river. On hearing the tangled debris scraping against itself in the river, about ten metres below us, Martin and I briefly looked at each other, shook our heads and determined not to jump to the other side. We said goodbye to our companions, despite them trying to convince us by all means that it was quite safe to jump to the other side, by jumping back and forth in front of us. My body responded with a clear *no!*

We cancelled Shimshal for that year and decided to do a four-to-five-day trek across the Patura Glacier again, to see how pastoralism had been transformed. We walked with two young men from Passu, namely Habibullah and Ali Sifa, whose families Martin had known since their childhood and who had spent their childhood with their grandmothers in the high pastures. As our guides, they would earn money and pass the time a bit in their long, drawn-out summer. They walked in front of us, disappearing at times and then showing up when needed, such as when it was time for soup, establishing the tent together or helping me over difficult or even dangerous obstacles. My walking rhythm and breathing were clearly different from their extended strides and breaths. They preferred Urdu to English and thus mostly talked to Martin, rarely to me. Nevertheless, they were terribly patient and friendly with me, since I was coughing and gasping a lot as a response to the altitude. They reached out to me when I needed them and let me walk step by step, listening to the glacier's ice and stones passing beneath my feet. With the increasing passage of time, I came to appreciate the feeling of lightness from the extensive hikes and looked forward to the approaching landscapes. Only when we had to cross a raging river on a few branches did I seriously doubt our undertaking. After looking around the bend, I knew that if I

fell, I would disappear into a glacier cave. The river was small compared to the Hunza River but still quite strong. I took a deep whistling breath and walked across. Habibullah told me that there was no other obstacle like this for the rest of the tour, yet I was aware that we would have to come back to this makeshift bridge on our return. The tour was long and exhausting for me, but it was also full of joy, thanks to the beautiful mountains around us, the huge glacier tail, the alpine pastures, the giant and yet so timid yaks and the vast and nimble flocks of goats.

When we reached the high pastures, we met two men from the poorer mountainous areas of Chitral who had been hired as shepherds. Hundreds of goats gathered around our tent in the evening as they scrambled to their sheltered sleeping place. Despite the noise made by the goats and the glacier, I fell asleep immediately, as I was just too tired to care about any possible threats. The next morning, I felt happy and proud when I was awakened by the same sound. I observed Martin taking pictures of the pasture, until recently the summer residence of the village women, and interviewing the shepherds. I was amazed that the villagers apparently walked these trails regularly and without much effort as part of their semi-nomadic pastoral economy. Later that day, the young men caught a partridge on the way and carried it injured over their shoulder to prepare a meal in the evening. When I saw that the other – much more heavily equipped – hiking group we had met on our tour had destroyed the makeshift bridge, I once again become painfully aware of the glacier cave behind the bend, and the power of nature.

We spent one last night under the starry sky before we started our descent along the little sandy trails back to Passu and the highway. The photos we took with Ali-Sifa and Habibullah in the village show the deep joy and unconcealed pride of all of us, I guess for very different reasons. I remember that I thought about many colleagues and friends and wondered what their walks had meant to them, to their research participants, their environment and their research. That was the moment when the idea for an essay on bodily experiences, intentions or attentions and on alignment with new landscapes was born. It has taken me almost eight years to find an opportunity to run with this idea and to start “poking around in a basket of older and more recent memories –

visual, auditive, tactile and visceral" (Tošić 2020, 22), in order to ask about affective, physical and scientific experiences while hiking. But what would have been a better occasion than Martin's *Festschrift*?

Coda

I have followed different ethnographers' intentionality and attentionality, probing their walking and hiking as part of and beyond their fieldwork. Walks and hikes provide access to places that can only be reached on foot, such as guerrilla camps and high pastures, as well as secret meetings. Additionally, they help fulfil local people's expectations, such as attendance at demonstrations or on instructive programmes. On the contrary, they can also lead to surprises, such as Manon's contemplative walks to distant ridges sometimes did. Feet also leave traces of unwanted meetings, dangerous places, prescribed education, political goals and violent encounters, thus creating memories and connecting the ethnographer with their field in various ways. Furthermore, the repeated footsteps engender affects, sensations and experiences, in addition to new scholarly insights, and thus they are more than ethnographic fieldwork. Often, research participants do not frame hiking in the same way, and sometimes they do not even have a word to describe it. In the Iranian borderlands, people do not consider the work of Kolbars as hiking but as going to the pass or over to the other side. Guerrillas march, jump or creep. People of the Armenian highlands use their car or ride a horse to avoid walking in the city or hiking to the top of a mountain, and feminist activists who protest with their feet in Buenos Aires see social transformation as the goal of walking. Refugees hardly see their walks in the hills of Bodrum as relaxing, when the rhythm is prescribed by others and their bodily sensations indicate stress. Many Norwegians do hike, and they obviously think that this physical technique is so much part of their identity that they have to teach it to future co-citizens, to make them fit for society.

Thus, it is not the difference between hiking and walking that creates the distinction between the will based on intention or the habit based

on attention. On the contrary, both walking and hiking enable the alignment of the body to the landscape, to listening, observing and feeling. Both may simultaneously feed into either the tourism industry or into anti-capitalist attentionality. However, only hiking is associated with national imaginaries that also shape the skills, will and practices of ethnographers. Besides relaxation and attention, hiking is also a 'happy object' of national and alpine identity with the potential of differentiation and exclusion as well as the need for critical reflection.

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Assembling Bits and Pieces

From Patchwork to Ethnography

Beatrice Odierna

I think I ought to say something in Favour of Patch-Work, the better to recommend it to my Female Readers, as well in their Discourse, as their Needle-Work.

Barker 1723: iv

When musing about the many sides of the ethnographer in the field, all of which inspired this Festschrift to celebrate Martin Sökefeld, a thread began to spin. I found myself thinking about my relatively new-found interest in patchwork and quilting. I see it as intimately connected to the genesis of my research, and, at the same time, I consider it an outcome of the particular research and living conditions I encountered when starting my fieldwork in the wake of the coronavirus pandemic. So, how did I get from patchwork to ethnography, or, rather, into patchwork while doing ethnography – as the two crafts might be even more related to one another than one would expect at first sight? Following the ‘multi-sided’ researcher thread, I would like to trace how, in the context of my fieldwork with so-called ‘refugee women’¹ in Germany (2020–22), patch-

1 In the following, I use the description ‘women addressed as refugees’. This denomination is the result of an ongoing struggle of ‘appropriately’ addressing and representing my research partners. In the course of many conversations

work became not only a private interest and creative activity, but also a methodological point of reference and, subsequently, a research lens – thus rather casually transcending the boundaries between the realms of ‘private’ and ‘professional’ life.

Patchwork as a metaphor for women writers

For a long time (especially in the North American contexts but also in some European countries²), patchwork and quilting³ were perceived and represented as ‘good’ activities for women and connected to the domestic

with interlocutors sharing the experience of being addressed as refugees, even years after their arrival in Germany, it became clear that they themselves do not (anymore) use ‘refugee’ as a self-description and sometimes also severely criticise the usage of this description by others. Furthermore, the terms ‘refugee’ and ‘refugee women’ are often used (also within interdisciplinary and anthropological research literature) without specifying if one is referring to a legal category (e.g., according to The Geneva Convention) or a social status. Therefore, following Liisa Malkki, who reminds us to take ‘refugee’ not as a given category for anthropological research (cf. Malkki 1995: 496), I use the term ‘women/people addressed as refugees’. This may not be a perfect solution, but at least it is an attempt to highlight the controversial character of the ‘refugee (woman)’ category.

- 2 There exists a wide range of quilting traditions in various parts of the world (e.g., *ralli* quilts in Pakistan or the *sashiko* method in Japanese quilting).
- 3 ‘Patchwork’ and ‘quilting’, though often used interchangeably in academic literature as well as in everyday language, refer to related but different textile handicraft techniques. Patchwork usually describes the practice and, simultaneously, the end product of piecing together small pieces of fabric (mainly cotton). As these are often remnants from other sewing projects, like a dress or a shirt, patchwork is primarily used as a way of recycling. It carries the connotation of both accepted imperfection and creative creation. Quilting, in contrast, involves sewing patterns on a so-called ‘sandwich’ consisting of background fabric, filling and cover fabric (often a patchwork) to hold the three layers together and also add decoration. In some cases, quilting is used deliberately to break up the pattern of patched fabrics, e.g., by using contrasting colours of yarn (see also Stalp 2007: 14 for a more detailed definition).

sphere. However, an interesting twist was added to the association between all sorts of textile handicraft (especially needle-work) and female labour when a connection was made between the patchwork method and women's literary output.

In 1723, Victorian writer Jane Barker argued in favour of patchwork as a replacement for more detailed, exhausting and time-consuming methods of embroidery as an occupation for women (cf. Barker 1723: viii).⁴ As Audrey Bilger shows in her article on patchwork and women novelists, in this context stitching and sewing, especially involving complicated embroidery, were often required from women (of certain class backgrounds) to keep them busy at home as well as to prepare their hands and minds for their later obligations as mothers and housewives (cf. Bilger 1994: 19 f.). According to Barker's argument in her introductory section addressing the reader, patchwork is a much quicker and less exhausting way of putting together a larger work:

I am glad to find the Ladies of This Age, wiser than Those of the Former; when the working of Point⁵ and curious Embroidery, was so troublesome, that they could not take Snuff in Repose, for fear of foiling

4 It should be noted that quilting in Europe is intimately interconnected with European colonialism: the most famous example is provided by early quilting in England, which was highly dependent on and profited from forced cheap fabric imported from the Indian subcontinent (cf. Bilger 1994: 18). This is mirrored by the production chains that support today's European or North American quilting industry: many textiles and sewing accessories are produced in countries (e.g., China or Bangladesh) with a low average income for the workers involved, while being sold relatively cheap to the consumers elsewhere. Furthermore, many of the workers employed in the textile industry are women (e.g., in Bangladesh, over 80% are women, cf. Kabeer/Mahmud 2003: 95). This stands in harsh contrast to the often claimed 'sustainability' of patchwork as a way of recycling – which is also contradicted by the fact that many quilters buy and use special fabrics that are made only for the purpose of quilting/patchwork.

5 Here, the author refers to the time-consuming technique of needlepoint stitching usually used to decorate cushions, handkerchiefs, napkins, etc.

their Work: But in Patch-Work there is no Harm done ; a smear'd Finger does but add a Spot to a Patch, or a Shade to a Light Colour. (Barker 1723: viii)

While proving to be less exhausting than embroidery and stitching – which she considered “pernicious to the Eyes” (Barker 1723: viii) – patchwork as a creative method also offered (mainly bourgeois and/or wealthy) women the possibility to spend more time on other activities, e.g., writing. Barker tried to reconcile the allegedly opposing crafts of sewing and writing, and she also advocated novels as an acceptable genre for women writers in particular (cf. Bilger 1994: 24). Within her own novel, which follows the introductory address to the reader cited above, Barker’s heroine Galesia struggles as a writing woman at a time when – and in a societal context in which – women were bound to the domestic sphere. In the end, however, she finds a way of presenting her writing as part of the novel’s eponymous ‘Patch-Work Screen’ – literally sewing together her written stories and poems as a patchwork. Here, Barker presents, (probably) for the first time, patchwork as a metaphor for women’s literary production (probably) (cf. *ibid.*: 32). This resonates within today’s feminist literature studies, wherein patchwork has once again provided a well-established metaphor for women’s writing since the 1980s (cf. Torsney/Elsley 1994: 3). In this regard, for example, Elaine Showalter argues for the US-American context that “a knowledge of piecing, the technique of assembling fragments into an intricate and ingenious design, can provide the contexts in which we can interpret and understand the forms, meanings, and narrative traditions of American women’s writing” (Showalter 1986: 227).

Patchwork as a methodological point of reference

However, not only with regard to women’s literary production, but also within social anthropology, patchwork has been used as a metaphor to refer to modes of researching and writing. For example, in her monograph *Friction: An Ethnography of Global Connection* (2005), anthropologist

Anna Tsing highlights her way of investigating ‘friction’ in global connections as “patchwork ethnographic fieldwork” (ibid: x). With regard to her research interest in global connections, she was dealing with such a wide range of social groups that she could not work with all of them in depth. Thus, she decided to move back and forth between different ‘chains’, zoom in on particular “zones of awkward engagement” (ibid: xi) and finally ‘patch’ them all together:

On the one hand, I was unwilling to give up the ethnographic method, with its focus on the ethnographer’s surprises rather than a pre-formulated research plan. On the other hand, it is impossible to gain a full ethnographic appreciation of every social group that forms a connection in the global chain. (ibid: x)

Another kind of ‘patchwork ethnography’ gained popularity in the context of anthropologists’ struggle to do fieldwork and defend their changed methodological approaches, especially during the coronavirus pandemic (see e.g., Günel et al. 2020; Fratini et al. 2022). In 2020, Gökçe Günel, Saiba Varma and Chika Watanabe issued a *Manifesto for Patchwork Ethnography* (Günel et al. 2020⁶) wherein they argue that the ideal of conducting long-term fieldwork at a designated place has long been questioned (cf. ibid: paragraph 1; see also Gupta/Ferguson 1997). While referring to multiple anthropological strategies to follow up on the needs of research partners, e.g., by following their movements in the course of multi-sited or transnational ethnographies, Günel et al. stress that the needs of the ethnographer are rarely taken into consideration (cf. Günel 2020: paragraph 2). Here, gender enters the stage in a different way: in reference to feminist critiques of artificial separations between ‘the private’ and ‘the professional’, the authors refer to the importance of care obligations that concern many (not only) women anthropologists but which are usually not taken into consideration in research designs.

6 For more information on the research project and associated activities and collaborations with other researchers, see their website <https://www.patchworkethnography.com/>

As a consequence, people/women with care obligations experience more difficulties in conducting ethnographic research – not exclusively, but certainly under pandemic research circumstances. To counteract some of these problems, the authors suggest a ‘new’ form of researching – ‘patchwork ethnography’ – which they define as follows:

Patchwork ethnography begins from the acknowledgement that recombinations of ‘home’ and ‘field’ have now become necessities—more so in the face of the current pandemic. By *patchwork ethnography*, we refer to ethnographic processes and protocols designed around short-term field visits, using fragmentary yet rigorous data, and other innovations that resist the fixity, holism, and certainty demanded in the publication process. Patchwork ethnography refers not to one-time, short, instrumental trips and relationships à la consultants, but rather, to research efforts that maintain the long-term commitments, language proficiency, contextual knowledge, and slow thinking that characterizes so-called traditional fieldwork. (ibid: paragraph 4, original emphasis)

However, in contrast to the recent appropriation of patchwork ethnography, especially by scholars trying to make sense of how they struggled with their research during the pandemic (e.g., Fratini et al. 2022), it might be argued that it provides a useful perspective, not only with regard to ethnography in/of the pandemic, but also beyond, as the authors touch upon (once again) urgent issues of how to design and conduct anthropological fieldwork. It might even be argued that patchwork might provide a useful metaphor for ethnographic research on a broader level, as I elaborate in the following.

Patchwork and ethnography – two related trades?

According to Maura Flannery (2001), many similarities can be found between quilting and science in general. In the following quote, she attempts to highlight these comparisons to strengthen her argument that quilting should be considered a useful metaphor for scientific inquiry:

In the case of the metaphor of science as quilting, quilting can be seen as a way to create order out of a multiplicity of pieces, just as science is discovering the order that underlies the multiplicity of phenomena that confront us daily. A research project often involves ideas and information that have been around for years but that may be used in a new context in the present work. Such a project is a patchwork of techniques and pieces of information that may have been gathered at very different times and in very different contexts but that happen to fit into the solution of the problem at hand. Scientists and quilters both spend their time trying to fit pieces together to make a pleasing whole, and often, this involves playing with the pieces, rearranging them to make them fit and to allow them to be used most effectively. (Flannery 2001: 633)

Flannery also repeatedly makes the point that establishing patchwork as a metaphor for scientific inquiry would be preferable to male-connotated metaphors such as “wresting [sic!] knowledge from nature” (ibid: 631), which in turn may also help to attract more women “to do science” (ibid: 630). This, however, is a somewhat problematic argument, because while presenting patchwork as a classically female occupation drawing on tacit knowledge and creativity, it presupposes that women do science in a different way, e.g., with more emotional empathy than men. Here, Flannery risks reproducing homogenising stereotypes of women as care-givers and emotion-driven while trying to argue in favour of a decidedly feminist scientific inquiry. Moreover, she ignores the contemporary (and by now well-established) critique of mainstream feminism’s appropriation of the category ‘women’ as neglecting the different contexts and varieties of female experiences (and struggling) (see e.g., Mohanty 1988).

Nevertheless, it seems worthwhile reconsidering her description of the similarities between quilting/patchwork and scientific inquiry – which heavily draws on her own background in biology – in relation to ethnographic research. It might even be argued that the metaphor works better with regard to the trade of ethnography than it does in reference to more deductive research approaches. Some representatives

of the natural sciences (but also some parts of sociology, see critically e.g., Smith 1987) claim to be doing ‘objective’ science. Ethnography, in contrast, aims to discover inductively ‘the unexpected’ in the field (in its broadest sense). While incorporating irritations and mistakes into its epistemology, the ethnographic researcher relies on rigorous data documentation and comparison – and last but not least, questions her/his impartiality at any time during the process (see Haraway 1988). Similar to their relevance for patchwork as a material practice, accepted imperfection and creative creation can be considered as guiding themes of ethnographic research and writing. In fact, a larger pattern or a central thread that connects the bits and pieces might become recognisable only in the process of *doing* both patchwork and ethnography.

Patchwork as a creative activity in challenging (research) times

In the course of my research, patchwork became important to me, not only as a methodological point of reference owed to the particular temporal setting of my research, but also as a haptic creative activity which in turn shaped and determined my research interests as well as the ways in which I engaged with my data – well beyond any pandemic conditions. Looking back, patchwork entered the scene at a very early stage. At the beginning of 2020, I was set and ready to start my PhD fieldwork in the context of refugee camps and the activities of social workers designated to host and support people addressed as ‘refugees’.⁷ Driven by the desire to learn more about the subjectivities and societal treatment of young women sharing the experience of being addressed that way, I planned to dive into my field and conduct interviews and extensive participant observation. Unfortunately, the plan did not quite work. In March 2020,

7 The PhD is part of a project on the ‘Processes of Subjectivation and Self-Formation of Young Women Addressed as Refugees in Germany’ based at the Institute of Social and Cultural Anthropology, LMU Munich, funded by the DFG (2020–23).

the coronavirus hit many parts of the world, including Munich and upper Bavaria, where I lived and had planned to conduct my research; a struggle with lockdowns and contact restrictions followed. Due to these rapidly changing research conditions, I had to adapt and reconsider my original research plan. For example, as face-to-face meetings were almost impossible, at least at the beginning of my fieldwork, I shifted to online tools to keep in touch with my research partners and also to reach potential interlocutors. Furthermore, we made long telephone calls, kept in contact via messaging and tried to meet up in our homes and flats or outdoors as often as possible. In the long run, these recurring and extended informal conversations and interviews with individual women became much more central to my methodological approach than I had originally expected.

While the pandemic forced me to restructure my fieldwork and made contacting and meeting my interlocutors very difficult, it simultaneously became mandatory to find some kind of occupation to fill “Those long, dark, lonely evenings” of the lockdown(s) – as one of my research partners once called it. However, although I was interested in all kinds of handicrafts before, and had worked with several different materials and techniques, I only started sewing when I caught Covid myself. While the following three months were a personally challenging time, sewing and patchwork helped me get through and kept me occupied.

In the aftermath, however, I continued my autodidactic journey into the world of patchwork: this included watching YouTube tutorials on how to cut and prepare the fabric, assemble and sew certain patterns, adjust a quilt ‘sandwich’ and successfully finish a quilt with an appropriate ‘binding’. Simultaneously, I started reading up on the history of particular patchwork and quilting patterns, such as ‘the lone star’ or ‘the carpenter’s wheel’, as well as different quilting traditions and also took lessons from the mother of a friend who had been practicing the craft for decades. While she showed me her newest piece, which she was slowly quilting by hand in a special wooden frame, I was also introduced to the schism between advocates of hand-made vs machine-supported quilting – and the connected struggles to combine notions of ‘authen-

ticity' or 'real' quilting with the temporal demands and restrictions of (mostly women) quilters' everyday life.

At the same time, I started talking about my growing fascination for this craft with friends and with some of my research partners. When chatting on the phone or face to face on a walk, we covered many subjects – not only 'official' research topics and questions, but also our daily struggles of getting through the day and managing work and private obligations as well as how to occupy and distract oneself in the face of social distancing.⁸ Furthermore, also some of my interlocutors were either themselves engaged in particular forms of handicraft or knew people who did. Thus, the simple exchange on how to spend their newly acquired timeslots sometimes resulted in lengthy conversations on this or that particular kind of stitching or type of patchwork which they knew themselves or which was practiced by acquaintances all over the world. Furthermore, we had intense discussions on the problem of recycling and the unjust conditions of textile production as well as the elitist character of 'creative' and leisure time activities such as sewing (e.g., who can or cannot afford the time, money and space to spend on such a 'hobby' vs sewing as an economic necessity). While I initially thought that these topics were not directly related to my research questions, the opposite

8 Between March 2020 and summer 2021, public life in Bavaria, as in many other parts of the world, was restricted repeatedly due to the quickly developing pandemic. However, my interlocutors and I were affected by the Corona regulations in different ways, very much influenced by our social positions intersecting with our legal situations as well as economic possibilities – and connected to such different factors as age, country of birth, etc. While most of my interlocutors addressed as refugee women were living with their families or in student apartments, and others still lived in camps, I could afford (and enjoyed the legal right to) live in a private flat – as many of my interlocutors from a Social Work background also managed to do. Especially in the context of camps, the situation was very difficult, in that the ever-changing official Corona restrictions were accompanied by rules differentiating from camp to camp. In some cases, the whole camp went into quarantine when one case appeared; in others, only floors were closed down. As many people fell ill one after another in her camp, one interlocutor told me she had been living in quarantine for months with virtually no interruption.

turned out to be true. While I became more and more aware of the increasing presence of sewing and other textile activities in the field, our conversations prompted me to question how these practices are embedded and interwoven with gendered, class-related and racialised dimensions of the everyday life of both researchers and research partners.

Patchwork as a research lens

Thus, while getting into patchwork as a form of “serious leisure” (Stalp 2007: 9), I started reading up on the topic and discovered that both quilts as material products and patchwork as a textile practice have long been a subject of anthropological inquiry (separately from its aforementioned usage as both a methodological approach and a metaphor). Anthropologists studying patchwork and quilting focus on a variety of different issues, including the significance of quilts in ‘Native American’ mourning rituals (Cariocci 2010), the importance of everyday quilting as a practice of self-care and self-fulfillment for middle-aged American women (Stalp 2007) and investigations of quilting conventions from the perspective of an ‘acafan’ (an academic who is also a fan) (Barrus 2021).

While Marybeth Stalp (2007) highlights the importance of the recent ‘quilt revival’ in the US-American context, in Germany quilting and patchwork are also becoming quite common leisure activities. Within most bigger cities, as well as some rather remote towns, there exist specialised shops and active patchwork communities consisting of numerous individual practitioners as well as regional groups and so-called patchwork or quilting guilds⁹ that also organise recurring exhibitions and conventions. However, up to now, not much (if any) anthropological research seems to have been done on this topic in the German-speaking context.

Among the many different anthropological approaches to investigating patchwork as a practice, Nancy Scheper-Hughes’s article on the Gees’

9 See, for example, ‘The German Patchwork Guild’ webpage (<https://www.patchworkgilde.de/>).

Bend Freedom Quilting Bee stands out, as it transcends the often-supposed boundaries between activism and research. Retrospectively, she describes her feeling indebted to a group of African American quilters that once took her in and supported her in an early research project. She describes her engagement and struggle to get these women's quilts accepted as art in the context of the North American art scene of the late 1960s (cf. Scheper-Hughes 2003). Here, Scheper-Hughes gives a very personal testimony of the changing representation of African American quilters within (North) American quilt history, as she experienced it as both a researcher and a friend of quilters from the Gees' Bend Quilting Bee. For a long time, this history was shaped by omitting African American quilters from official narratives, e.g., in dedicated quilt histories published since the early 20th century (cf. Klassen 2009: 298), as well as from the multi-million dollar quilting industry¹⁰ (cf. Stalp 2007: 8). Only recently has African American quilting been acknowledged as a part of this record (cf. Klassen 2009: 299 ff.), and it has even found its way into inter-/national art museums.

However, the construction of 'African American quilting' or 'Native American quilting' as distinctively different to '(mainstream) American quilting' can be criticised. First, quilters addressed by either of these labels respond to 'trends' in mainstream quilting in their own patchwork and quilting practices (ibid: 298). Second, patterns claimed to have originated in this or that particular quilting tradition¹¹ are constantly exchanged (ibid: 298), rearranged and altered, thereby making any patch-

10 The selling of countless books on patchwork and quilting, fabric collections for different occasions and patterns for a variety of patchwork techniques is by now quite a lucrative sector of the arts and crafts' economy, not only in North America, but also in other parts of the world.

11 One example of this kind of sometimes very ambivalent appropriation might be provided by the ubiquitous log cabin pattern, which – consisting usually of three rows of angular logs in light and dark colours arranged around the sides of a red square that symbolises a fire – is claimed as both an invention of 'settler-colonists' on their march to the American West as well as a symbol of the underground railroad helping people escaping slavery to find their way to the safe North, where slavery had already been abolished (cf. Stalp 2007: 7).

work quilt both a citation of pre-existing patterns or historical quilts *and* a product of individual manual labour and creativity.

With regard to my research, another aspect of textile handicraft became of interest, namely the importance of women's (manual) work for humanitarian activities. For example, in her monograph *The Need to Help* (2015), Liisa Malkki considers the engagement of mostly Western women in the production of material objects of aid, such as hats, shawls and knitted cuddly toys (e.g., 'aid bunnies' or 'trauma teddies') for the victims of war or natural disasters *elsewhere* (cf. Malkki 2015).

In her investigation, Malkki convincingly shows how these hand-made objects become emotionalised and considered essentials of any humanitarian aid – especially, but not only, with regard to humanitarian activities in other countries. For example, Malkki refers to the carrying and distributing of 'trauma teddies' by the Red Cross in the context of numerous catastrophes such as the World Trade Center attacks in 2001, tsunamis following an earthquake in the Indian Ocean in 2004 and the Russian bombing of an airplane in Ukrainian airspace in 2014 (cf. Malkki 2015: 108). As products of a decidedly voluntary humanitarian engagement, these objects of aid are meant to soothe the pain and fear of children affected by disaster. Here, children represent both the pure victim and the "exemplary human" (ibid: 9), and thus they ennoble the voluntary production and giving of bunnies, hats, teddies and the like as morally 'good' and benevolent practices. At the same time, these activities can be considered highly gendered, as mainly women do (and are called upon to) produce these objects of aid (ibid: 106).

Similar enterprises became also quite common following the arrival of more than a million people fleeing war and disaster in 2015/16. Rapidly, women – especially those in their fifties and sixties (among them also some of my relatives and acquaintances) across Germany – started to engage in all kinds of textile handicrafts, e.g., knitting, sewing and so forth, to produce objects of aid 'for the poor refugees'. These activities included the production of countless hats and shawls as well as pullovers, with sometimes hundreds of pieces pro-

duced per person.¹² This type of occupation was further encouraged by fundraising campaigns; the by now annually repeated campaign “Shawl for Life” (in German: “Schal für das Leben”) in favour of ‘Syrian refugee children’ can be considered one of the most prominent examples of this kind of campaigning at the intersection of individual and corporate humanitarian commitment. The campaign was originally initiated by the well-established German women’s magazine *Brigitte* and the NGO Save the Children e.V. in cooperation with Lana Grossa, a wholesale trader specialising in the distribution of all sorts of wool and yarn, and it is still running today (Spring 2023).¹³

However, in subsequent years, and again in the context of the pandemic, comparable connections of gender and handicraft kept reappearing. For example, while at the beginning of the pandemic in early 2020 nobody had any masks, people started to sew them themselves from small pieces of cotton, and also offered them to others as one of very few known preventive measures against COVID. In the beginning, inhabitants of refugee camps were targeted as beneficiaries of this kind of aid. However, after a short while, increasingly more social organisations started to offer sewing opportunities, especially to refugee women, to learn how to make these masks and make them accessible to the public, often advertised under the label ‘they want to give something back’¹⁴. Thereby, these organisations were participating in the subjectification (cf. Butler 2015) of particularly women addressed as refugees in an ambivalent way – both as aid recipients and at the same time ‘good’ and ‘grateful’ (future) members of German society.

12 See for example “Bunte Mützen, damit Flüchtlinge nicht frieren,” 18.02.2016 (<https://www.shz.de/lokales/eckernfoerde/artikel/bunte-muetzen-damit-fluechtlinge-nicht-frieren-41555818>).

13 For more information on the campaign, see e.g., “Alles zur BRIGITTE-Aktion ‘Schal fürs Leben’”, February 02, 2023 (<https://www.brigitte.de/leben/wohnen/selbermachen/stricken/schal-fuers-leben/>).

14 See, for example, “Flüchtlingsfrauen nähen Masken”, April 29, 2020 (https://www.berliner-woche.de/charlottenburg/c-soziales/malteser-verbinden-integration-und-corona-hilfe_a263650).

Fittingly, many of the organisations I worked with over the course of my fieldwork developed special activities for refugee women that both drew on and reproduced the long-established imagination of (manual) care work in the domestic sphere as women's domain. At the same time, however, they complemented this presupposition with the assumption that refugee women are eligible (and willing!) to work in these areas. For example, in many cases, creative activities such as sewing and other forms of needlework were connected to the aim of empowerment. By offering courses on machine sewing and fashion design under the empowerment label, and opening up these avenues only to women, occupations such as tailoring were presented as desirable areas of work for women addressed as refugees.

In this way, Social Work contributes to gendered ways of integrating refugee women into the German labour market against the backdrop of the activating welfare state¹⁵ (see e.g., Kessl 2019). Here, Social Work actors take over the mission to prepare women addressed as refugees to become 'good' working members of society – but only in areas of occupation that are deemed adequate for them as both women and refugees.

Conclusion

As we have seen, patchwork and quilting provide well-established metaphors for women's novel-writing, (feminist) scientific inquiry and social anthropology. In the latter context, patchwork especially is used to designate research approaches that 'jump' either between different research areas and sites while investigating global phenomena or with respect to changed research conditions under the impression of the

15 The 'activating' welfare state model, as established in Germany through the introduction of the 'Hartz4' laws in the early 2000s, is oriented towards the improvement of the single citizen; as Kessl argues, the activating welfare state model is very much related to neoliberal thinking (Kessl 2019: 118). Furthermore, Social Work in Germany can be considered to play an essential part in the implementation of the state's hope 'to activate' all members of society, and especially those that seem not 'active' enough (cf. Kessl 2019).

corona pandemic and/or care obligations. These are not necessarily compatible with the current trend of publication cycles becoming faster and faster (cf. Günel et al. 2020). Therefore, some authors call for a new form of ethnographic research – especially questioning the ideal of the classic one year of fieldwork or village study in the Malinowskian tradition – under the buzzword “patchwork ethnography” (ibid).

While these works provide fascinating insights, in the context of my research, patchwork became significant as a methodological point of reference in a broader sense, due to the many limitations and restrictions heaped upon both research and private life in the wake of the corona pandemic. Additionally, though, the boundaries between these two spheres became increasingly blurred, as in the course of my fieldwork patchwork also became a creative occupation and offered the possibility to engage with others on a personal level in times of spatial distance and social distancing. Therefore, my ‘private’ interest in patchwork and sewing as creative techniques led to a new focus of attention: while engaging with patchwork and quilting as subjects of anthropological inquiry and through conversations with my interlocutors, I became more attentive to any kind of handicraft activities in the field and its embedding into gendered, class-related and racialised dimensions of the everyday life of researchers and research partners. These included not only the differing and unjustly distributed opportunities to learn and pursue such a ‘hobby’, but also the (questionable) significance of voluntarily producing material aid objects in the wake of disaster.

Furthermore, it made me aware of the recurring connection between manual care work and notions of what can be considered an appropriate occupation for women – as a general category but (with slightly different implications) especially for women addressed as refugees against the background of the activating German welfare state. Finally, this new research lens made obvious not only our differences and commonalities as researchers and research partners, e.g., our shared difficulties in dealing with new living circumstances in the pandemic, but also our (different but related) struggles of living as women in a society profoundly shaped by patriarchy and racialisation. Once more, this might be taken as a reminder that researchers and research partners are not trapped in differ-

ent times and spaces but should rather be considered as “actors within the same political and societal surrounding, although they occupy different positions” (Sökefeld 2002: 89, my translation).

Against this backdrop, it has to be acknowledged that I was not the only researcher (or ethnographic patchworker) in this context, as my interlocutors were also highly involved in the joint discussions on first ideas and interpretation, as well as constructive critiques regarding the whole research process. Therefore, I owe them a lot of gratitude for teaching me how to “weave” (cf. Ingold 2000) together the various patches that come together to make up this research.

Finally, patchwork and ethnography might be regarded as two related trades, as they both rely not only on a rigorous methodological procedure, but also on the element of surprise. Furthermore, as ethnographers *and* quilters, we often need to make various attempts to rearrange ‘the material’ until we can recognise any overarching themes and patterns that might help us make sense of all those bits and pieces we have collected over time. However, we have to acknowledge that any resulting insights are never just on our own merit but depend on the patterns designed by those that came before us and the knowledge of those that kindly take the time to talk and explain to us how it all fits together.

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The Travelling Carpet

Mobility, Trade and Identity along the Karakoram Highway

Alessandro Rippa

I had already been in Pakistan for about two weeks, which I had spent mostly between Islamabad and Rawalpindi. I had already interviewed several Uyghur migrants and two former engineers, who had worked on the construction of the Karakoram Highway (KKH) in the 1970s and met some traders I knew from previous fieldwork in China's Xinjiang Uyghur Autonomous Region. On that day, a Monday in February 2013, I visited the Pakistan-China Institute, a think-tank aiming at promoting Sino-Pakistani ties and 'people to people' exchanges. There, I spoke with one of the assistant editors of *Youlin Magazine*, a monthly publication co-sponsored by the International Culture Exchange Association of Xinjiang. In the afternoon, I spent a few hours at Quaid-i-Azam University, where a colleague from the Department of Anthropology helped me locate a few Master's theses I was interested in. One caught my attention: it focused on carpets and carpet-making, featuring fascinating images of how designs were rendered on paper and written into what the author called 'carpet language' or 'design script' (see Figure 3). I was not sure whether this would be useful for my doctoral research – which centred on trade and migration along the Karakoram Highway between China and Pakistan – but I decided to make copies of it just in case. After a *chai* with a couple of instructors from the Department of Anthropology, and quite happy with a productive day of fieldwork, I headed back into town to have dinner with Martin Sökefeld.

I had never met Martin before, but we had been in touch throughout the previous year via email. I had contacted him regarding some of his work in Gilgit-Baltistan and asked if he knew any members of the Uyghur community in Gilgit. Martin put me in touch with colleagues at Quaid-i-Azam University as well as a few other scholars I would meet during my time in Pakistan. In previous months, I had also applied for a short-term fellowship to spend some months in Munich, with the intention of writing a chapter of my thesis under his supervision. Only two weeks before our first meeting in Islamabad, the application turned out to be successful and I was thus keen to thank Martin in person for his help. He had supported me without really knowing me – a PhD student from the University of Aberdeen.

Martin suggested that we meet at Kabul Restaurant, in Jinnah Super. “I am already longing for Kabuli kebab,” he wrote in an email the previous January, as we planned our meet-up in Islamabad. I was rather happy with his choice, partly for my own appreciation of Afghan food, but also for the central location of the restaurant and its proximity to some of my favourite carpet shops. The food, to be sure, did not disappoint, and over dinner we spent a couple of hours talking about the Karakoram Highway, the state of construction work around the Attabad Lake and trade with China. Given my background and experience in Xinjiang, I knew a lot about the Chinese side of the border, but much of what Martin told me about Gilgiti traders was new. I left the restaurant convinced that I would have a productive time in Munich the following summer, and with a very positive impression of Martin.

Following dinner that day, I did not go straight to my guesthouse. Instead, I decided to stop by ‘Saarouq Carpets’, a small shop tucked away on the second floor of a rather shabby building in one of the least busy areas of Jinnah Super. During my time in Islamabad, I had already been to Saarouq Carpets twice: I liked what they offered and enjoyed talking with the owner, Arslan, even more. Originally from Afghanistan, Arslan had inherited the shop and a distinct passion for carpets from his father. Over the years, they had gathered an impressive collection, with some unique Turkmen pieces. My own appreciation of carpets, I had told Arslan the first time we met, grew out of the time I spent in Kashgar, in

Xinjiang, over previous years of travel and research. There, not only did I learn about local designs, but I also had a chance to see some beautiful Afghan, Kazakh and even Iranian carpets. Arslan, it turned out, was quite interested in learning more about Xinjiang carpets. He had seen a few ‘pomegranate’ pieces, he told me, but he would have liked to see more. I promised that I would come back with my laptop so that I could show him some pictures I took in Kashgar. That evening, I did just that – and we spent a couple of hours browsing through carpet pictures and talking about what makes a good example. I could never grow tired of those conversations, and, apparently, neither could he.

Over the following weeks, I returned to Arslan’s shop several times and purchased a few carpets for friends in Europe who had asked me to keep an eye out for good specimens. With this goal in mind, I visited many carpet shops, including Arslan’s, and took pictures of some of the best on display. I then shared those pictures and made inquiries about specific pieces, at times negotiating prices and placing orders. For me, this was not about making a profit – I never got anything out of those exchanges, and I bought fewer than ten carpets in total. Nevertheless, I not only enjoyed spending time in these shops, but this activity also allowed me to gain precious insights into the nature of commercial exchanges in both Xinjiang and Pakistan. More than that, though, I learned how carpets represented a unique commodity exchanged along the Karakoram Highway and how the history of their designs could shed additional light on the nature of transnational mobilities in the area. In other words, from being a small side-interest, a personal passion, carpets played a serendipitous role (Hazan and Hertzog 2011; Rivoal and Salazar 2013) in how I thought about the Karakoram Highway and the nature of cross-border exchanges between China and Pakistan.

In this chapter, I build on my experience with and among carpet dealers in Islamabad and Kashgar to recount the little-known story of Xinjiang carpets from Khotan and their unique designs, as these have travelled between China, northern Pakistan and Afghanistan since the opening of the Karakoram Highway in the 1980s. Through this story, I show how Khotanese carpets have become both a symbol of local identity and the transnational mobility of goods and capital, and how the Karako-

ram Highway features prominently in these narratives. I conclude by discussing how this example can contribute to ongoing ethnographic studies of roads and by commenting on the value of multi-sided ethnography. In doing so, and in line with the purpose of this volume, I show how a side-interest, a distraction, a personal passion, can become a surprising opportunity for seeing connections and dynamics that would otherwise remain out of sight.

I begin, however, with a brief history of the road that made these exchanges possible: the Karakoram Highway, or KKH.

The Karakoram Highway

The People's Republic of China and the Islamic Republic of Pakistan share just over 500km of (contested) border in the high Karakoram mountains. The boundary itself was agreed upon by the two countries in 1963 and has never been recognised by the Indian government, thus making it a very sensitive area of both connections and disjunctions (Lamb 1973, Fravel 2008). Following the 1963 agreement, China and Pakistan developed one of the world's closest bilateral relations. Leaders of both countries, in fact, rarely miss an opportunity to remark that their relationship is 'higher than the Himalayas and deeper than the Indian Ocean' and 'sweeter than honey'. Throughout the decades, this pompous vocabulary has taken some concrete form in various coordinated projects, with an explicit effort to improve the security and economic ties between the two countries (Small 2015). Surely the most outstanding result of this relationship is represented by the Karakoram Highway, which connects Kashgar to Islamabad (Figure 1). In addition to its obvious strategic value, the road, open to civilian traffic since 1982, was built to foster trade and enhance both countries' control over the border regions (Kreutzmann 1991; Stellrecht 1997; Haider 2005). For Pakistan the road provided improved access to the disputed territory of Kashmir and has become the main factor in the economic and political integration of Gilgit-Baltistan with the rest of the country (Sökefeld 2005).

Figure 1: The Karakoram Highway



Map designed by the author

Within this context, the KKH remains today the region's most important infrastructure, and while it would be wrong to argue that Gilgit-Baltistan was isolated before the construction of the KKH (Sökefeld 2014; Stellrecht 1998: 80–1), it nevertheless remains evident that the road has radically transformed the region (Karrar 2020). As Sherbaz Ali Bercha, librarian at the Gilgit Municipal Library and historian of the region, told me:

Before the KKH was opened, people here in Gilgit-Baltistan were self-sufficient. The KKH connected us to Pakistan, which is a good and important thing, but there are also negative aspects. For instance, our dialect has been hurt. And, most importantly, now we are completely dependent on the road. If the road is blocked for some sectarian violence, or for some natural disaster, we suffer too much (Interview, June 2013).

The issue of Gilgit-Baltistan's dependency on the KKH is a topic of frequent discussion in the region, particularly when the road is obstructed for natural or political reasons and shops throughout the region run out of certain items. In fact, over 30 years after its official opening, the Karakoram Highway remains today a precarious infrastructure. The highway's accessibility is constantly threatened not only by natural factors such as landslides and precipitations, but also by its use during protests and demonstrations of various kinds, or the closure of some of its sections due to sectarian violence and other security reasons. Nonetheless, from its very conception, it seems to express powerful promises: connectivity, development, mobility and business opportunities. Although, here as in other contexts, roads often fail to fulfil most of their promises, the powerful sense of mobility that they embody remains generally untouched (Campbell 2012; Khan 2006; Jamali 2013). In the case of Pakistan, Haines has persuasively shown how the Karakoram Highway was part of a national integration, modernisation and development project. Through the trope of the 'Silk Road', moreover, the highway became a major connector between an imagined past and a likewise imagined future (Haines 2012). Promises of 'progress and prosperity' have more recently taken the shape of the China-Pakistan Economic Corridor (CPEC) and its integration as one key vector of China's ambitious Belt and Road Initiative (Oliveira et al. 2020; Rippa 2020a, 2020b).

To understand how infrastructural promises are negotiated on the local level, Penny Harvey and Hannah Knox (2012) apply the notion of 'enchantment'. They suggest that the experience of roads' dereliction and abandonment, for instance, "strengthens the desire for them and con-

stantly renews the sense that sometime soon they will appear and life will change for the better” (534). Rather than diminishing people’s faith in the positive impact of road development, or in the government’s ability to deliver it, moments of failure can thus strengthen the promises that infrastructure holds. This dynamic is confirmed by my observations along the Karakoram Highway, where regular encounters with the material disintegration of the road, or its closure for political reasons, seem to reinforce people’s expectations and belief in the ability of the road to improve their futures. In the case of the highway, the framework is even more complex, given the transnationality of the road, a condition which – particularly in Gilgit-Baltistan – seems to intensify those expectations. In the region, there is a widespread belief in both China’s capacity and commitment and in the proclaimed strategic relevance of the CPEC, which has often led locals to conclude that, this time, “Pakistan cannot forget us.”¹

The Karakoram Highway, in this sense, possesses a subversive power, too. Being the product of a joint effort by the governments of China and Pakistan, it has become the locus of thoughtful discussions and painful comparisons. In the Pakistani context, as mentioned, the road has been a major means for channelling state power and forging the nation-state (Haines 2012). This narrative, produced and sponsored by the Pakistani state, currently faces a major contradiction posed by the conditions of the road itself. At the time of my fieldwork, in 2012 and 2013, for instance, the highway was blocked due to a major landslide in Attabad, and a Chinese company – the China Road and Bridge Corporation (CRBC) – was constructing the necessary tunnels around the lake as well as undertaking important upgrading work on the Khunjerab-Raikot section of the road. In the eyes of most inhabitants of Gilgit-Baltistan, this represented a sign of Pakistan’s incapacity – and/or unwillingness – to invest seriously in the region (Sökefeld 2012). This perception, certainly connected with the status of Gilgit-Baltistan within Pakistan, seems, then, to be

1 On the feeling of abandonment, and even betrayal, felt by many in Gilgit-Baltistan, due to the region’s lack of political representation and right, see Ali (2019).

constantly reinforced by encounters with Chinese road workers, or by the experience of the road itself.

Cross-border traders have an even more defined idea of China's role in building and maintaining the Karakoram Highway. Throughout my fieldwork, I had countless discussions about the different conditions of the road on the two sides of the Khunjerab Pass – smooth on the Chinese side, bumpy and often interrupted on the Pakistani side. A direct experience of the highway itself thus produces narratives that often contradict the official version sponsored by the central states, and in this way it becomes a mirror for what are considered the lies, weaknesses and empty promises of the state. On the other hand, the Karakoram Highway also provides an emblematic example of Chinese state power, another topic of numerous discussions, particularly in Gilgit-Baltistan. In some of those discussions, the road becomes a means through which the Chinese state is attempting to infiltrate the region: it is not only an infrastructure for economic development, but also a tool for social, cultural and even political change. Many people in Gilgit-Baltistan shared with me their fear of losing their own culture and traditions, or even their limited political and economic independence. To borrow an expression from Harvey and Knox, this seems to be yet another case of *disjunctive doubling*, i.e., “where roads are dreamed of as solutions to a pervasive sense of abandonment and underdevelopment, and yet dreaded for their proven capacity to destroy fragile natural and social environments” (2015: 135).

Over the course of my fieldwork, and mainly driven by my personal interest in carpets, I also came to appreciate some of the more subtle ways in which the Karakoram Highway affects and engenders social lives in China and Pakistan. Below, I discuss the example of Khotani carpets and their circulation across the Karakoram. As several recent studies point out, objects are both bearers and makers of social change – and carpets represent a case in point (Dzüvichü and Baruah 2019; Tagliacozzo and Chang 2011). More generally, as I discuss towards the end, such an object-oriented approach can help us appreciate some of the least apparent consequences of road construction and can thus represent a welcome contribution to the growing field of infrastructure studies in anthropology.

Transnational carpets

Different kinds of carpets move along the Karakoram Highway in opposite directions. Shortly after the KKH was opened for civilian use in 1982, a few Pakistani dealers began to transport handmade carpets to Kashgar. Most were either made in Peshawar or Afghanistan; occasionally, however, Iranian, Turkmen and even Uzbek pieces were sent to Xinjiang as well. Most of these carpets were quite expensive and not meant for the local Uyghur market; rather, they were intended for Western – and increasingly Han Chinese – tourists visiting Kashgar. Indeed, a few carpet shops opened around the main touristic spots, namely the Chini Bagh Hotel, the main bazaar and the Idgah Mosque area. During my fieldwork, I repeatedly visited these places and became particularly close to one local carpet dealer: I shall call him ‘Polat’.

Polat was born in a small village on the outskirts of Kashgar, and as a young man he used to visit the city weekly to sell leather and vegetables at the local bazaars. One day, he told me, he visited a small carpet market in Kashgar and thought about his home village, where a few families were still weaving these items in the traditional way. The next time he travelled to Kashgar, he brought a few of these carpets with him and immediately sold them, making a good profit. “This is how I became a carpet dealer”, he told me. “It was 1987, and I didn’t know much about carpets back then” (interview, May 2013). Shortly afterwards, he met some Pakistani dealers and learned that they were not only interested in selling Afghani carpets, but also in buying old local pieces. He thus travelled extensively around Xinjiang and collected an impressive number of old rugs, particularly ‘Khotani’, some of which are extremely valuable. ‘Khotani’ carpets were made around Khotan, in the south of Xinjiang, once famous for producing these items with very peculiar designs, including complex compositions of pomegranates and large medallions. Polat bartered many of those carpets with Pakistani dealers in exchange for new Afghani pieces. In 2001, he opened his own shop.

Polat was not the only Uyghur trader involved in this kind of business, and hundreds of old Khotani pieces in those years were moved across the Karakoram into Pakistan, from where they would be sold to

international dealers across the world (Mascelloni 2015). Today, Polat told me, it is difficult to find old Khotani pieces in good condition, and as a result he has stopped searching for them around Xinjiang. Although he still owns a few of them, his main business now lies in selling new Afghani pieces to Western and Han Chinese tourists. Among those pieces, however, some are weaved in a ‘Khotani’ design similar to the old ones he once sold to the Pakistani dealers. He explained this to me in the following way:

Nowadays, people in Khotan don’t make good-quality carpets anymore. But the design is very beautiful, so now they make them in Afghanistan. They copied the design from the old carpets I sold to the Pakistani, and now they bring me new ones. This [pointing at one of those carpets] is made in Afghanistan, but the design is from Khotan. It’s a local design (interview, May 2013).

Figure 2: Old (above) and new (below) Khotanese carpets.



Photos by the author

Although a close examination of old and new Khotanese pieces would reveal significant differences in the complexity of their design, and the quality of the wool and that of the dyes, the resemblance is still significant (see Figure 2). More interestingly, for the argument of this chapter, this 're-localisation' of the production of Khotanese carpets happened entirely because of the Karakoram Highway. Not only are carpets woven today in Afghanistan imported into China via the highway, but many of the designs themselves have travelled in the opposite direction on the same road. Even more significantly, in the process of selling these carpets to tourists, this whole story was continuously re-enacted by Polat as if it were adding value to the carpet. As I observed – and helped – him on several occasions he was trying to sell a carpet, I noticed that he particularly insisted on the trans-local character of the piece, pointing out that although the design was from Xinjiang, it was made in Afghanistan and had been brought to Kashgar by Pakistani traders through the Karakoram Highway. The narrative concerning the carpet, its story, eventually seemed to become as relevant as the object itself. The carpet was valuable not only because of the originality of its design, or the quality of the knot work, but also because of its adventurous, transnational story. It was treated by Polat as both a local and a transregional product, a symbol of Uyghurs' long and rich cultural history and an indicator of Kashgar's historical role as a key trading hub and of the international flows of contemporary trade.

Within this story, the Karakoram Highway played a double role. It served as a prime artery for the exchange of goods (carpets) and knowledge (carpet design), but it also figured in the stories related to these carpets as a dangerous, exotic and inaccessible mountain road. These carpets, in my discussions with Polat, seemed to embody a local, Khotanese tradition as well as a trans-local element well-expressed by the Karakoram Highway, which functioned both as a form of narrative and fascination. The highway had thus become part of the carpet, defining its trajectory and contributing to its story.

Roads and carpets

After flipping through some of my carpet pictures from Kashgar, Arslan and I continued looking at some of the items he had in his Islamabad shop. At one point, he unrolled a carpet I had not seen before, in a particular design I did not recognise. I asked him if he knew what that design was called. His answer was rather straightforward:

Look, I don't really know the name of this [carpet's] design. My father used to tell me that when he was young, they knew three or four designs. Now there are hundreds, so dealers who don't know them will make up some name just to sell more. I won't lie to you. And it's not important. Look at it [he points at the carpet]: what really matters is not the name of the design but the quality of the material. Look at the knots [he turns the carpet over and scratches a row of knots with his finger nail], look at the pile [he brushes the pile with his palm], this is a good carpet. This is a very good quality carpet; you can wash it a hundred times and it won't wear out. This is what's really important (interview, February 2013).

This conversation echoes what many carpet dealers in China and Pakistan regularly hinted at throughout my fieldwork, namely the important difference between the design of a carpet and its material characteristics, such as the quality of the wool and of the knot work. But how do the design and materiality of a carpet inform one another? What are the circumstances through which a piece comes to be made – and how? These are questions I kept asking myself during my fieldwork in China and Pakistan, prompted by encounters and conversations such as the one I just described. Over time, I recognised, I began thinking not only about carpets, but also about roads in similar terms. In the case of the KKH, this turned out to be an useful exercise in my attempt to reconcile the promised function of the road, with its (dire) material conditions. Once again, from being a small side interest, the stories of carpets and their design serendipitously informed my thinking about infrastructure, mo-

bility and identity along the Karakoram Highway. For this reason, I now turn to weaving, before heading back to roads and trade.

In anthropology, among the authors who have engaged with the process of weaving, Tim Ingold's discussion of 'making' is particularly interesting (2010). Ingold argues against what he calls the 'hylomorphic' model of creation, based on the Aristotelian principle that creation is a coming together of form (*morphe*) and matter (*hyle*). The hylomorphic model, accordingly, envisages that a pre-existing design is imposed by the creator upon passive matter. Against this view, he contends that making is rather a practice of weaving: "not the imposition of form on pliant substance but the slicing and binding of fibrous material" (2010: 92). The hylomorphic model is thus turned upside down, and primacy is given to "the processes of formation as against their final products, and to the flows and transformations of materials as against states of matter" (2010: 92). Furthermore, materials are constantly in a state of variation, and to separate the design from the making, or to disengage it from the material, means avoiding such consideration, thus treating the material for what it cannot be: a passive recipient of human agency.

Returning to the carpet trade along the Karakoram Highway, the examples of old Khotani pieces brought to Afghanistan and 'copied' seem to fit quite well within the 'hylomorphic' paradigm. Apparently, a specific design is taken and copied, probably sketched on a piece of paper and then hung beside the loom so that the weaver does not lose track of it (Figure 3). And yet, the carpets I used to see in Polat's shop reveal another story, particularly when compared with the old pieces he possessed. The reproduction of old designs holds a creative element. The general idea of the design did remain similar, and certain motifs – such as the typical pomegranate – look almost the same, but then each and every carpet has some interesting singularities. First, the dyes and the quality of the wool are radically different, which brings about different changes in the design itself, as different kinds of wool imply different degrees of 'precision' in the weaving process, while 'new' colours allow for different combinations. The new Khotani carpets, moreover, included in their designs a few geometric details and motifs that did not appear in the old ones. In some cases, a distinct Caucasian or Iranian influence was clearly dis-

cernible. Each carpet, thus, not only was inspired by a specific design, but also expressed the personality and the taste of the weaver – or, more likely, echoed some changes in the demand of the market.

Figure 3: Carpet language / “design script”



Photo from Mushtaq 2009

In fact, even when hung on the loom, a ‘design script’ always leaves room for some sort of improvisation and inventiveness. Mistakes are quite common, rather unsurprisingly, given the conditions where the weaving process takes place, as the work of small-scale weavers can be constantly interrupted by other activities that need to be carried out. Even professional weavers – as anybody who has ever visited a carpet factory must have realised – work in very confusing environments where people chat, eat and pray around the looms. The design of a carpet, therefore, rather than being simply the result of the imposition of form upon matter, not only exists exclusively through this ‘matter’, but also owes its characteristics to a complex entanglement of various elements: the ideal design, the quality of the wool, the taste of the weaver, work conditions and so on. In other words, design and materials do not

exist as a separate dichotomy but instead represent a coming together of ideal motifs and mundane instances, symbolic forms and practicalities. In this sense, carpets function as a particularly apt metaphor for roads: not in that they connect, but precisely because the connections they engender inevitably contribute to re-making social, cultural and political worlds. Furthermore, as the interruption (in trade and mobility) caused by the Attabad Lake shows, such connections are never given once and for all: they are instead continuously challenged, shaped and informed by the lives of other forms and materials.

This understanding of roads is not new in the growing anthropological literature on the subject, and so they been shown to allow the ethnographer to work through and across different categories and disciplinary boundaries and represent an ideal point of entry for anthropological analysis (Dalakoglou and Harvey 2012: 2; Dalakoglou 2010, 2012; Mostowlansky 2011; Argounova Low 2012; Rest and Rippa 2019). Echoing this literature, and moving from my experiences across many carpet shops, I address the Karakoram Highway not only as an object of analysis per se, but also as an inclusive space created by economic, social, cultural and material interactions. The road is thus understood as both a cross-contextual category of analysis and as the privileged locus of ethnographically bounded, localised dynamics.

The example of handmade carpets further highlights the connections between roads and the commodities that travel along them, hence showing the complexity of material encounters along the Karakoram Highway and the different directions in which the road seems to run. Polat's story shows the entanglement of local and transnational, personal and material, which characterises encounters along the road. Carpets are made in Afghanistan, based on a Khotanese design, and are sold in Kashgar as exotic products that have travelled through the high passes of South and Central Asia. In Kashgar's carpet shops, moreover, this transnational character is combined with the origin of the design, renewing a sense of belonging and proximity to the Khotanese tradition. When dealing with Western and Han Chinese tourists, as shown above, these two elements help (without apparent contradiction) add value to the carpets, as visitors often seem stunned by the mixture of tradition and trans-locality

they seem to reflect. To put it differently, as a road like the KKH cannot be studied without considering the expectations, promises and fears it continuously evokes, in Polat's shop, a carpet would lose meaning and value if detached from its story.

Conclusion

A road project might be completed, but as shown by the continuous making and unmaking of the KKH since its opening in the early 1980s, it is never quite finished. Carpets, too, might be woven; yet, throughout their lifetimes they might be repaired, their colours will fade and some of their corners will be eaten by bugs. Even more so, their stories will take thousands of different shapes and forms. Indeed, as Ingold puts it, nothing is ever finished, i.e., "everything may be something, but being something is always on the way to becoming something else" (2011: 3). Ingold makes a similar point while discussing what he sees as a 'radical distinction' between objects and things. Things, as opposed to the completeness and finitude of objects, are "a gathering of materials in movement" (2012: 436). Moreover, a thing does not stand before us "as a *fait accompli*, complete in itself" (2010: 96), but rather as potential "for further making, growth, and transformation" (2012: 435). The world of things, in other words, is a world of becoming, of transformation, namely a lifeworld. Things, as I have shown, particularly concerning carpets and roads, are necessarily intermingled with the meanings and narratives they continuously evoke. Meanings, however, are not simply 'attached' to objects by their human users or creators; rather, they emerge in contextualised encounters between things and actors, places and stories. The advantage of ethnography is precisely its ability to address these encounters in the details where lifeworlds are continuously generated and through often unexpected opportunities that emerge through fieldwork.

Therein lies a crucial element of much ethnographic research that needs to be explicitly addressed: the passion and care that drives it and the often seemingly futile interests of the ethnographer that shape it. The example of my work with carpets outlined in this chapter is an apt exam-

ple in this regard. Carpets ended up playing a significant role throughout the course of my fieldwork. At first, however, my interest was due simply to the fact that I found handmade carpets, rugs and kilims utterly beautiful and fascinating. Carpet shops were places where I enjoyed spending my time, and conversations with carpet dealers occupied a significant part of my days. I also had the opportunity to see weavers at work, an experience which produced important insights for my own research. Carpets thus became an object of my investigations, and as this chapter reflects, an important tool for thinking about roads and transnationalism in the context of the KKH.

To take a cue from the title of this volume, one could say that ethnographic research is always – and inevitably – multi-sided. Our passions, private lives and personal trajectories often drive not only *where* we work but how we conduct our research, who we spend time with and the issues we attempt to unravel. Were it not for my appreciation of carpets, for instance, I would probably never have met Polat and Arslan, nor would I have ever learned about how carpet design travelled between China, Pakistan and Afghanistan, affecting an entire segment of the market. Possibly, I would have ended up with a rather different understanding of what a road like the KKH is and what it means for those who travel and trade along it.

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Section Two: More-than Kinship

Ethnography with a Faith Community

Oscillating between In- and Outsider

Deniz Coşan Eke

Introduction

First let me introduce myself, I am a Turkish Alevi woman who immigrated to Germany and then, throughout her academic journey, discovered her deeply veiled identity and belief. That said, although I grew up in an Alevi family and an environment in which many Alevi live, I only started to identify as an Alevi after I started my PhD research. The main reason for this was my personal security, because in my childhood, Alevi's religious activities were mostly carried out in secret, as Alevism was not considered a legitimate (in either legal or religious terms) faith group in Turkey. Another reason was fear of discrimination, which is a common feeling amongst Alevi, not only personally, but also as a community of faith. My family warned me not to say explicitly that I was an Alevi, in order to protect myself from discrimination and socio-cultural pressures (Erdemir et al 2010). Consequently, I did not carry out any research on the Alevi faith until my doctoral research, and I did not participate in any religious rituals or practices during my time in Turkey due to the chilling effect of religious pressure. Therefore, my doctoral studies were a unique experience, in that I faced my own fears and in turn learned about the intricacies of my own identity. During my research, I was in a unique 'insider' position, as I was not only a curious PhD researcher, but also had the same cultural and religious background as the partici-

pants. Being Alevi evolved into a relational interface that has helped me to position myself within the existing networks of Alevi communities.

In my PhD research, I conducted ethnographic research on Alevi communities in Turkey and Germany in order to set the theological tradition of a faith sanctified in texts in relation to being embodied in rituals and practice (Wigg-Stevenson 2018). Hence, I analysed Alevi practices to explore how spirituality contributes to collective memory and identity. In brief, I examined the Alevi faith not only from its own internal perspective, but also to develop an external perspective by exploring narratives about believers, especially religious leaders.

Boundaries between the insider and outsider

During four long years of doctoral studies in Social Anthropology at LMU in Munich, I had the chance to do research both at home, in Turkey, and away from home, in Germany, in a country foreign to me, whilst also working with an inside belief group as a member of that group. This allowed me to “immerse in the social context under research” (Sökefeld/Dürr 2018:230). When I started my research, I defined Turkey as my home country, as I knew its language, culture, politics and social structure better than Germany. However, after many years of living in Germany, and with my time in Turkey dwindling, it was difficult for me to describe the latter as fieldwork at home. Of course, this does not mean that I would describe Germany as fieldwork at home, either. Rather, my situation and experience of feeling at home was time-dependent, as the Italian sociologists Paolo Boccagni and Carlos Vargas-Silvas also point out:

As immigrants' life course proceeds, feeling at home has to do less with continuity with the past than with the possibility to live in a place perceived as safe, familiar and orderly, possibly with one's own family members. As time in immigration goes by, home ends up being a matter more of perceived normality than of ethnicity (2021: 12).

The changing meaning behind the concept of 'home' – for me as an immigrant – shaped my research and constantly led me to reconsider both my ideas and my feelings of belonging. During ethnographic work, researchers often discuss how they can reveal their identity, which in turn affects their research (see Smith 2016; Atay 2017).

After a while, my investigation also became an auto-ethnography in which I looked at myself as both an insider and an outsider to the group I was studying. This personal process allowed me to focus on many new topics, such as belonging and related experiences of fear and trust that were challenging for me.

Drawing on these personal experiences and feelings in my PhD research inspired me to write this article about my fieldwork, which in turn may provide insights for other researchers in this domain. Therefore, the main research question of this chapter examines some of the difficulties I encountered as an insider researcher doing ethnographic research within a faith community. I argue that research depends on whether the person conducting the research defines him or herself as a member of that belief or not. In other words, the vulnerabilities and privileges of researchers who are 'insiders' of a faith differ from those who are 'outsiders'. At the same time, my own example shows that the boundaries between insider and outsider are not that clearcut but are fluid and shaped by complex social, political and historical processes.

Religious ethnography

For an insider researcher, although research may seem to have many advantages in establishing close and trusting relationships with research participants, this position may represent the risk of losing critical distance and objectivity (Denscombe 2010). For example, the supernatural *Keramet* (miracle) stories that are part of Alevi mysticism are used by *dedes* to gain authority and legitimacy in front of their followers and against non-Alevi communities in Islam (Tee 2010). This authority may influence the researcher and make him or her want to believe in miracles.

However, there are two important points in this regard. First, when the researcher carries out fieldwork and runs with the narratives of these miracles, they may have more chance of understanding the practices and rituals of belief more deeply than an outsider researcher who does not believe or is not a member of the group. Second, if the researcher is a member of this group, questions about whether they believe in these miracles can be used to test how much they relates to the faith group. In either case, the researcher can use their insider theological knowledge, i.e., knowledge of the divine of religious belief, as an additional tool in their ethnography, in order to gain insights into different places, actions, ideas and groups.

Ethnography as “dense description” (Geertz 1973) can help in discovering believers’ theological perspectives about a faith by using qualitative methods such as participant observation, field notes, interviews, diaries and document analysis (Whitehead 2005).

According to Julian Murchison and Curtis Coats, “ethnographic methods will also allow us to remain attendant to the social dimensions of complex flows and networks and avoid the tendency to perceive these religious practices as entirely individualized” (2015: 991). Along these lines, theological knowledge, discovered through ethnographic methods, can ensure that research on religion is an in-depth and comprehensive analysis. In this way, as James Bielo points out:

Religious ethnography pushes the limits of the anthropological imperative of total fieldwork immersion. Ultimately, religious ethnography provides a sharp reminder that fieldwork is not simply a research task and a social endeavor, it is a moral, existential, and ontological project (2015: 33).

A researcher studying a faith community must consider that everyone therein has an idea, a feeling or an understanding of that faith. Thus, when one takes on the role of a researcher of a faith, even if they are a member of that faith, their expertise in all aspects of that belief will be tested by its followers. This testing situation can sometimes involve questions, and sometimes it can be in the form of observing the re-

researcher's participation in belief rituals. While the researcher observes a community, they are also observed by that community. In ethnographic fieldwork, surveillance or being observed by research participants is experienced for different reasons and to varying degrees (see Sökefeld and Strasser 2016). Most of the Alevi involved in my research, who had taken an active role in or supported leftist ideological movements, had had experience with state surveillance and were therefore sensitive to being observed, and thus they observed me in order to protect themselves from oppression and discrimination.

However, this observation – and this is what I wish to underline in this article – is not only a group's desire to ensure its own safety or to control the researcher, but it also an opportunity to ensure how sincere the researcher is in their expression and presentation of religious belief. Therefore, the researcher may perceive even more pressure than the believers of that faith to know in detail the sacred, theological and historical details. Despite this burden, the other important point is that ethnographic research involves serendipity and is based on social relationships and intensive interactions with research participants; hence, it often produces non-expected results. Therefore, the questions asked at the beginning of the fieldwork may change during the research and/or be enriched by complementary fieldwork as more and different questions arise. The methodological practice of anthropology, which characterises its difference from other disciplines, is aptly summarised by Martin Sökefeld and Miriam Ince (2016):

For anthropology it is not a methodological problem to change the research question or the original research hypotheses in the course of research. On the contrary, it is almost a methodological premise of anthropology that the research perspective changes, because this is evidence that we have learned something new 'in the field' and therefore have to adapt our original research question to the new findings. Field research is thus based on the principle of trial and error and the constant feedback of (preliminary) results on the research question and methodology (2016:6).

I had a similar methodological problem when I started my PhD fieldwork and was flying blind until I found suitable questions for my research. I felt really stressed when I decided to change many of the questions I wrote down at the very outset. Throughout my earlier academic career in sociology, I had researched mainly by trying to answer the following questions: “What happened? Has it happened everywhere? Has it been repeated over time? What are the underlying causes of this phenomenon?” Consequently, I did not need to change the research questions, as I was collecting data around more generalised questions. In contrast to sociological research, as stated by Biolo, “ethnographic success and failure hangs in the balance of how well relationships with fellow humans are established and nurtured” (2015: 31). As a matter of fact, ethnographic research as the primary methodological approach used to analyse faith groups makes it possible to communicate effectively and build mutual trust with them.

By participating in the research group and their religious and cultural activities, my PhD thesis advisor, Martin Sökefeld, stated that establishing a bond of trust, especially with the members of a belief community, is the most fundamental condition of cooperation with any research participants, in order to engage in more face-to-face interactions, reflect the characteristics of the group and ensure the reliability, validity and accountability of research findings. Therefore, ethnographic research on a belief group, exploring religious, practical and socio-cultural dimensions and spending a certain amount of time with them in a particular place, helps formulate research questions to reflect issues within the group. Thanks to his guiding suggestion at the beginning of the research, both my relationship with the research group and my views on the research questions and objectives changed as time spent in fieldwork increased.

Another important contribution to my thesis was Sökefeld’s book on the Alevi community, namely “The Struggle for Recognition: The Alevi Movement and the Transnational Area in Germany” (2008), which examined changes to and the transformations of Alevis through ethnographic methods in the context of Alevi organizations. For this purpose, he carried out field studies, focusing on the German city of Hamburg,

with Alevi cultural centres between 2000 and 2004. This book, which gave me an idea for my doctoral research, is used as one of the main reference sources in academic studies on Alevis in the diaspora. What makes it a standard reference is that it is an important academic study on a religious group with transnational levels on the politics of identity within the debate on recognition, as well as the contribution of its theoretical framework in the fields of religion, diaspora, identity and social movements. Also, his research raises awareness that general models of immigrant and religious groups that are essentialised into homogeneous and monolithic categories should be avoided to ensure broader and more careful considerations of different minority communities within Germany. Before sharing my experiences and research based on the Alevi community, it is essential to provide a brief overview of my research group, i.e., the Alevi faith group.

The Alevi community

In addition to the historical origins of Alevism as a faith, which is closely related to Sufism, Alevi society has actually been influenced more by the unique characteristics of Turkey's historical, political and socio-cultural infrastructure. Alevi values and rituals are mostly studied as part of Turkey's local and national religious contexts. As a religious and cultural community, Alevis define their faith as a spiritual path to awakening the sacred, divine power within itself. According to Alevi mysticism, it is the unity of the creator and the created, and the harmonious unity of creation is the cause of creation and the basis of its continuity. In Alevism, there is a belief that each part of the universe in some way reflects the whole or God. Alevis also believe that humans, as God's supreme creation, have divine power (sacred power) within themselves. The God of Alevis is the divine spirit that is in everything and everyone. This divine spirit is hidden in the human heart. The phrase "Do not

seek God outwardly! If your heart is pure, God is within you” has been accepted as one of the guiding principles by Haji Bektasch-i Veli¹.

The main religious ritual of the Alevi is a *cem*, and Alevi gather for their religious rituals in *cemevis*. Alevi clergies are referred to as *dedes* (the male form) and *anas* (the female form). For example, the spirituality of *dedes*, who are religious leaders, is based on their genealogy and their ability to perform miracles (*keramet*), which is also believed to be an inherited characteristic. Since *dedes*' spirituality includes a divine and sacred power, *talips* (believers in Alevism)² believe that *dedes* can realise their wishes and desires (Cosan Eke, 2021). As the social anthropologist David Shankland (1998: 18), who has worked with Alevi for many years, stated, “In the context used by the Alevi, it is used to mean favored by God by virtue of being able to perform a miracle, as in Sufism in general” (1998: 18). Men and women pray in the same place during *cem* rituals, during which Alevi *dedes* address members of the congregation as “*Can*”, meaning “pure soul”, because Alevi believe there is no ethnic, economic and/or gender difference between those who attend the *cem* ritual after passing through the door of the *cemevi*. Although Alevi live predominantly in Turkey, Alevism as a faith has been part of transnational flows and networks in recent years, and they have settled in various countries as international migration has increased. Despite being a minority group wherever they live, they are the second largest belief group after

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- 1 One of the most important Turkish Sufi orders, Bektashism, is named after the Anatolian saint Haji Bektash Veli (d. 1271). Alevi place great importance on Haji Bektash because Haji Bektash Veli has a sacred authority in Alevism, given the messages of universal peace, unity and togetherness. (More information: Küçük, Hülya. 2002. *The Role of the Bektāshis in Turkey's National Struggle. A Historical and Critical Study*. Leiden u. a. 2002. Dressler, Markus. 2015. *Writing Religion: The Making of Turkish Alevi Islam*. Oxford University Press. Kara, Cem. 2019. *Grenzen überschreitende Derwische Kulturbeziehungen des Bektaschi-Ordens 1826–1925*. *Gottengen*: Vandenhoeck & Ruprecht).
 - 2 Not every Alevi can be called a *talip*. To be a *talip*, a person must define himself as an Alevi, belong to a particular *dede* lineage and promise a *dede* from his own lineage to abide by the rules of the Alevi faith.

the Sunni group in Turkey. However, this fact has not yet led to official recognition of their religious and cultural identity by the Turkish state.

There are two main reasons for this non-recognition. First, in the Turkish understanding of the secular state, there is a soft separation between religion and the state, because Sunni Islam has a privileged status in relation to political, economic and legal interests determined by said state. Second, under Turkish law, Alevi are included in the general Sunni-Muslim identity and are not defined as a separate faith group. Moreover, Alevism is used as an umbrella term to describe many different faith communities, including Nusayris, Bektashi, Abdals, Tahtacılar, Yörüks and Kurdish Alevi.

Alevi in Turkey lament that the political doctrine deprives them of their social, political and cultural rights, thereby causing a sense of social exclusion. Such a lack of official recognition has led to a position of undefinable and undebatable discrimination against Alevi in Turkish society. Moreover, they have been exposed to social repression, and even massacres, for centuries because of their faith, and so they have preferred to conceal their identity, a practice that is called *takiyye* (dissimulation). The beginning of the process of giving up hiding their identities is emigration, which has been increasing since the 1960s (Cosan Eke 2021).

The migration process from Turkey to Germany started with a recruitment agreement in 1961. This migration, which increased very rapidly with the recruitment of less educated and less skilled workers living in the villages in 1968, continued to increase in 1973 when immigrants started to live with their families in Germany under the Family Reunification Law (Cosan Eke 2021). It is estimated today to be around 4 million people of Turkish origin living in Germany (approximately 4% of the country's population) (Curtis 2013:69). Although the cultural representation of immigrants from Turkey as 'others' who do not want 'integration' is still being discussed, Martin Sökefeld underlines the studies that show that immigrants from Turkey have a high level of emotional attachment to Germany in terms of both media use and language skills (2004).

It is estimated that 48.5% of the population of Turkish Alevi living in Germany were born in Germany, and the majority of them are Ger-

man citizens (Sirkeci and Erdoğan 2012). According to Alevi organizations in Europe, 25% of Turkish immigrants in Germany are Alevis, while the AABF (The Alevi Federation Germany e.V. (Turkish: *Almanya Alevi Birlikleri Federasyonu*, abbr.: AABF) states that there are 700,000 Alevis in Germany alone (AABF homepage). It is difficult to talk about exact figures because there is no official data on the Turkish population based on their religious or ethnic identity. The emergence of organisations established by Alevis, which started in Germany at the end of the 1980s and spread to other European countries. These organisations have a very important place in the Alevi movement through their solidarity network, which includes local, national and transnational processes.

Nevertheless, there is currently no comprehensive study of all of the individual and collective rituals, ethical rules, canonical texts, doctrines, daily practices, roles and behaviours of religious leaders, organisations, identities, places of worship and activities that make up the Alevi faith, because local differences are integrated with the national and international divergences of Alevi communities. Jeremy Menchik explains this trend in his article:

This heterogeneity means that what constitutes religion is historically specific rather than universal. This fundamental observation has clear implications for researchers. A suitable theoretical starting point for researchers is institutionalized practices (contextualized and historical) in which practitioners of religions are involved. (2017: 564).

By following Menchik's contextuality, I designed my research on the institutionalised practices of a religious group, thus helping to limit my dissertation, and I focused particularly on Alevi associations and federations in Turkey and Germany. Although Alevis are exposed to assimilation, discrimination and legal non-recognition as a religious community in Turkey, they are nevertheless officially recognised as a divergent faith from (Sunni) Muslims in some German states³. As part of my thesis, I

3 The five states, namely Berlin, North Rhine Westphalia (NRW), Hesse, Baden Württemberg and Bavaria, accepted the applications for recognition of Alevis

discovered that the Alevi movement in Germany has endeavoured to develop its identity by combining religious, ethnic and national elements. Although Hurd (2008, 16) points out that “to define the boundaries of the secular and the religious is itself a political decision”, I have observed that the Alevi identity constructed in Germany has become more secular and has developed in ways that lead to a multi-layered transformation of their identities through migration experiences and political and legal arrangements (Cosan Eke 2021).

Consequently, thanks to their efforts, Alevi associations in Europe have been strengthened, and they have transformed themselves from a social movement into a transnational community (Sökefeld 2008).

Conclusion

Returning to my research question of whether I encountered any difficulties when doing my ethnographic research with a faith community, I can answer this question from two main perspectives. The first lies in the difficulties involved in doing research on religion itself, as it contains meanings, not only in terms of certain rules and practices, collective rituals and activities, but also in the personal and daily life routines of individuals. The meanings that this belief produces are involved in people's interactions with one another, in the form of language, objects, practices, written sources and stories (Wuthnow 1994). Therefore, religion is a multi-faceted research phenomenon and a “too unstable category to be treated as an isolable entity” (Hurd, 2015). Furthermore, due to a religion's inherent fluidity and heterogeneity, it is difficult to generalise about a faith group (Menchick, 2017). According to the British anthropologist Simon Coleman, who focuses his research on global Christianity, religion is a particularly difficult subject for the field researcher to

as a belief community. Alevis in Hamburg (2012), Lower Saxony (2013) and Bremen (2014) signed an 'Equality of Rights Agreement' with these state governments, thus granting them important rights and providing the community with legal status.

handle, partly because of its non-empirical nature and partly because of the “rational assumptions of most social sciences” (2002b: 77). It is difficult to deal with the theological frameworks of faith because the theological background of faith, as a form of spiritual discipline, is not an area of in-depth knowledge enjoyed by all people within that faith. These difficulties in defining faith, especially the theological narrative, may be a compelling reason for analysing field notes.

In this regard, the conceptualisation of religion consists of seven dimensions developed by Ninian Smart (Smart, 1998, 1999), which help the researcher gain insights into the fuzziness of religion and shape their field notes. These dimensions are narrative/mythological, doctrinal, ethical, institutional, material, ritual and experiential. The narrative dimension refers to the historical stories of a religion, communicated either orally or in written form. The doctrinal element refers to the philosophical nature of the religion, such as the nature of God. Next, the ethical dimension means the rules or laws of a religion. The institutional dimension includes the organisation of a religious community, for example within a church, a mosque or a temple. The material dimension refers to specific places that are important to a religion, whilst the ritual dimension concerns specific practices, such as prayer and meditation. Finally, the experiential dimension means strong emotional experiences generated by rituals (Smart, 1999).

Another difficulty relates to the position of the researcher in the field. At the beginning of my research, I did not have to put in much effort to be accepted locally, because most of the people involved in the research trusted me once they knew that I was an Alevi. However, I have not had the same migration experiences as the majority of Alevis living in Germany because the vast majority of the group I focused on in my fieldwork were Alevi immigrants from Turkey who had immigrated to Germany after the Bilateral Labour Recruitment Agreement in 1961 (Steinert 2014; Kolinsky 1996; Abadan-Unat 2011)⁴ or were born and raised in Germany

4 After World War II, there was a major labour shortage in Germany and high unemployment in southern European countries, including Turkey. The German government signed bilateral labour recruitment agreements with Italy

and had little or no personal life experience in Turkey. In terms of migration history, I can also define my position as that of a partial outsider to the research group.

Performing religious ethnography as an insider might make it seem that one is more competent in understanding a religious group and their experiences more deeply and authentically (Stringer, 2002 in Ganiel & Mitchell, 2006). Therefore, my research participants had high expectations from me and my work, especially in terms of Alevi theology, history, political and sociological knowledge.

As far as I am concerned, my insider position gave me a chance to gain access to the group, albeit this insider position as an Alevi was at the same time an outsider position, namely that of a researcher doing fieldwork. It was mostly difficult for me to live up to the expectations of the participants as an insider researcher because my position was not aligned with the migration process or the economic and social status of the participants. As a result, I decided to focus my research on participant observation and in-depth interviews as an outsider. At the end of my research, I noticed that the multiple identities involved in field research provide an opportunity for comprehensive perspectives. As claimed by Wilcox:

Many permutations of partial insider, totally inside and totally outside identities among researchers offer us a range of perspectives on the same phenomenon (2002:51).

Although the analysis of the researcher's position in fieldwork is complex, they are simultaneously an insider *and* an outsider, though the

(1955), Spain, Greece (1960), Turkey (1961), Morocco (1963), Portugal (1964), Tunisia (1965), and Yugoslavia (1968). More information: Steinert, Johannes-Dieter. 2014. "Migration and Migration Policy: West Germany and the Recruitment of Foreign Labour", 1945–61. *Journal of Contemporary History*. Volume 49, Issue 1. Kolinsky, Eva. 1996. "Non-German Minorities in German Society" in Horrocks, David & Kolinsky, Eva (eds). *Turkish Culture in German Society Today*. Oxford: Berghahn Books. Abadan-Unat, Nermin. 2011. *Turks in Europe: From Guest Worker to Transnational Citizen*. New York & Oxford: Berghahn Books.

dichotomy can disappear in the same research. After a while, when they started to invite me to their religious and cultural activities, they knew that I was not only a believer, but also a researcher from a university. Generally, the people who participated in my research allowed me to record their religious services for my research, even though they felt somewhat uncomfortable with this arrangement, because they had been pressured in the past in Turkey. This is a prime example of combining my internal and external positions. As stated by Setlhabi (2014), members of other cultures become insiders by researching a social setting and sometimes by spending extended periods of time in the field; on the other hand, members of a culture become outsiders thanks to their position as researchers.

During my doctoral studies, Martin Sökefeld provided me with unique advice, backed up by his theoretical and methodological knowledge and critiques of my research, which would also apply to future studies. Indeed, one of the most important perspectives he gave me for ethnographic study with a belief group is that it is necessary to avoid categorically problematising, essentialising and fixing differences therein, since ethnographic research can offer a comprehensive and deep analysis that includes changes and transformations within the boundaries of social and cultural contexts, in which 'reality' itself is actively created by all interactions.

After a long period of ethnographic research, I argue that the paradigm emphasising the duality between outside and inside, or between observer and observed, can be used to describe the diverse and ever-changing position of a researcher. This fluid status has positive effects on gaining access to data and the acquisition of multi-layered information in the field (Narayan 1993; Caronia 2018). Religious identity especially can be grasped deeply in the context of debating multiple identities. As argued by Martin Sökefeld, identities shaped by personal experiences are never fixed, as they are always "under construction" (2008: 20). In this context, both the position of the researcher and the identity of faith groups formed in the context of migration processes are subject to such dynamic variability, and both should be assumed to be always 'under construction'.

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Family 'Opening' the Field

From Ethnographic Odds to Ethnographic Teamwork

Menahil Tahir

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Family 'opening' the field: From ethnographic odds to ethnographic teamwork

I had met Rubab¹ multiple times in the café, where she worked long hours. We used to talk when there were no customers to deal with, but for a detailed conversation, we decided to meet at my house on her only day off, i.e., Sunday. The afternoon was cloudy and presented with no sunshine to enjoy, but she still chose to sit outside.

“Our house [in Afghanistan] also had a little garden like this,” she remarked.

1 All names used in this text are pseudonyms, based on various Afghan musical instruments.

It was only when the evening dawned upon us and started to make us feel cold that we chose to go and sit inside.

Just before she was set to depart, my mother came into the room with a little present for her. It was a small jewellery item, thinking that Rubab would like it. Exhibiting her usual concern – with which I am not at all unfamiliar – my mother added a pack of chips and biscuits to the package, since Rubab had not taken much with tea and was leaving without having had dinner with us. This was something rather unimportant for me in that moment; however, it acquired a great deal of significance because of the emotional worth that became associated with it.

Rubab was the only one from her family who migrated to Pakistan, in December 2021, with a group of her colleagues from Afghanistan. Her family (parents and siblings) moved to Iran a few weeks after her own migration. In those few hours we talked, she mostly discussed her family, including the many crucial and grim times they had gone through. Her voice remained steady throughout. At times, though, she struggled to find words – not because she was overwhelmed with emotions, I felt, but because she was talking in a foreign language, namely English, despite being fluent in it.

However, the pack of chips was what forced the tears to flood from her eyes. Hugging my mother, she cried uncontrollably; my mother wept, too! I was there, sitting next to Rubab, not knowing what to do in such a situation and for the most part just being a silent observer.

“My mother used to bring this for me whenever she went outside, because she knew I really liked it!” Rubab expressed while still crying and holding that pack of chips in her hands.

My mother responded consolingly.

“You are always welcome here; come whenever you desire.”

My mother reminded Rubab of her own mother. I had an idea this may be the case, because, more than once, she had stated, “You are very lucky; your mother is with you.”

The discussion with Rubab left me wondering if my understanding of the ‘field’ and the people constituting it could ever be complete and entirely wholesome without my family in it!

This vignette presents a glimpse into what I had never expected of my fieldwork – the active engagement of my family in the field, and then the field site eventually coming to include my home in it. As Stars et al. (2001:76) rightly noted, “To bring fieldwork and family together is a joy, if a refined and nuanced one.” My ‘partial sightedness’² served as the first and the foremost reason for my family getting involved in my ethnographic fieldwork. In their efforts to ensure that my impairment would not pose significant limitations and hinder the construction of ethnographic knowledge, my family played an active role throughout, thus enabling me to keep going, thanks to their multifaceted support in the field.

In ethnography, the field has been defined as “an almost random assemblage of sites that come into coherence through processes of fieldwork itself” (Ready 2009: 90) and “a thoroughly social network” (Middleton/Pradhan 2014: 363). In my ethnographic experience, my family significantly – if not equally – contributed to making and negotiating the field and in mobilising social networks. This subsequently opened new ethnographic possibilities for me, many of which I now intend to elucidate through this text.

Setting up the field

Peshawar is the administrative capital of the Khyber Pakhtunkhwa province in the north-western part of Pakistan. With the aim of conducting ethnographic research with Afghans in Pakistan, I selected this city as the primary site for fieldwork, based on two premises. First, Peshawar was close to the Pak-Afghan border crossing at Torkham, and second, a considerable number of Afghans had been residing there for decades. Up to that point, most of the involvement I expected of my

2 The use of ‘partial sightedness’ here is only to make it clear for the readers and/or listeners, based on the general understanding of the term. But in fact, for me, it refers to my ability to see as many others do not, despite my inability to see as many others can.

family in my research was to mobilise their social network(s) and to find suitable accommodation. This is in line with what other female anthropologists, who were unmarried at the time they conducted the ethnography, have (briefly) dwelled upon by mentioning their fathers accompanying them to the field. For instance, Lila Abu-Lughod's (2016 [1986]) father, who himself was an Arab (though not a Bedouin), understood the culture and society well and was the one to introduce her to the Bedouin community in Egypt. Later, she realised that his presence ensured respect for her in that society as a result of being accompanied by male kin. Something similar was noted by Vandana Chaudhry (2017), whose father accompanied her on fieldwork in India, to help her make initial arrangements. Based on her experience, she opined:

I quickly realized that his presence in the field, even briefly, was commensurate with cultural norms of gender and familiarity. In the context of a patriarchal regional culture, having my father in the field established social boundaries of propriety and provided security for me to conduct my research (2017: 74).

In addition, I also sought the support of my family in finding a dependable companion for the fieldwork, keeping in view my personal limitations – based on my 'partial sightedness' – and the associated potential challenges related to orientation, navigation, and safe mobility in an unfamiliar space.

My initial plan was to stay for a couple of weeks with my family in Rawalpindi before starting fieldwork in Peshawar. All was set, but almost nothing went according to plan. Just a day after I landed in Pakistan, a suicide bomb explosion in a mosque in Peshawar during the Friday prayer on March 4, 2022, claimed over fifty-five lives and injured more than 190 worshippers. This was one of the first steps in taking me away from Peshawar. There is a history of police harassment towards Afghan refugees following terrorist incidents, particularly after the massacre in the Army Public School (APS) in Peshawar in 2014 (cf. Human Rights Watch 2015).

Considering that the number of undocumented Afghans residing in Pakistan had increased significantly after the Taliban came into power in August 2021 (Voice of America 2021; UNHCR Pakistan 2022), local informants told me that areas hosting a greater number of Afghans were under increased surveillance. Subsequently, I expected that doing ethnographic fieldwork in Peshawar might not turn out to be as productive as I had previously hoped, keeping in view the heightened insecurities and concerns of my prospective interlocutors. Simultaneously, during the first few weeks in the Twin Cities of Rawalpindi and Islamabad, where I intended to conduct only a few meetings with some official sources, I realised that the Afghan population had increased significantly,³ as evidenced by their discernible presence in public places such as markets and parks. Despite not giving up hope for fieldwork in Peshawar, I actively started to explore the Twin Cities as the field site which allowed my family to become part of my fieldwork and the field also came to include my home in it.

Navigating the field with my parents

Both of my parents had already retired from service when I began my fieldwork in March 2022, and so they were readily available to facilitate me in my research. My father, a doctor by profession, worked in one of the biggest public hospitals in Rawalpindi for most of his career. My mother, on the other hand, primarily worked as a public education administrator. As such, both had a wide range of experience when it came to meeting people from all strata of society. Their involvement in my fieldwork was neither planned nor sudden; it was progressive. It

3 More than 73,000 individuals had been registered by the Society for Human Rights and Prisoners Aid (SHARP) office Islamabad by March 16, 2022 (number disclosed in personal communication), which is responsible for the reception of asylum seekers in the capital city and its adjoining/neighbouring areas/cities. See also, *UNHCR Operational Data Portal*, "Afghans in Iran and Pakistan" for updated data on Afghans in Pakistan.

began due to my mother's protective nature and her repeated concerns "*mujhay tasalli nahin hoti*" (I don't feel satisfied), followed by their somewhat evident motivation to address the practical challenges in the field so that my ethnography would not become a "broken" one characterised, according to Randol Contreras (2019: 2), by setbacks and obstacles, "or one that seemed impossible to finish – or get off the ground".

Trying to keep (field)work and family apart, I exhibited much reluctance in the beginning. During the initial months, with a few exceptions, it was only my father who often became a part of my research, as I usually went with him to meet my research partners. Despite my utmost longing, however, I could not be the 'lone ethnographer' and was to rely on another individual for safe navigation of the field. This led me to having a general feeling that I was received differently when accompanied by a non-related assistant⁴ and when with any of my family members. Whether it was the intimacy of the relationship (i.e., a daughter accompanied by her parents) or their credentials that influenced behaviours, I was welcomed more warmly, respected and readily trusted when accompanied by one or both of my parents.

Many ethnographers, in addition to dwelling on the positives of going into the field with family – especially children – have also brought into the discussion a number of complications, such as lodging, logistics, caregiving and dealing with the tantrums and moods of children (cf. Starrs et al. 2001; Stobart 2020; Johnston 2015). Contrastingly, I found it convenient and less challenging, primarily because I was accompanied, in most cases, by one or both of my parents, who not only ensured my security and safety, but also made the environment light and open, owing to their amicable and sociable natures. Starting with the spirit of support, it became a matter of companionship – and clearly the field would not have opened the way it did for me if it were not for my parents.

4 In my research, non-related assistant refers to any individual who offered help to navigate the field, mostly by accompanying me to interviews, outside of my parents and sisters.

From reluctance to acceptance

During the first week of my fieldwork, in March 2022, I met Tambur, an official of a non-governmental organisation (NGO) that is also one of the implementation partners of the United Nations Refugee Agency (UNHCR) in Pakistan. Starting with a warm reception to leaving the office with a souvenir (a mug with the organisation's logo on it), the meeting of over an hour (in which my father also accompanied me) was very revealing for me. Largely remaining focused on the topic, it was only towards the end of the meeting that I no longer remained his addressee. He had assigned that role to my father, telling him about his parents, particularly his mother, and their role in his life, also in the form of their dreams for him and their prayers that kept him motivated to achieve those dreams. "He would not have shared this if you had not have been there," was my remark to my father on the way back home – an opinion he also supported.

It was only in June 2022, at an event organised by UNHCR on World Refugee Day, that I met Tambur again – this time, with both my parents. Quite interestingly, he shared the same information about his parents with my mother. "You are very lucky that both your parents are here with you," was one of the few sentences Tambur said directly to me. Having lost both his parents at an incredibly early age, he was referring to the support that my parents had extended to me to realise my research. I had so far been hesitant towards my parents accompanying me, particularly in formal settings, but it was from this point on that I settled on seeking no other assistance, and they formally became my research companions.

Engaged presence: Enablers or influencers?

My family became a valuable resource in establishing deeper relations in the field, fostering an environment that further enabled me to conduct my ethnographic fieldwork. I particularly owe my mother's presence extensive credit for allowing and aiding me to develop more accomplished relations in the field. Having met Ghichak, along with his wife

and children, multiple times in the protest being staged by the Afghan immigrants in Islamabad,⁵ we decided to meet him at his house. He lived on the first floor, and the staircase had no railing on the side. Ghichak offered my mother his hand for support, saying, “You are just like my mother.” In relation to this statement, I had become his sister by the time we departed, and he clearly expressed this sentiment while bidding us farewell – and with that, he shook hands with me.⁶ In addition to addressing me as ‘*Menahil jaan*’ (dear Menahil), he also started using the term ‘*khahar jaan*’ (dear sister). From that day onwards, we maintained our fictive relationship, which owed its basis to my mother.

Correspondingly, it became very usual for my interlocutors to ask me about my parents, if they did not meet them with me, and resultantly asked me to convey their regards to them. The warmth and affection exhibited towards me by my family, also in the form of support, did not escape the notice of many of my interlocutors. Apart from the remarks such as “Your mother is very kind” and “Your father loves you a lot,” their presence (including that of my sister), even if only for a few minutes, was sometimes enough to prompt a conversation about my research partners’ loved ones, particularly those who were no longer with them.

In the café where Rubab worked, my sister accompanied me a few times to ensure that I arrived there without any trouble, following which she would leave and then return after a couple of hours for us to go back together. One day, while I was there, Tula, another of the staff members, asked me if my sister would be coming that day. When I informed her that she might arrive a bit later, Tula took a seat in front of me and started talking about her sisters in Afghanistan regarding their education and (unfulfilled) desires and dreams about their professions (one of

5 This protest, in front of the National Press Club, of the Afghan immigrants/ asylum seekers – most of whom migrated to Pakistan after the Taliban took charge of the country’s affairs in August 2021 – started as a sit-in in April 2022, with the slogan “Kill Us”. Over time, the protestors divided into three groups: ‘Kill Us’, ‘Save Us’ and the ‘Hazara Asylum Seekers’.

6 This came as a bit of a surprise to me, as two unrelated individuals of different genders, culturally, do not come into physical contact with one another.

them had the same profession as my sister). Although my sister was not there, she had a role to play in shaping our conversation and making Tula believe that I would understand her feelings for her sisters, with whom she shared a strong bond.

Going into the field with my family brought to the fore some aspects of my familial life, in addition to me being the researcher. Hence, having my family with me also contributed to an increased rapport without any conscious effort on my part. With most of my interlocutors, I had developed a reciprocal relationship whereby I was not the only one accessing personal information, as they also got to know my family and me. This resulted in reduced suspicion, increased trust and a greater willingness to have open, honest and comfortable conversations. This complements the views of Chaudhry (2017: 74), who has maintained that the socialisation at this level, where the ethnographer presents his or her familial being, has the potential to help gain “social legitimacy” and “cultural synergy”, which was also evident in my interactions in the field. The multifarious roles that my family took on during the fieldwork all indicated one common aspect – their influence over the extent or depth of the emotional relationships I formed with my research partners. Additionally, in a way, they also sometimes guided the content of the discussions even without being present.

My perfect companions

The engagement of my parents not only contributed to establishing a comfortable setting in which to conduct difficult conversations, but their socialisation skills and an immense pool of stories based on the people they knew proved to be an added advantage. This was further illustrated in May 2022, when I went for a short trip to Peshawar accompanied by my parents. I had already established some contacts through telephone or during my previous visits, but this trip was dedicated to Zirbaghali, an Afghan residing in the central part of the city. We had met him pre-

viously, during one of the visits in April, and also had *Iftaar*⁷ at his home with his family. Zirbaghali had agreed to introduce me to a few individuals in his Afghan network. The next day, he took us to his friend's place. His friend, Daap, was born in a refugee village in Pakistan but had stayed for a few years in Kabul (Afghanistan) for the sake of his degree in medicine, after which he returned to Pakistan in 2017.

The sitting room presented an amalgamation of Afghan and Pakistani styles – a couple of sofas and a centre table (the Pakistani element) in addition to floor cushions (the Afghan component) lining the opposite wall. My mother and I settled ourselves on one sofa while my father and Zirbaghali sat on the second, in a position perpendicular to ours. The host chose to go for the floor cushion in front of me and my mother, with Zirbaghali on his side of the sofa. Our conversation, after some time, shifted to his choice of profession, which was influenced by his brother, who had also been a doctor. When I probed into the use of the past tense in reference to his brother, he confided that he had been missing since 2005. My father, sitting next to Zirbaghali, heard him say something to Daap in a relatively muffled voice and in Pashto,⁸ after Daap mentioned his brother who went missing. Following his instinct, my father related an anecdote of one of his colleagues who went to Afghanistan to fight and was killed when a rocket hit him outside a cave.

This brief account was what made Daap comfortable enough to open up. He picked the conversation up from there and continued, talking about his brother who had gone with his wife to Afghanistan; both had, in fact, conducted suicide blasts targeting foreigners. He also shared details about the date and location of the blasts as well as the number of casualties and their nationalities. It took me a couple of minutes to recover from the shock of the information shared. Meanwhile, my mother, out of sheer curiosity, asked a couple of questions regarding his brother's family, not only leading the conversation, but also giving me ample time to

7 Iftar is the fast-breaking evening meal of Muslims especially during the month of Ramadan.

8 The conversation was otherwise carried out in Urdu. I have only a basic knowledge of Pashto; my parents do not.

recompose my thoughts. This vignette provides an insight into how my parents turned out to be my perfect companions in the field, complementing my (in)abilities with their exposure, experience and amiability.

(Supporting) 'sight' at my side

Planning my field activities with my parents not only made it all much simpler, but also gave me an added sense of security and comfort. Furthermore, through them, I gained access to supplementary '(eye)sight' to complement mine. This also facilitated me in 'looking' for familiar faces, especially in larger groups, as my parents (owing to their sustained engagement) started to recognise them all, and vice versa. In March 2023, an educational institution for the Afghan community, founded by a couple of Afghan immigrants, celebrated its first anniversary, in which I participated with my mother. The event, organised on a rooftop, had a gathering much larger than I expected. My mother, in this setting, played two crucial roles: first, by identifying the individuals we were meeting as well as pointing out who else was present there from the ones we knew,⁹ and second, helping me make sense of the happenings around us through her 'running commentary'.¹⁰ Consequently, it was through her that I got to know the details, including the type of (traditional) dresses (and jewellery) the participants wore and the performances on the stage.

This 'running commentary', or verbal description, was not limited to special occasions. Instead, my family members also lent me their eyes to make sense of the visual content (such as photos and/or videos) shared with me or else shown to me by my interlocutors. These ranged from the pictures of their houses in Afghanistan to photographic evidence of the physical torture that some of them had experienced before coming to Pakistan.

9 It is challenging for me to recognise individuals in large gatherings, with noise as an added factor contributing to this difficulty.

10 This is a habit my family members subconsciously developed over time, benefiting me immensely.

From May to July 2022, I paid frequent visits to the Afghan immigrants' protest site(s) in front of the National Press Club in Islamabad.¹¹ With the exception of the first few visits, I was always accompanied by both my parents. Being the only Pakistanis visiting the protest site rather frequently, we started to receive a warm welcome, with hospitality extended in every possible manner.¹² As my mother always remained by my side, her 'running commentary' explained most of what was going on around us. It became a routine that our way back home (not only from the protest but everywhere) turned into a debriefing session, with either or both of my parents sharing their part of the story with my insertions in between.

Admittedly, the 'running commentary', as well as the sharing of information afterwards, served as an important part of my data collection process. This primary data from fieldwork, mostly based on visual observations, took the form of secondary data for me, as it was perceived by eyes and processed by a mind other than mine before it made its way to me. This complicated the dynamics of me being the participant observer in my ethnographic study. My parents, who often assumed the activities that any fieldworker would undertake, were not intentionally being participant observers. Instead, my fieldwork served as an episode in their personal lives whereby they were simply accompanying their daughter in the field.

Blurring distinctions: Fieldwork or family time?

Where relationships extended also to my family, it became even more difficult to draw a distinction between the private and the professional. Introduced to them through fieldwork, from having meals to celebrating (cultural) events together, I became entangled in the conundrum

11 One of the groups (mentioned in footnote 5) moved to a different place for almost a month in between.

12 We were offered drinks (including water), watermelon and boiled eggs, depending on availability.

of whether to record conversations and observations in fieldnotes or to keep them as part of my personal life, as those moments were not shared with me as an ethnographer but were rather family oriented. For instance, I experienced this on the day following *Shab-e-Yalda* (the longest night), i.e., 22 December, 2022. Ghichak, who had been living with his family in Pakistan for over ten months by then, shared a song in Persian related to the festivities of *Shab-e-Yaldah*. Following this, he invited us (my family) to have dinner at his house. I had not met him and his family for the previous several months, as I had been in Germany. I deliberately chose not to take fieldnotes of this occasion, considering it solely an interaction between two families. However, I did talk to Ghichak about some points later that had been brought up in our meeting that evening – ensuring I did not miss something significant in my research because of the entanglement of the (field)work and family.

Unlike the aforementioned interaction, when I decided to keep it private, the gathering of friends to celebrate Nowruz (the New Day) did make its way into my ethnographic data. Nowruz is celebrated on March 21 to mark the Persian New Year. In 2022, it fell on a Monday, a working day in Pakistan. Therefore, a group of Afghans had decided to gather in a public park on Sunday to celebrate the day in advance. Mangey, who also partly assisted me in the first couple of months of my fieldwork, being one of them, invited me (with family) to the gathering that evening. As I was only a week into my fieldwork, I found it a good opportunity to get introduced to his acquaintances, with whom I could arrange individual meetings later.

This group of Afghans was diverse in composition, ranging from those born to refugee parents in Pakistan to others having arrived in the country only a couple of weeks prior to our meeting. It also included Afghans visiting Pakistan for the first time, in addition to the multiple crossers, i.e., those who had lived (as refugees) in Pakistan before, returned to Afghanistan and then travelled back to Pakistan in 2021. Greatly intrigued by the diversity and richness of their experiences, I was on my toes all the time, so as not to miss anything or any conversation going on with or around me. It was only after dinner that I became more actively engaged in the festivities, in which my family (parents

and sister) had been involved from the beginning. My parents, being the oldest at the gathering, enjoyed special status and were referred to as '*hamaray mehmaan*' (our guests). Though Mangey was the one who had invited us, the whole gathering extended immense hospitality, trying their utmost to ensure that no one was left unaccompanied or unattended.

In addition to this celebration, which turned out to be a recreational activity for my family, a couple of trips to Peshawar in April 2022 also turned into a family vacation. They were planned especially for weekends so that my sister could also join us. It was not only the family that travelled together, but also Mangey, who had arranged the meetings in Peshawar. Going with my mother and sister was particularly beneficial, as they could accompany me inside the house, where male and female guests were hosted separately. Also, the usual 'running commentary' of my family members – including reading sign boards, picking out the names of shops and scripts on any passing vehicle and commenting on whatever they thought might be interesting – always made any form of travel much more enjoyable for me.

Taking the Grand Trunk (GT) Road for our trips, this commentary made the presence of Afghans very evident as soon as we entered the territory of Khyber Pakhtunkhwa province. Starting from Nowshera to Peshawar, the names of 'driver hotels' and restaurants increasingly depicted an Afghan element in them, primarily through the use of 'Kabul' in their names. From eating Mantoo (from the place recommended by the locally residing Afghans) to having the typical Afghan *Jalaibi* (much different from the usual Pakistani style ones) in Board Bazaar Peshawar, and then also having *Iftaar* in an Afghan restaurant on the way back, all were made possible solely because of arranging it as a family trip. It was moments such as these that added an extra flavour to the ethnographic experience, making it more memorable while at the same time contributing to the increased indistinctness between the fieldwork and family time.

Remaining situated in the field

The increased presence of my family during fieldwork, aligned with the fostering of family ties, had a few expected as well as surprising outcomes. Many of my research partners struggled with language and communication-related issues during their initial few months in Pakistan, and the lack of valid travel documents further compounded their troubles. These issues also aggravated their access to free medical facilities provided by the government in Pakistan. Since my father had maintained contacts at the hospital he previously worked at, which was also in close proximity to many of my research partners, it not only presented us with opportunities to express solidarity, but it also became a means of extending relations with them. In many cases, they directly contacted my father, who always tried his utmost to facilitate them. As my father made himself available in case of any need – even when I was not there – he also became a reason for them to cooperate with me.

Upon my return to Pakistan for the second phase of my fieldwork, after around five months, one of my research partners – after affirming his support – remarked, “Do pass on my *salaam* (regards) to Uncle (referring to my father); your dad is such a gentleman!” Hence, the fieldwork resulted in me as well as my family making friends in the field; a few of them even became Facebook friends with my father. After almost a year since I began my fieldwork, it came as no surprise when my father received a message from one of my research partners to inquire about our safety after an earthquake hit Afghanistan and Pakistan on March 21, 2023. From receiving greetings on special occasions, such as Eid, to hosting people (with or without family), originally known through my ethnographic fieldwork, in our home, even in my absence, this clearly indicated the formation and sustenance of ‘family’ ties in the field. Korpela et al. (2016) highlighted the same when stating that, for accompanying family members, these relations (in the field) are not associated with any (field)work but form part of their private/social lives.

Having my home in the same city, namely Rawalpindi, meant that my family was situated in the field without having to exit it, even when I had to do so via a return flight to Germany. This further contributed to

strengthening certain relations in the field. For some, who had either lost their intimate relations or were away from them, my parents, especially my mother, were much-desired company. For instance, Dahina, who lost her mother in the suicide bomb blast at the Hamid Karzai International Airport in Kabul in August 2021 (which claimed more than 180 lives and injured more than 150 individuals), asked my mother to continue visiting her even after my return. Hence, even when I left the field, my family remained in situ. As a result, I came to realise that the presence of my family in the field played an added role in sustaining relations with my interlocutors.

Concluding remarks

I cannot agree more with Starrs et al. (2001:74), who maintained that “Fieldwork with family invokes a whole range of experiences”. These experiences are not only limited to the fieldworker, but also extend to family members. This text, nevertheless, only presents my voice and sheds light on my thoughts and reflections as an ethnographic fieldworker accompanied, for the most part, by her family in the field. In attempting to engage in the discussion on mitigating the practical and methodological challenges during my fieldwork, I also acknowledge the fortuitous yet significant role of my family, particularly my parents, in producing ethnographic knowledge, which is, nevertheless, not free from their influence.

Presenting a reflexive account of our ethnographic teamwork, I dwell on how the field was co-constituted by me and my family. My ‘partial sightedness’ is what resulted in their involvement, and paradoxically this same impairment opened up new avenues for me to explore. Perhaps this appears as an ethnographic odd, but I share my methodological experience as a researcher with a relatively different set of (in)abilities, which ultimately led me to explore ways to not ‘see’ my personal limitations as hurdles but to transform them into creative opportunities. My family contributed to the relationships I established in the field by fostering trust, comfort and openness which subsequently influenced the quality

of the information shared and the time spent together with my research partners. Hence, I maintain that it was largely my family, my parents in particular, that brought the field into existence for me.

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Giving Up the Field

When Friendships Outweigh Scholarship

Stephen Lyon

Earning the trust of people in a community, and particularly in specific families, is time-consuming and imposes obligations on all parties. In ethnographic research based on long-term participant observation over several years, the relationships that develop invariably veer into a host of different forms. Not all of them become close or affectionate, but as with our natal family relationships, there are levels of intimacy that affect people across the entire network. I carried out field research in a Punjabi village in 1998 and have continued to visit this community on a semi-regular basis ever since. It took years to work through locals' expectations of how guests must be treated and how unrelated men could interact with local women. After more than a decade, I felt rewarded with a 'goldmine' of anthropological access. I worked with a handful of households in a community that was ostensibly very closed to outsiders but who had nonetheless generously opened their doors to me. It was at this point, however, that I began to realise there was a new barrier to my research ambitions. Although I now had privileged access to several households, documenting and reporting on their private details was tantamount to a betrayal of the people I had come to love and think of as my own family. On the one hand, I was in a much better position to understand the ethnographic phenomena that are the bread and butter of anthropology, but on the other hand, publishing ethnographic accounts from my 'primary' field site could hurt people. Like Martin Sökefeld, I believe I have come to prioritise the research that might be the most beneficial for the people I have come to care about, rather than ruthlessly, in

my view, pursuing the research questions that might be the most compelling to me personally. Ultimately, I am not convinced that my research contribution will be less significant, but I am aware there is knowledge that will never be shared with wider audiences – and that is not something I would have predicted would be part of my scholarly legacy. To be clear, it is unlikely that the people with whom I have worked will ever read my publications in particularly large numbers, but details about human weakness and mistakes can be used to undermine the reputations of individuals and households. To disguise identities whilst divulging private information would invite speculation that could wind up being directed at anyone. The last thing I want is for my work to become fodder for baseless accusations that could damage someone's standing in their community.

Best laid plans...

Anthropological research rightly encompasses scientific approaches, and it should be empirical, rigorous and, above all else, honest. I have taught multiple cohorts of students to think carefully about their research design before they commence fieldwork. I make them read methodological discussions in the discipline from the days of armchair anthropology, the early field working anthropologists, the challengers to paradigmatic unity following the Second World War and, of course, the heady days of Marxist and Feminist anthropology in the 1970s and the nod to literary approaches of the 1980s and 1990s. The fragmentation of methods has mirrored the breaking up of theoretical approaches in the discipline throughout this time. Some of this fragmentation has unquestionably been positive for the discipline, but it has resulted in an academic field that often appears in disarray and as if it cannot agree on its own boundaries or priorities. My aim in this chapter is not to lament the loss of an anthropological paradigm but rather to offer a brief description of my personal journey, from intellectual certainty to the shifting sands of a scholar who has had to prioritise people's wellbeing over some research aspirations.

My journey maps on to some of the decisions that Martin Sökefeld has made in his career. Like me, Martin's anthropological studies were conducted in the shadow of *Writing Cultures* (Clifford and Marcus 1986). We were both trained in Europe, but the differences between Anglophone UK traditions and those of Germany may have affected some of our decisions.

My doctoral fieldwork began in early 1998, when I arrived in a Punjabi village to understand local farming practices. I had a clear agenda that included participating directly in farmers' tasks. I wanted to till soil, join in with the routine maintenance activities of farm equipment as far as possible, sow seeds, engage in weeding and harvesting activities and then tag along when the crops went to market. My aim was to learn local dialects and hang out with the people who actually worked on the land. I had little interest in the powerful landowners, but of course, I understood that I would never be granted permission to carry out any research without their consent and support. So, my introduction to the 'village' was actually to the landowning households. My first visit was in the company of extension workers and agricultural scientists working for an international NGO and the national Agricultural Research Center. As a result of the specific entrée into the village, I wound up interacting with the peasant farmers associated with the household that first welcomed me.

I had lived in Pakistan before, so I knew the significance of hospitality; as a guest, I would not immediately be allowed to engage in work that made me tired and got my hands dirty, but I was confident that I would eventually work past the constraints of hospitality and become someone more useful to my new neighbours. Following those initial visits to the village in early 1998, I returned to the UK and created a detailed research plan to cover not only twelve months of fieldwork, but also the writing up and publishing parts of the process that followed. I therefore arrived in the autumn of 1998, fully armed with a detailed, week-by-week research plan to learn as much as I could about local farming practices, social organisation and economic patterns.

It will come as no surprise that very little of my elaborated research plan survived beyond the first few weeks. I continued to try to force bits

of the plan back into my daily activities, but the resilience and determination of the local community proved far more effective than my will. In the end, I had to write a very different doctoral thesis, and my research questions were completely derailed to match the observable priorities of the landowning households. The capacity, and indeed the necessity, to adapt to local priorities is perhaps one of the more common features of ethnographic fieldwork.

The reality of my straitjacket came home to me in a number of visceral ways throughout that doctoral research period. For instance, on one occasion, I was hanging out with some local peasant farmers who were cutting fodder with a hand scythe. I squatted down and used the hand scythe to imitate them. I was doing an okay job, though clearly not as efficiently or as elegantly as the people around me. They found this very amusing and were happy to let me carry on for about 10 minutes. As I started to really get into my stride, a car drove by on the nearby road, about 100 metres away. On seeing the car, the men immediately jumped into action and took the scythe away from me, manhandled me onto the *charpai* and shoved a cup of tea into my hand. They frantically told me to relax and look like I was having fun.

I was a little taken aback and asked why I could not continue cutting fodder. They looked nervous and explained that the car belonged to people from a neighbouring village. They then went on to say that if those people reported to the landlords in our village that the European guest was being made to do manual labour, they would all be in a lot of trouble. I argued with them and said that it was my job – I was *supposed* to be doing manual labour. I told them, not for the first time, that I did not come from a rich family; part of my ancestry was Appalachian hillbillies who knew all about hard labour and surviving off the land. It will come as no surprise, however, that they were not persuaded.

I raised the matter with the landowners, who were equally unmoved. They reminded me that I was their guest, and they would never ask their guests to do manual labour. In one exchange that I now find amusing but which was frustrating at the time, I shouted at my best friend that he was interfering with my work. He shouted back that it was *his* village:

“The happiness of everyone in my village is *my* responsibility!” he shouted at me.

“You don’t know what will make me happy!” I shouted back.

He shook his finger at me and shouted, “Trust me, Dr Sahib, you don’t know what will make you happy! I know better than you what will make you happy in *my* village!”

I learned so much from this man, but much of it was difficult and even sometimes infuriating. After the fact, though, I can look back with eternal gratitude. He did not let me do what I wanted to do, but he *did* help me understand his village and his culture *far, far* better than my carefully crafted research plan ever could have done.

Narratives and reputations...

I therefore adapted my research plans by compromising my objective scientific aspirations out of respect for the constraints that people in the local area felt were non-negotiable. I had set out to produce a comprehensive account of indigenous, or at least local, farming practices that included etic metrics of crops, soil composition, fertiliser and pesticide systems along with an elaborated model of decision-making among primary food producers. In the end, I developed an argument around the narratives of conflict, resolution and prestige. At that point, it was relatively simple for me to publish in ways that did not compromise anyone’s reputation, because the focus was on publicly known and shared narratives. I returned to the UK, completed my doctoral fieldwork, wrote up the thesis, turned some chapters into a few journal articles and published a version of it as a monograph (see Lyon 2004). All in all, I did not, at that point, realise just how much compromising I would wind up doing as my relationships became closer and people started sharing ever more ‘secret’ stories of their lives and their relationships. I also did not appreciate at the time just how comfortable I would be with *those* compromises.

After almost nine years from the first time, I visited the village I considered my ‘primary’ field site, and I was deemed trustworthy enough

to meet the ladies of the landlord families. This was not a pre-planned event, or at least if it were, the planning did not include me. I was in the village for a few weeks and somewhere in the middle. I was in my little room, alone after 10.00 at night, when someone came knocking on the door. He urged me to follow him, as one of the Maliks was requesting my company. I was a little tired and half felt like making an excuse but did not. As so often happens, those occasions when I overcome my fatigue or timidity turn out to be some of the most profound opportunities.

I followed the young man through the maze of little passageways through the village and went through a door I honestly had not even noticed before. It was small and I had to crouch down to get through. On the other side, it opened into a lovely spacious green garden. Sitting in chairs in a semi-circle were about a dozen women. They were all smiling and beckoning me to come sit down in an empty chair on the end of the semi-circle. I was a bit speechless and did not know exactly what to say or how to behave. I was acutely aware that 90% of the Urdu I had spoken in my life had been to men and largely about things that men do. I was not entirely confident that I had the right vocabulary to speak to women in Urdu.

We had a lovely dinner during which they asked about my children, my wife, my university and what I thought of the food they had been providing over the years and what I thought of the village. At the end of this world-changing evening, as I was walking back to my little room, one of the young landlords explained that they had known about me for nine years and wanted to meet me in person. It could have happened sooner, he explained, if the ladies had asked for the meeting. I will never know if that is true. It could be that the ladies had asked for the meeting many times before, but the men did not trust me, or that it was indeed an accurate account of why I was privileged on that occasion.

There was a sea change from that evening. I was not suddenly given unfettered access to family areas, but people were more relaxed about gender politics and more forthcoming in explaining women's involvement in household decision-making. When people called me 'Brother', it somehow felt more sincere than before. I was trusted enough to interact, however superficially, with the women of the landowning households.

What I had not appreciated was the extent to which this would render much of my 'research' on the landowning families problematic, if not outright impossible. This was brought home to me in a visceral way in two incidents that occurred a few years after I was allowed to cross the hitherto strict gender segregation barrier, referred to as *purdah*.

I had a small grant to study *khawjasara*, or the South Asian Third Gender. I was working with a colleague in the UK who was studying trans* communities in the UK and the US. At first, everything went smoothly. I explained my research questions to my village 'family', and they really expedited progress. One of their relatives living in the city had a *khwajasara* domestic worker who was apparently happy to sit down with me and talk about her life.

Trouble began when I explained that I wanted to visit a *khwajasara* household and spend time with the Guru and hangout observing the ordinary life of the third gender in Pakistan. My village brother was shocked. A conversation with a trusted *khwajasara* behind closed doors was one thing, but to publicly go to a *khwajasara* household and spend significant amounts of time with people from that community would do irreparable damage to those people closest to me.

I could not understand how this research project could possibly harm people who were not involved in it, beyond an initial introduction. Then my brother patiently explained that I was not at liberty to do whatever I wanted anymore – I was part of his family. Anything I did could impact the *izzat*, or honourable reputation of my village family. As he talked me through his rationale, I had visions of that evening not long past, when the women of the family had put their collective foot down and insisted I was a real brother who should be allowed to interact with his sisters and aunts. There was a price to pay, and this was part of that price. The reputation of that family mattered enormously to me – I did not want to harm them and have tried hard never to do so since. But once I was so intimately drawn in, I no longer had the authority to decide what protecting their reputation meant. Their own attitudes and values had taken priority over my reasoning, and so they needed to monitor and police my behaviour – just as if I had been born into one of the households. My

‘guest’ status had been revoked, and what I did mattered in a way that it had not before that fateful integration.

The example that finally ended my ‘researcher’ relationship with the village occurred several years after I abandoned my plans to study *khwa-jasara* properly. I had been given the opportunity to interview one of the oldest women in the village. She was happy for me to record these interviews, and I was ecstatic at the opportunity to really be allowed to have more in-depth conversations with a woman about life in the village. I was curious about how women influenced decisions, how involved they were in land disputes and a host of other subjects that could only be addressed by speaking directly to women.

We had several fantastic conversations, and this wonderful older woman dropped several bombshells of revelation on me. She had such a rich knowledge of her village and the landowning households, and she told me stories that contradicted some of the tales the men had told me over the years. She quashed any notion that women were only interested in food and children – an assertion that had been made by more than one man over the years.

Then, her grandson came to me and told me not to publish anything she told me. I was surprised because she seemed so relaxed about it all and wanted her story to be recorded and known. I have had many opportunities to meet her since my conversation with her grandson, and occasionally I ask her about these restrictions. The first time I asked, she laughed and said, very easily, that the men in her family would not like some of those stories to be known outside their own intimate circle. I was, she said, her son, and so I needed to know these variations in the family history, but they were not for people outside the family.

The privilege conferred on me by that family is immeasurable. It’s humbling and it’s huge. It’s also a substantial barrier to satisfying one of my professional duties as a scholar. I always tell my students that research that is not published is incomplete. I believe that wholeheartedly, but I also believe that the recordings I have produced of this lovely woman are invaluable and should be preserved forever, but *never* shared. Her words of wisdom are borne from a lifetime of involvement and observation of the challenges of being part of a farming family in a rainfed

part of Punjab. It is not an easy life for anyone, but it was unimaginably tough for much of her life. I benefit so much from being allowed to listen to her that to ask to publish as well seems not only greedy, but also immoral. My 'job' as a scholar of that village is incomplete and will remain so. I changed the focus of my research questions to new parts of Pakistan and different households. I continue to go by the village to catch up on gossip, as any good family member should, but I hardly ever take notes anymore and have no intention of publishing the 'data' I may generate. Anything I write about that village family in the future will be historical and carefully curated to ensure that their reputation is not impacted.

Following what I consider my familial adoption, I effectively ceased to 'do research' on my original communities. Instead, I used the conceptual understanding I had gained from those farming communities to understand the wider electoral cultures of Pakistan (Lyon and Mughal 2016; Lyon 2019a; 2019c; Lyon and Hassan 2022). I also shifted my attention to the political and environmental contexts in which my earlier ethnography existed (Lyon 2019d; 2019b; Lyon and Mughal 2019). Finally, I also began writing for non-academic audiences about some of the challenges facing rural communities in Punjab (see for example Lyon 2022).

The hand of Martin

So how does this relate to Martin Sökefeld's work? I argue that it aligns very closely to the humanity he has always brought to the field. He is fortunate enough to have worked with some households that do not appear to have exercised such strict *purdah*, though he has encountered some comparable gender-segregated households in Gilgit as well. Theoretically, one can see this in his careful analyses of the concept, function and implications of 'identity' (Sökefeld 1999; Sökefeld 2001; 2008). At the analytical level, he thoughtfully deconstructs both essentialising and anti-essentialising approaches in anthropology (and, more broadly, in social sciences and philosophy). It seems to me that this is driven, in part, by a genuine understanding of the political implications of such

a concept for marginalised communities, both in Pakistan (Gilgit) and in Germany (Alevi Muslims). As he frames his theoretical arguments, he situates them within political contexts in which Alevi communities in Germany, for example, are subject to discrimination based on an essentialised attribution that renders them vulnerable within both a wider Germany and Turkey as a whole, as well as within what he effectively demonstrates is a fragmented German-Turkish population.

Martin's theoretical positioning is robust and couched in philosophical language that avoids the first-person, anecdotal style that I have adopted here. This is both laudable and necessary to ensure academic credibility, but I believe there can be no doubt that resting at the heart of his analysis is an awareness of the political consequences of reductionist and easy arguments about identity.

Martin is nothing if not eclectic in some of his methodological and empirical approaches. Knowing the sensitivities of the political situation in the northern areas of Pakistan, which remain formally disputed territories with India, regardless of the political realities on the ground, he has delved into the historical record of the region to make sense of the politics of rumours (Sökefeld 2002). Through a fascinating focus on the political rumours evident throughout the northern frontier of British India (Gilgit, but called Yaghestan at the time), he teases out their significance in relation to local politics between the British and local populations, as well as those circulating about World War I. This use of historical material allows him the flexibility to comment on the interconnectedness of global political conflicts that find their way into local political discourse. He, of course, has not shied away from analysing contemporary political conflicts, but the consequences of missing the mark when discussing Gilgit-Baltistan must not be underestimated. He treads carefully because he knows that to do otherwise may compromise the well-being of those he clearly cares about.

Lastly, it would be remiss if I did not highlight one of his more light-hearted contributions to the discipline. Martin has made a name for himself as an anthropologist who takes the ubiquitous truck art of Pakistan seriously (Sökefeld 2000). In this research, Martin examines an art form largely overlooked by 'connoisseurs'. He makes important points

about the aesthetic and techniques that are to be rightly admired, but through his deft weaving of humour he promotes and validates a sector that suffers from low prestige in the wider population (less so now, in part, thanks to him). In short, he uses his analytical credibility to do what the best anthropologists have always done, namely listen carefully to the ignored and the neglected and treat their activities and narratives with as much respect as their more elite and powerful neighbours.

The consequences and politics of compromise

So, what does it matter if people choose to carefully consider the consequences of our writing? On the one hand, it is incumbent upon members of anthropological scholarly associations, all of which place a premium on the well-being and security of the people with whom anthropologists work (for the UK, see *Association of Social Anthropologists of the UK and the Commonwealth (ASA) Ethical Guidelines for Good Research Practice* 2011). Increasingly, research funding institutions similarly demand robust ethical reflection as part of the design process (again, for the UK, see UKRI 2023 for guidance from one of the most important funders of anthropological research). However, neither scholarly associations nor funders quite capture the real driver of compromise for field-working ethnographers who return to the same sites over multiple years. What drives Martin and me, I believe, is a deep and profound respect for the people with whom we work. Martin's decades-long commitment to both his Gilgiti and Alevi friends and research participants is a strength, in that it has obviously given him access to confidential aspects of those communities. It is no doubt also a constraint. There will be actions and narratives that will never make their way on to the pages of his astute and erudite scholarly publications. There are no doubt secrets he will take with him to the grave. For the most part, these are probably relatively banal in the grand scheme of things, but they might embarrass or compromise an individual or a household within their own social worlds.

I can live with the compromises that I have made, and I assume that Martin is similarly at ease with the choices he has made. It strikes me

that the depth of trust and ease in the relationships that emerge from such considerations allows for producing a powerful and effective empirical dataset. While some of that may never be explicitly shared with 'outsiders', it nevertheless serves to strengthen and clarify the anthropologist's understanding of other people's perspectives and experiences. We can never truly walk in the shoes of another, but the process of forming sincere and open relationships ensures we can begin to understand better some salient aspects of *other* people's lives. These relationships introduce a responsibility that goes beyond scholarship. Our scholarly associations instruct us to do no harm, but harm is itself a culturally contextualised concept. So, part of what we must learn in the field is understanding what the people we work with consider harmful. It makes for some messy ethical reviews, but ultimately it has the potential to generate robust explanations and descriptions that are both moral and useful.

Anthropology is littered with the tragedies of anthropologists who have chosen to publish regardless of the potential harm that may arise from revelations. Some of the horror stories may well be blown out of proportion, and some of the most famous, or infamous, cases inspire vigorous defence from both the original ethnographers as well as their allies. Some of these celebrated cases have produced ethnographic material that I have enjoyed and benefited from, but it becomes uncomfortable reading when one realises that the subjects of those studies may have experienced physical, material or social harm as a result. I will not list them here, because I also owe a debt to my own anthropological 'community' that seems to descend into distracting self-sabotage all too frequently. Rather than list problematic ethnographies that will trigger endless arguments, I hope to focus on the humanity that *should* drive our research and our relationships. The price for such humanity, though, may seem high at different points in a career. It's relatively easy for me to step away from my primary field site after more than a decade of research and publishing, but had I felt compelled to take such a decision after the first year, then the cost would clearly have been considerably more painful. Regardless of when and how we decide to navigate these culturally and socially specific ethics, we must always strive for honest and credible accounts of the people we work with – we owe them and our

discipline no less – but to do so with cavalier disregard for the feelings and context of the people we work with is profoundly disappointing (at best).

It is therefore with some pride that I put myself in the same category as Martin Sökefeld, as an ethnographer and an anthropologist who has sought always to consider the people who have been generous enough to welcome me into their communities. *Their* sensibilities and *their* ethics must weigh as deeply and consequently on our scholarly output as those of our home institutions and our broader disciplinary communities.

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Section Three: More-than Representation

Constructing the Field or Cementing It?

On Partnership, Method and the Hardening of Ethnography

Lisa Burger and Tim Burger

When you ask anthropologists how their fieldwork came about and how it turned out to be, you usually get answers along the lines of, “It was all a total coincidence,” or “It was really hard, but also so enriching.” Personally, we were always rather annoyed with this sort of casual mysticism. That is, until we ventured out for a longer period of research ourselves. As it turns out, these are not brush-off answers, they are strikingly accurate. Before embarking on doctoral fieldwork to the Azores, for which Tim had gotten funding, we had been doing smaller projects individually. Under Martin Sökefeld’s supervision, and even before knowing each other, Lisa had conducted fieldwork on djinn conceptions and rationality in Fes, Morocco. As a single mother with a then one year-old daughter, she had gone through the ups and downs of fieldwork: the scary bits and the moments of epiphany that ethnographic research entails (L. Burger 2015). Tim, back then matching the established image of the “lonely anthropologist” (Gottlieb 1995), had carried out fieldwork on legal pluralism and state decentralisation in urban Java, Indonesia (Pöhlmann 2018). We both had suffered a bit and learned a bit. Young, eager and self-confident, we thought we had understood how it works.

This chapter builds on our shared experience of conducting fieldwork as a couple with children on the Azores archipelago, Portugal. Reflecting on our personal stresses, thematic interests and care obligations, we interrogate how ethnographic fieldwork – and thus, ultimately,

anthropological knowledge – is shaped by not doing it on one's own. Much in contrast to our earlier experiences in Morocco and Indonesia, on the Azores our ethnographic multi-sidedness as partners, parents and anthropologists was wound into the research process from the very beginning and turned out to be epistemologically crucial. Reflecting on this fieldwork beyond interviews or participant observation, we place a focus on care commitments and intersubjective selfhood, to take seriously the constructivist approach – emphatically advocated by Martin Sökefeld in his teaching – that 'the field' is not a place but a social context.

This requires a closer look at the prevalent constructivist foundation of fieldwork methodology, namely the idea that 'a field' is not simply *there* but is created in collective processes marked by power asymmetries (Amit 2000; Gupta and Ferguson 1997; Hastrup 2004; Sökefeld 2002). In short, fieldsite are constructed through researchers' relationships. Tracing how constructivist principles emerge and change throughout the ethnographic process, we identify a conceptual problem. If a certain social context, or rather 'the field', truly accommodates the relational mess that brings it into being, it simultaneously appears to be almost impossible to transfer into ethnographic writing. There is a gap between the fluidity of an intimate, contingent fieldwork experience and the 'hardness' and clarity of a mainstream publication. What exactly, we ask, happens between the vulnerability of confused field experiences and the sweeping, confident ethnography making up anthropological discourse? Where *and how* does all the personal go? We argue that while constructivist premises are correct and helpful for understanding 'the field' in the abstract, they are confronted with certain epistemic predicaments, once taken to practice in order to write an anthropological account of this very field. In other words, our goal is to examine the moment in which field-experience crystallises, and hence hardens, into ethnography.

In the remainder of this chapter, we briefly describe the Azorean place where we stayed, as well as the relational situation in which we found ourselves as partners, parents and as ethnographers. Generalising from the dynamics of partnership on a North Atlantic archipelago,

we suggest thinking of any ethnographic endeavour as *fieldwork-with*, emphasising the premise of intersubjectivity during research. We then explore what those insights mean for an overall concept of ‘the field’ and – building on a particularly difficult moment during our fieldwork – why a truly constructivist account of written ethnography might ultimately be impossible.

The troubles of partnership in an Atlantic fieldsite

The last decades have brought some welcome complications to the canonised practice of fieldwork coined by Malinowski (1979 [1922]: 24–49). The most recent examples include the presence of children (Cassell 1987, Braukmann, Haug, Metzmacher and Stolz 2020), gender-based vulnerabilities (Clark and Grant 2015, Johansson 2015, Kloß 2017), the role of research assistants (Middleton and Cons 2014) and the neoliberal transformation of academic institutions (Günel, Varma and Watanabe 2020), among others. In light of such critiques, the Malinowskian vision of a lonely hero setting up his tent in an exotic place increasingly appears like a relic, and yet it strangely persists in academic teaching and in the anthropological imagination. Adding to these complications, we bring into focus one of the most significant, yet simultaneously understudied, relationships within a fieldsite: between a paid ethnographer with an explicit research agenda and an accompanying partner; or, put differently, between partners who are differently positioned in the field. According to some historical examples, there is usually an intimate and enduring relationship between the two partners-in-the-field, frequently both are trained anthropologically and, more often than not, a man will conduct fieldwork and a woman will accompany him (Ariëns and Strijp 1989). Recognising the epistemological and practical implications of this specific relationship, Felix Girke notes that “the question of how partners and families actually live their anthropological lives is still shrouded in the much decried mysticism that for so long has haunted fieldwork and the way it is taught” (2020: 259).

Of course, not every ethnographer embarks on fieldwork with a companion, but once such a situation exists, the methodological and social consequences are significant for the construction of 'the field' and the making of ethnographic knowledge. In our case, the way we had ended up on the Azores, the presence of our two children as well as certain institutional entanglements, such as the school our daughter attended, increased the relational complexity in the field and highlighted the idiosyncrasy of fieldsite formation. Recalling the introductory sentences to this chapter, for us it had all been a total coincidence.

We had arrived rather hurriedly on São Jorge Island. Until a few months prior, we had expected to conduct ethnographic fieldwork in Indonesia. When the Covid-19 pandemic struck in early 2020, Tim was still on a research trip in upland Central Java, which he then had to cut short. The following moratorium on global mobility and the acquisition of research visas prevented this fieldwork from happening. We swiftly replanned towards the Azores, a key reason having been that the Azores remained accessible for lengthy stays and, overall, were not hit particularly hard by Covid. Tim worked through regional literature and Portuguese language classes, and between July 2020 and October 2021, we spent over a year on the archipelago. For the most part, we stayed in a tiny parish, which Tim turned into the spatial basis for his doctoral fieldwork (see T. Burger 2023), Lisa, too, followed up on her ethnographic interests. Throughout the year, the key tension in our partnership would remain the unequal amounts we respectively felt entitled to pursue active fieldwork or felt obliged to perform childcare. On one side stood Tim, a male, remunerated doctoral student with the pressure to bring home sufficient material for his thesis. On the other side stood Lisa, a female, unpaid Master's student getting stuck with the bulk of care labour. Whatever high-held feminist ideals we had premised our relationship on beforehand, we quickly folded into a 'traditional' model of partnership, which led to daily conflicts and disputes.

It did not help that the rural fieldsite we had stumbled into was organised according to patriarchal understandings of care labour, households and everyday work roles. This, at least, was our intuition once we had settled onto the archipelago. A superficial glance might con-

firm the impression of a rural, isolated and deeply conservative society. The Azores are located approximately 1400 km off Portugal's mainland coast, literally in the middle of the Atlantic Ocean. An Autonomous Region receiving EU subsidies to alleviate its 'outermost' geographical condition, the Azores have been coping with administrative neglect and economic marginalisation for centuries. Yet, while visibly 'remote', a closer look reveals a more connected and complex story. Settled from the 15th century onwards, the volcanic archipelago served as a springboard for Lusophone imperial expansion and provided a decisive hinge for facilitating the exploitation of a colonised Atlantic world (Duncan 1972). Moreover, ties to the Americas, Africa and Europe were constantly made anew through a highly established pattern of outmigration (Chapin 1989; Matos and Sousa 2015). Over the last 60 years, migration towards the United States and Canada has translated into a drastic demographic shrinking in the Azores; between 1960 and the mid-1970s alone, the overall population dropped by roughly 30% (Estatísticas dos Açores 2021). To this day, the decline continues, albeit in less dramatic fashion. Depopulation, then, has become a focal problem in current Azorean lived reality, affecting not only agrarian livelihoods, but also gender roles and social institutions like the household. What we had initially taken to be traditionalist patterns of social organisation increasingly turned out to be the intricate and highly disputed results of more recent structural shifts.

In the village, situated on the periphery of São Jorge Island, the formerly 450 inhabitants had dwindled to fewer than 100 permanent residents. Almost every second house was unoccupied, and once profitable horticulture – with its historical importance as a source of subsistence and selfhood – had become increasingly difficult to maintain. Our landlady was a young woman who, furnished with economic and cultural capital, had returned from studying in mainland Portugal to promote tourism on São Jorge Island and skilfully make a living from it (see also T. Burger 2023). We moved into one of the few renovated houses in the village, and while Tim immediately began leaving this place behind to deploy the classic ethnographic toolbox of language immersion, participant observation, interviews and fieldnotes, Lisa felt rather tied to that

same house. At first she was euphoric about the unfamiliar surroundings, confused about the novel social relationships she had to learn how to read and very busy with childcare, but as time passed, she increasingly grew unsure of what she was supposed to do there.

From that point onwards, we were haunted by the image of the well-behaved wife who devotedly boosts her husband's anthropological career. Margaret Mead, always occupied with her own research and publication projects despite being married three times, found pejorative words for such a role when she accused some female ethnographers of accepting "the combined role of secretary and technical assistant, at rates cheaper than such functions command in the market place" (quoted in Ariëns and Strijp 1989: 8). Mead, of course, was writing in a different time, about a different time. Yet a certain imbalance seems evident in the longer run. For instance, few people have read Hildred Geertz's "The Javanese Family" (1961) and yet hardly any contemporary anthropologist can get around the influential works of her famous husband (C. Geertz 1960, 1963, 1973) – and this despite them being 'in the field' together. Was Clifford Geertz simply more brilliant? Not necessarily, since a similar disparity holds true for Edith Turner versus Victor Turner, Esther Goodie versus Jack Goodie and, fortunately less clearly, Laura Bohannan versus Paul Bohannan. More recent exceptions to the rule, such as 'the Comaroffs', seem to confirm a male incline in the distribution of fame. Nevertheless, the point is entirely contradicted by the prominent Marilyn Strathern with her rather unknown ex-husband Andrew Strathern. Everything we know and could find out about these couples stems from remarks in prefaces, informal gossip and filmed interviews with the anthropologist Alan Macfarlane.¹ Girke is thus quite right when he claims that for academic anthropology "partnership might still be an even more sensitive topic than one's own children" (2020: 260).

Intradisciplinary reflection since the 1980s on the social and historical contexts in which ethnographies come about has hardly dealt with

1 See his YouTube channel: <https://www.youtube.com/playlist?list=PL722949E70B77BBFD>

this 'sensitive' topic: "there is no established genre or medium [beyond gossip, one might add] for this specific biographical aspect of anthropological lives" (ibid.: 265). This is surprising because few would doubt the profound influence that the constant intellectual exchange, trust bonds and intimate issues between partners in the field exert on the course of an ethnographic research (see also Pauli 2020: 48). In addition, fieldwork is a deeply interpersonal process with one's interlocutors. Ilva Ariëns and Ruud Strijp, therefore, argue that almost all "anthropological couples" they know of struggled "to find a way of living acceptable in the eyes of the native population and satisfactorily for themselves" (1989: 18). This complex tension seems too difficult for many partnerships to navigate. Ariëns and Strijp, for their part, emphasise the relatively high rate of divorce and separation between fieldwork couples in anthropology (ibid.: 6).

In our case, as fieldwork progressed, Lisa felt increasingly marginalised within the ethnographic project while Tim reproduced a relatively antique idea of both fieldwork and partnership. This trajectory shaped the way we acquired and constructed knowledge on the Azores. To examine this process further, we wish to probe the epistemological consequences of such an unbalanced situation by asking: how is ethnographic knowledge generated when fieldwork is conducted by partners and the various troubles they carry along?

Recognising accompanied fieldwork as fieldwork-with

Approaching this question, a brief pause is in order so that we can explore *who* is actually doing research here and *what* ethnographic knowledge is based on. This engenders both the ethnographer's social identity and the peculiar practice through which her experiences are turned into knowledge, i.e., writing. A commonplace critique has been aimed at the separation of serious ethnographic work (fieldnotes) and its more intimate Other (diary entries) – a separation we were taught in fieldwork classes at both German and British universities (see also Russell 2011: 291–305). The idea is that while fieldnotes presuppose an individ-

ual fieldworker, cleansed of subjective impulses, the diary leaves room for social identity, emotions and personal insecurities. Obviously, this distinction is flawed. Academic credibility, as we think of it, derives from describing the specific conditions of knowledge-making as comprehensively and objectively as possible. In that case, the detailed account of a researcher's social identity, family situation and emotional conundrums should lead to greater credibility, if not 'objectivity'. However, as Myerhoff and Ruby (1982: 26) point out, this basic rule of the 'hard' natural sciences does not seem to apply to anthropology; paradoxically, the contrary seems to be the case. The more accurately an ethnographer describes the particular circumstances underlying their theoretical conclusions, the more subjective and untrustworthy he or she appears. Consequently, the concealing of personal experience in the diary appears somewhat as a credibility strategy originating from a certain academic genre.

From an empirical perspective, this can be countered by the reasonably well-established – yet canonically marginalised – practice of “accompanied fieldwork” (Stolz, Metzmacher, Haug and Braukmann 2020; Cupples and Kindom 2003). Just as Lévi-Strauss did not *really* march through the Amazonian rainforest alone but was accompanied by his wife,² numerous instances of accompanied research appear in retrospect as solo efforts (Cornet and Blumenfeld 2016: 1). Accompanied research, then, has existed for a long time, and this fact has also been long pointed out (Cassell 1987; Butler and Turner 1987; Scheper-Hughes 1987). A productive recognition of the existence of complex intimate relationships ‘in the field’, and the work they require, appears inevitable. This entails not only an ethical and pragmatic perspective, but also aspects of funding, institutional support and, crucially, epistemological consequences (Stolz, Metzmacher, Haug and Braukmann 2020: 14–17).

As an alternative to the well-worn image of the lone hero, we would therefore like to suggest the term *fieldwork-with*, which refers to the entangled, relational and processual constitution of the individual researcher during and beyond the fieldwork process. Emphasising more-

2 Crucially, Lévi-Strauss only mentioned her presence in a three-liner on page 296 (Lévi-Strauss 1978), which means he literally wrote her out of his research.

than-spousal and more-than-parental bonds, it is distinguished from 'accompanied fieldwork', in that it also leaves room for social ties, institutional entanglements or personal burdens that are not present 'in the field' (i.e., as direct company) but nonetheless play a key role in constituting the selfhood of the person doing the fieldwork. This broader notion of fieldwork-*with* valorises the fact that the non-existence of a distinct and stable individual, as propagated by Western European philosophy, is not an ontological anomaly or a problem. Rather, as has often been shown (Handler 1994; Mauss 1985; Sökefeld 1999; Strathern 1988), the isolated individual is the consequence of a particular and gendered history of ideas. Ethnography itself has always been good at destabilising such a unitary construct of the 'Western person' through the inevitable "interpretation of the self in the Other" ("Selbstausslegung im Anderen [i.O.]", Rottenburg 1998: 217). Accordingly, we know selfhood to be dependent on context, situation and dynamic social and material relations. What works on the theoretical level as an anthropological critique of taken-for-granted ontological assumptions of 'our' society can then also be applied to empirical research, as Flinn argues: "The solitary ethnographer model suits the Western notion of the person, yet many of the peoples anthropologists work with have 'sociocentric' views of the person, and they interpret fieldworkers accordingly" (1998: 10). The fieldwork-*with* model is thus closer to theories of personhood in non-Western-European places, which often understand people as only "human" through their family relationships (Engelke 2007: 165). In short, we argue for a methodological concept of the ethnographic self as an intersubjective process.

Much in the same way that almost all acknowledgement sections in published ethnographies highlight one's interlocutors 'without whom' the present work could never have come into being (see Ben-Ari 1987), we cannot but understand the practice of fieldwork itself as even more clearly co-constitutive, i.e., not just "I worked with these interesting people" but rather "this author, in her intimate relational constitution, worked with these interesting people (changing both herself and them)." What exactly forms the intimate relational constitution varies from case to case. Next to the obvious and formative fact that fieldwork always

occurs *with* one's interlocutors, there is also fieldwork *with* children, fieldwork *with* partners, fieldwork *with* one's supervisor, fieldwork *with* an interpreter, fieldwork *with* doctoral students, fieldwork *with* a fast Wi-Fi connection enabling the virtual presence of family and friends, fieldwork *with* research assistants, fieldwork *with* a ton of work over the term break, fieldwork *with* funding organisations breathing down one's neck, fieldwork *with* friends and fieldwork 'at home' *with* contact with the parental house around the corner. The list could go on. How can we assume that all of these factors, relationships and problems play no role in the construction of the field – and thus, ultimately, in the form of ethnographic knowledge that 'the field' produces?

What is a field?

What does all of this mean for ethnographic research in the field? So far, our goal has been to highlight and conceptualise a profound, yet often ignored, dimension of the social construction of a fieldsite, namely the role of accompanying partners. In this section, we address the concept of 'the field' itself as well as the consequences of an intimate partnership 'in the field' for the formation of anthropological knowledge. Contrary to earlier notions of fieldsites as stable places where anthropologists would go to 'collect data', years of reflexive engagement with methodology have led to a fundamental insight: an ethnographic 'field' does not exist a priori but is created and constantly reshaped by the specific relationships, choices and experiences of a researcher over time. Recognising the constructed nature of the field has been a breakthrough in the history of the discipline (Amit 2000).

Akhil Gupta and James Ferguson (1997) made a crucial contribution to stressing this constructedness of ethnographic fieldwork. They noted that after the concept of culture (for example, Abu-Lughod 1991, Wagner 1975), as well as the genre of ethnography as a means of authority and reasoning (Clifford and Marcus 1986), had been challenged, the idea of 'the field', an equally basic component of anthropology, was still allocated a powerful role in the discipline beyond critical reflection (Gupta

and Ferguson 1997: 2). Fieldwork, the taken-for-granted practice of anthropology, continued to be influential precisely because of its apparent self-evidence.

James Clifford (1997: 194), therefore, described fieldwork as “disciplining” in a two-fold sense. On the one hand, it is such an idiosyncratic part of anthropological activity that it establishes the discipline of anthropology *per se*, *vis-à-vis*, say, linguistics or philosophy. On the other hand, the paradigm of fieldwork carries a disciplining effect on young ethnographers by setting limits for what counts as anthropology and what does not. Nothing about an ethnographic field is actually there beforehand: “[The field] must be worked, turned into a discrete social space” (Clifford 1997: 186). Gupta and Ferguson pointed out that because of this critical and ‘fetishised’ meaning of ‘the field’ for anthropologists, the world and the ethnographic theory thereof had changed drastically, but the method of fieldwork and the idea of the field had not: “What are we to do with a discipline that so loudly rejects received ideas of ‘the local’, even while insisting ever more firmly on a method that takes it for granted?” (1997: 4). They countered the problematic concept of ‘local’ – problematic not least because of the colonial undertones reverberating in the required spatial distance of the ‘field’ from one’s own ‘home’ (i.e., Western Europe and the USA) – with the concept of ‘location’. More precisely, they called for “an attentiveness to social, cultural, and political *location* and a willingness to work self-consciously at shifting or realigning our own location while building epistemological and political links with other locations” (ibid.: 5, italics in original). What stands out here is the processual nature of “location-work” (ibid.), which allows for constant adjustments, shifts in perspective and recognition of situated knowledge in order to absorb and reinterpret the real circumstances that make up our contemporary world (ibid.: 39–40). Fieldwork, then, continues to occur in concrete places, since interactions are always spatially grounded (Bräuchler and Naucke 2017: 426; see also Escobar 2001: 140), yet ‘the field’ is best thought of as a fragile social context through which interacting and situated individuals navigate.

Our lived reality, which we perceived and with which we engaged on a daily basis, was primarily structured by the paradigm of fieldwork. We

lived less in a village on São Jorge Island than in an ethnographic field-site that emerged *with* and *through* us. The same place would be entirely different if we were to experience it from a tourist's point of view, for example. Our family relationships and 'the field' therefore mutually constituted each other, or, as Girke puts it for a fieldsite shared with his partner and child, "the choices we made [constructed] not only our fields but also our life in the field" (2020: 275). 'The field' was consciously created as a knowledge-generating construct by Tim – as an active, remunerated ethnographer – and yet simultaneously co-constituted by Lisa and her actions, thoughts and comments.

This intersubjective formation of 'the field' is not limited to an anthropologist and an accompanying partner or person. The same applies to research overall. Both fieldworkers and their interlocutors are agents in a shared political, historical and social context. It would be mistaken to assume that only fieldworkers investigate how 'others' lead their lives or that only those others act while fieldworkers observe. Both are actors in a shared field (Sökefeld 2002: 91, Middleton and Pradhan 2014) and ultimately collaborate, a process that has even be called "teamwork" (Gottlieb 1995: 22). Recategorising the making of ethnographic knowledge as a collaborative endeavour has consequences for what we perceive as the basis of ethnography, namely who is doing the research, a *me* or a *we*? When we pointed to the multiple, relational constitution of the self in order to develop a broad notion of fieldwork-*with*, we were also concerned with the pragmatic consequences for field construction and knowledge creation. As Funk puts it:

While, nowadays, critical self-screenings have a solid space within most ethnographical accounts, they nevertheless tend to focus on the researcher's own person (or self), but omit important others like partners, lovers, and children, with whom the researcher might have intense affective bonds (Funk 2020: 186).

We suggest going one step further than Funk, who holds up the individual person (or a bounded self) in this way. Instead, we propose that the condition of anthropological knowledge – that is, the collective and

chaotic process of ethnographic research (which, in turn, is itself contingent on the epistemic paradigm of the relational self) – should be understood less as a copyright problem or lip service paid in acknowledgements but more as a positive commitment to cooperation, collaboration and co-constitution of knowledge. A multitude of persons creates field-knowledge. What happens to authorship in the mills of the publication landscape thereafter is beyond the scope of this chapter's argument (but see Gupta 2014).

In how far does the recognition of this collective makeup of fieldwork affect ethnographic knowledge? Not much changes, Kristen Hastrup would probably suggest, insisting on a phenomenological approach of post-positivism. She shows that there is no objective world that could be known by an individual but rather that it is ethnographers who always-already co-create an object as a result of their mere attention: “[O]ur relation to the object is already installed as part of the object when we begin to understand it” (2004: 468; see also Pöhlmann and Sökefeld 2021: 10–11).

The shared experience of interpersonal fieldwork is thus the source of ethnographic knowledge, not the practices, rituals, narratives or disputes of the Other per se. By sharing a frame of reference with her interlocutors – their point of view or location, so to speak – an ethnographer can understand something ‘real’ about the common world. Hastrup describes in an anecdote how an initially strange experience enabled her to understand the world of her interlocutors better. For months, she had asked about elves or other beings, and for months she had received negative answers: in the past, people had believed in them, but that was the past. She only adapted her approach when, in an irritating situation, she herself had the feeling of seeing beings that fitted the idea of elves. Hastrup then no longer asked, “Do you think elves exist?” but “When was the last time you saw any?” By no longer questioning but sharing the basic assumptions of her interlocutors’ reality (and starting a conversation about details of that reality), she established a “true relationship” (Hastrup 2004: 357) characterised by taking place in a joint frame of reference. And yet, as Hastrup insists, and as we shall elaborate below, the moment she writes these insights down, and thereby analyses them *ethno-graph-*

ically, she once again exits this shared frame of reference. Still more, she objectifies it (ibid: 458).

Now, it is an established narrative strategy in ethnographies to describe an initially frustrating, disturbing or confusing experience and then triumphantly report how one overcame it and how 'the field' subsequently opened up. In the following, we counter such success stories by describing an incident that pushed us to the limit of our personal capacities and undercut our idea of a workable, co-constructed fieldsite. What might retrospectively sound like a minor event made us doubt the existence of a shared frame of reference at all. Ultimately, far from being a 'success', the event had modestly productive intellectual outcomes for Tim; Lisa, on the other hand, as a mother and partner, was about to leave the field.

Cementing the field

All this did not happen without antecedents. For eight months we had stayed in the village and, by then, were relatively exhausted. For several days, our landlady had been renting the basement of our house to a family from the other side of the island who were distantly related to her and relatively well-known in the village. There had been some issues with their family dog in the shared garden and ongoing ambiguities about our rent with our landlady. After days of trying to coordinate our children's rabbits with the dog, it went awry one evening. The unleashed dog jumped inside the rabbits' fence, killing one and forcing the other to jump over a wall. Lisa had just been on her way to return the rabbits from their enclosure into their cage while Tim and the kids were eating dinner. Screaming, Lisa tried to drag the dog out of the enclosure while Diogo, the dog owner, helped with what seemed like a guilty conscience. It was a disaster: our daughter crying at the window, one blood-drenched rabbit in the garden, the other fleeing in panic, an adrenalised dog and Portuguese curses all over.

The main problem, however, was that the event did not remain limited to a dead rabbit but escalated socially. After the dog had been

leashed again, Diogo claimed Lisa had attacked him with her fists, thrown shoes at him and pushed him off the wall. He stated that he would call the police and have us deported from the island. Our landlady, rushing over, bought into Diogo's story. We were shocked by what we felt was brazen lying and Diogo's portrayal of Lisa as a hysterical, aggressive woman. While we retreated into our house and left the village early the next morning in order to distract ourselves, Diogo had different plans. As we later realised, he had spent the morning laying out his version of events about Lisa, sometimes furiously quarrelling and boxing, sometimes throwing tools, always worth a laugh. When we returned in the afternoon, we had lost a fight over public opinion that we did not even understand was going on. Diogo skilfully made Lisa realise her powerlessness against him, an established man whom everyone would believe.

Our argument is that right here, in this emotional chaos, we can observe the genesis of 'the field' itself. To what extent our anger was justified or simply the fragile response of a privileged couple faced with an everyday conflictual situation is not primarily relevant to our methodological argument. As a family, we *were* distressed and overwhelmed by this situation, but Tim was less so. He wrote it all down, objectified it, analysed it, put it into contexts of kinship obligations, idioms of masculinity and structural economic pressures. His disciplined (and, in Lisa's opinion, heartless) distancing of the incidents changed those very incidents for him. While Lisa was going through a social crisis, Tim eventually ended up working out the ethnographic description of a social crisis. The two things have little in common with each other. Playing on the double-meaning of the German word *geteilt*, our shared field was suddenly divided (cf. Girke 2020: 263).

We suggest that the incident with the dog was the moment when Tim's intentions to take constructivism to heart – and therefore to try to recognise the situational (and familial) contexts in the production of any empirical insight – turned into a more old-fashioned epistemological assumption. He felt it would have been too much to truly include the shock and anger of himself, his wife and daughter in the overall ethnographic description, as it would have overextended and perhaps

collapsed his fieldsite, which suddenly appeared *all-too* relationally constituted. He split 'the field' from the rest of the family and thereby reified it. His theory of the field, based on which he would continue to work over the following months, was now more localised, more individualised and more stable. It suddenly seemed closer to Malinowski than to Gupta and Ferguson. The fieldsite now had deliberate boundaries, kept emotional trouble out and was directed towards specific goals in order to ethnographically utilise the remaining time of the research period. The relational fieldwork-*with* was transformed into an ethnographically controllable 'field', and for his resulting doctoral thesis, this worked out.

Put in different words, Tim took the 'Hastrup route' by using an unsettling experience as an opportunity to reflect on his research strategy and to think anew about the social relations that surrounded him. In Hastrup's case, however, this approach led to the formation of a common frame of reference with her interlocutors, whereas Tim became acutely aware of the divisions, conflicts and the mistrust between him and certain village residents. This resulted in a move away from Hastrup's phenomenological constructivism. There is no doubt that epistemologically and ethically an understanding of the relational and context-bound constitution of 'the field' is the correct way to go. An ethnographic field is characterised by the specific social processes, situational choices and sociocultural imprints that all participants carry with them: it is this messy mixture from which knowledge emerges. And yet, such a theory, now established in anthropology, does not sufficiently explain how, despite this, the vast majority of ethnographies are still published by individual authors claiming some sense of empirical authority over their subject. Nor can it explain how personal relationships hardly ever take centre stage outside of the acknowledgement-section. In other words, most publications of ethnographic knowledge still seem to be based on the fact that at some point in the research process, a fieldsite in the old-fashioned sense has emerged and congealed.

We base this argument on Pierre Bourdieu's (1990 [1980]) contention that without making an object, scholarship does not take place: "If it is to be more than the projection of personal feelings, social science necessarily presupposes the stage of objectification" (*ibid*: 11). Furthermore, and

crucially, Bourdieu sees a written description as an “instrument of objectification,” which is also what we emphasised about Hastrup’s argument above, i.e., even the most jointly created frame of reference is altered and objectified by systematically writing about it (2004: 458). Bourdieu does not automatically approve of the resulting rupture and distance, pointing out that it is not a matter of sweeping away “the distance magically through spurious primitivist participation, but to *objectify* the *objectifying distance*” (1990 [1980]: 14, our emphasis). The rift between anthropologists and ‘Others’ does not consist in a primordial cultural difference but in the respective “relations to the world, one theoretical, the other practical” (ibid). Our point here is that an objectification or reification of a different world inevitably occurs in the ethnographic research process, and we need to understand *how exactly* this process of objectification plays out in regard to the persistent figure of ‘the field’.

What in Tim’s case may be a particularly well-defined moment in time (the conflict with our neighbours and the resulting family crisis) may be related for other researchers to entirely different events, to previously held convictions or to particular routines. In all cases, between ‘arrival in the field’ and ‘publication’, a transformation occurs from a consciously constructed *field* (the processual fieldwork-*with*) to a traditional **field** (the local, Malinowskian fieldsite). A relational, situational and spatially grounded context (*field*) becomes a “cleared place of work” (Clifford 1997: 186) or, as we refer to it here, a **field**. The inevitability of this process is due to the fact that the **field** remains comfortably implicit while meeting scholarly demands (writing, theorising, publishing, telling anecdotes, etc.), but the *field* does not do so. What works well during the first months in, say, a village does not work well during a crisis, and certainly not in journal articles or at conferences – except for events explicitly dealing with methodology and ethnographic theory. In this crucial transformation from *field* to **field**, private notes, intersubjective euphoria and dislike, as well as intimate family ties, gradually disappear. It is right in this process that a researching individual is created and the everyday chaos of the *field* is ordered and othered.

Conclusion

Is the specific constructivist theory of the *field*, then, a phase-out model? Only if it continues to articulate itself in merely two ways: either as a wholly theoretical and often reprimanding contribution to the epistemological conditions of anthropological methodology (e.g., Gupta and Ferguson 1997; Hastrup 2004) or if applied in a cheerful way that aims to demonstrate how much more productive fieldwork-*with* ultimately is (e.g., Pauli 2020; Halme-Tuomisaari 2017). Little seems to be gained when, outside of these two genres, the interaction with interlocutors (however intimate, wary, hateful or familiar), as well as the complex circumstances of the actual research (fieldwork-*with*), remain outsourced in private diary entries and acknowledgements. The truth, namely that there are no or only “partial” (Clifford 1986) truths outside of the power-imbued processes of intersubjective meaning-making, remains limited to routinised self-screenings in teaching and publications. Anthropologists mention something about reflexivity and then move on to an ethnography implicitly based on a **field**. At the same time, the more radical option of including deeply subjective insights in scholarly texts remains at threat of tipping over into pure navel-gazing, with little to no ethnographic insight.

A middle ground is not apparent, and perhaps it is not supposed to be there: the elementary dialectic of fieldwork oscillating between proximity and distance is simply unresolvable; or rather, its persistent tension is so fundamental to the ethnographic process that its resolution would be the end of the anthropological method itself.

This means, conversely, that the miraculous disappearance of private confessions on the way from fieldnotes to written ethnography seems unavoidable for current anthropology. As long as the academic landscape is primarily composed of monographs, journal articles and lectures, and also continues to reproduce Malinowskian paradigms in teaching, there is little room for multi-sided fieldworkers who collectively and relationally seek knowledge. Little seems to have changed since Gottlieb noted just over 30 years ago that we anthropologists had “an oddly isolationist view of ourselves” (1995: 21).

Let us sum up. This chapter has demonstrated that more people than just the official ethnographer (Tim) and Others (residents of São Jorge) were involved in the construction of 'the field'. Lisa's epistemic agency or our children's social presence played into various research stages. The first part of the chapter offered a stance moving towards a constructivist theory of fieldwork, illustrating in how far 'a field' is a chaotic, relational, collective process, not a stable 'site'. The second part, subsequently, traced the conversion of this fieldwork or field-experience into ethnography, i.e., a written account of a group of people. It was a move from everyday messiness and personal vulnerability to distance and stability. We argued that most published ethnography has undergone this shift, which in turn means that constructivist methodology is limited to a certain extent.

Our argument has centred on the observation that as anthropologists we not only construct but also tend to cement our 'fields', turning them from socio-spatial contexts into individual sites in the process of writing ethnography. We have shown how a relational *field* was transformed into an ethnographically controllable **field** through one specific practice that Lisa did not share, that only one person in the village seemed to engage in systematically and obsessively: writing. Tim wrote and distanced himself from the world, working through social crises and everyday encounters. And while Lisa got stuck in a social conundrum, ruminating over all sorts of encounters, Tim was writing. Put differently, he was writing up the **field** and himself, as an inquiring individual vis-à-vis a relational, collective self. Both of these elements, among others, played a crucial role in the ethnography of 'the' villagers of São Jorge Island in the form of his doctoral thesis. And yet, the whole problem of ethnographic representation – the shifting, the distortion, the detachment, the hardening and cementing – ultimately began with the first fieldnote and its instrumental potential to objectify a shared world.

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Home/Transit

Martin Saxer

Introduction

Preparing the final manuscript of my doctoral dissertation, I spent a couple of days going through the archive of images I took during my fieldwork in Tibet, looking for the right ones to accompany the text. I realised that there was a considerable discrepancy in themes and outlooks between my writings and my photographs. The images, especially the ones I liked, captured a set of situations different to my field notes, on which most of my dissertation was based. The stories my images told seemed a bit random a first sight – a fleeting observation here, ambient light that caught my eye there. At the same time, the archive of images was also more comprehensive, more encompassing than my field notes. Systematically going through this archive, a year after I had left Tibet, I saw a plethora of stories, encounters and observations that felt utterly relevant to the world I was trying to describe. This archive of images showed the many sides of my time in Lhasa and Xining. While most of them did not directly pertain to the topic of my research – the creation of a pharmaceutical industry for Tibetan medicine – the experiences from which they emerged had been crucial for understanding my surroundings. Yet, these stories, encounters, observations and experiences did not find room in the book I was finishing.

I felt I had developed some kind of academic tunnel vision, and I did not like it. What to do with the multitude of sides that ethnographic research entails? What to do with the bits and pieces of ethnography that do not fit with the academic narratives we seek to distil?

Once the dissertation was submitted, the book under contract and a postdoc grant secured, I went back to these images and started a visual ethnography blog. I called it *The other image*. I began working on a series of posts on my time in Lhasa. They were a pleasure to write. Unencumbered by questions of academic relevance, and also a little less focused on the omnipresent politics surrounding Tibet, I found myself going back to that precious joy of noticing, which had always drawn me to ethnography.

A radical thought crossed my mind. What if I would give such random visual asides more room and move them to the centre? What if I would base my writing less on discourses, opinions or the search for meaning and more on sets of images that often stubbornly resist being co-opted into elegant concepts and grand theory? Could this help me find a style of writing that would keep things more grounded? Could this, perhaps, become my own approach to ethnography and the discipline of anthropology? Such an approach would be quite different from the strategy I had so far pursued.

My strategy, which I had learned at university, had roughly been this: Be open at the beginning and know that things may not be as they appear at the outset. Be patient and let more layers and aspects come into view. But don't get carried away with complexity. Settle on an angle or a framework, ideally one that has academic merit and ties in with current debates.

This strategy is neither wrong nor bad. However, it comes at a price. The experiences and observations that do not really fit with a chosen angle or framework will fade into the background – often simply because one doesn't have the eyes yet to see how saturated they are with the world from which they emerge.

I felt that *The other image* could help in this regard. The short pieces I compiled for my visual ethnography site were more processed than raw field notes but less ephemeral than social media posts. They were personal – private, even – and yet not focused on me but rather looking outward. I saw *The other image* as an intermediary step between field notes and actual pieces of work. Casual enough not to feel like serious writing, the posts had the quality of bookmarking seemingly random observa-

tions and keeping them in view. They felt like shells and bits of flotsam that form tidemarks on the beaches of memory. Writing a post meant collecting them and threading them up to a simple necklace.

The other image accompanied me through the years, and I got hooked on this short form of visual writing unbound by purpose. Nevertheless – or, perhaps, for this very reason – the blog became the backbone of most of my written academic work.

However, I also found myself writing posts for *the other image* outside any field work or research context but rather following observations in my daily life wherever I was. Right after I came back from Tibet, my daughter was born and the periods of ‘proper’ fieldwork became shorter. Around the same time, we moved to Singapore and my ethnographic curiosity became attracted to my new surroundings. We travelled extensively as a family, seeking to combine these journeys with research. In between, we spent time with my daughter’s grandparents in Switzerland and Russia. As family life and work blended and we kept moving from place to place, the concepts of ‘field,’ ‘home’ and ‘travel’ became less and less distinct. I often felt more like a stranger in Singapore than in the Himalayas, and I started looking at the village in Switzerland where I grew up through the eyes of an outsider, whilst the spacious suburbs of Saint Petersburg felt almost more like home whenever we visited.

The other image was no longer just a vessel for asides during fieldwork but rather an attempt to keep developing my ethnographic eye with the help of a lens regardless of where I was. I started gathering the posts that did not fit within the larger research themes under the label “Home/Transit.”

What follows is a lightly edited selection of these posts, roughly ordered chronologically. The selection has no claim to coherence or have a purpose, nor does it seek to answer a larger question. There is no obvious conclusion or an afterword. The posts filed under “Home/Transit,” however, did eventually come to an end. We moved from Singapore to Munich, and once my daughter started school, journeys became shorter and the city a more permanent home. The curiosity that comes with gaining, losing and re-gaining familiarity with a place slowly fizzled out. As the boundaries between ‘field,’ ‘home’ and ‘travel’ became clearer again,

my visual interest shifted to other ventures. What remains is a fragmentary archive of an anthropologist's attempt at integrating visual and academic work and making himself feel at home in the world.

Ghosts and other mysteries

12 May 2012. Hungry spirits, rusty oil barrels and awkward disks on top of our housing block in Singapore.



When we arrived in Singapore, we soon found a flat in a public housing block managed by Singapore's Housing Development Board, known as "HDB." More than 80% of Singaporeans live in such subsidised HDB developments, all of which have names. Ours is called *My Buona Vista*.

When we moved in, there were two things I could not make sense of: the rusty oil barrels at every entrance and the giant, awkward disks on top of every building.

The purpose of the barrels quickly revealed itself. They are used to burn offerings of incense and paper money during Chinese festivals –

especially in the seventh lunar month known as “Ghost Month,” during which time the gates of Heaven and Hell are open and the deceased are free to roam on Earth. The offerings appease their hungry spirits.

The purpose of the giant disks that crowned my house, however, remains a mystery to this day.

The illuminated disks and the otherwise functional austerity of HDB architecture, the lived spirituality of my Chinese neighbours chanting in the morning and burning offerings, the omnipresent sports facilities for adults and playgrounds for children – these are my daily vistas of *My Buona Vista*.

HDB architecture, spirituality and recreational facilities probably have little to do with each other. Yet, I cannot help seeing all of these things together; they make the little world I currently call “home.”

Footpath to Biopolis

15 August 2012. Across the jungle between our housing block and Singapore’s incorporated bio-future.

A makeshift path across a defunct railway line links two worlds as different as can be. On one side there is the residential world of government-subsidised housing blocks with playgrounds for children, covered sitting areas for the elderly, small shops, busy food courts as well as ghosts and other mysteries.

On the other side, there is Biopolis, Singapore’s incorporated vision of what a global centre for life sciences and cutting-edge biomedical research should look like. Constructed between 2003 and 2006, Biopolis consists of nine large buildings with names like Nanos, Genome, Matrix or Immunos – linked by illuminated passages with each other.

The old railway was shut down in June 2011. The rails were dismantled and people started using the narrow corridor as a shortcut between Biopolis and the residential housing blocks north and east of it. Several footpaths across this no-man’s land emerged.



These images were taken on a Sunday evening. Biopolis feels eerily empty and silent on weekends. Traffic lights give way to absent pedestrians and imagined cars at the intersection of Biomedical Grove and Biopolis Drive. The distant sound of ventilation systems magnifies the silence. Maintenance staff exit Matrix and head home across the abandoned railway lines.

The caravansary

7 February 2013. Little has changed at Khaosan Road, Bangkok.

Those who have travelled in Southeast Asia will know Khaosan Road in Bangkok, the quintessential backpacker ghetto packed with hotels, travel agencies, street vendors, 24-hour bars and people from around the globe.



While Bangkok has changed tremendously since I first visited in the late 1990s, the area around Khaosan Road has in many ways remained the same. There may be more travellers in their 40s and 50s now, more children, more café latte, Wi-Fi and foot massages. The hawkers selling wooden frogs that sound like frogs, the roasted scorpions for the brave, the hippie clothes, fried noodles and cut pineapples, the music, beer and cocktails, however, have remained exactly as they were.

Passing through Bangkok, I sometimes avoided Khaosan Road, sometimes I found it a convenient place to get things done. And sometimes, especially when arriving late at night, I enjoyed the charm of its hustle.

Khaosan, I think, is the contemporary equivalent of a caravansary: A stable realm of perpetual transit, full of stories and things and promises for a meandering mind. Within 24 hours, you can get a fake driving licence or a university diploma, a henna tattoo or a tailor-made suit, a ticket to the beach, a wedding dress or whatever you may need for the next leg of your journey.

Pandora City

22 October 2012. Visions of a more potent, more stunning and less messy version of Singapore's tropical nature.

A few months ago, Singapore's *Gardens by the Bay* opened to the public, a large park on reclaimed land at the island's southern tip. Advertised with much fanfare as an important step towards the transformation of Singapore from a "Garden City" to a "City in a Garden," the park features cooled conservatories and a grove of "supertrees" – between 25- and 50-metre-high steel structures that will eventually be overgrown and form an artificial canopy of flowers and climbers. The canopy is envisioned to regulate temperature and provide shade, whilst photovoltaic systems mimic photosynthesis, and rainwater is harvested for irrigation.





We buy an 18-dollar coupon for an aperitif in the treetop bar of one of the supertrees. An elevator lifts us up into the canopy. Artificial flowers abound and the golden face of a Buddha looks down on us, embowered in artificial vines. Even the flickering tealights on the table are LED-powered, much to the delight of my little daughter, who touches them carefully, stunned by the magic trick. A large flatscreen shows an eternal parade of young models walking down a runway. The glamour of Fashion TV blends naturally with the surreal treetop shelter and the view of the city's illuminated financial district.

Singapore, just north of the equator, has developed a special relationship with nature. Constant gardening is required to prevent the tropical forest from recapturing the city. At the same time, the city state has invested much in recreational facilities to make the tamed nature accessible – a bird park, a large zoo and several treetop walkways allow for its consumption. *Gardens by the Bay*, however, takes the relation between humans and nature to a new level. The project manifests a vision of a more potent, more stunning and less messy incarnation of Singapore's tropical surroundings. An avatar of nature itself. Welcome to Pandora City.

Avatara was originally a Sanskrit term that stands for the voluntary descent of a deity to the world with all its illusions – its *maya*. But for us contemporary denizens of planet earth, the term “avatar” rather stands for the little images representing our identities in cyberspace, or maybe James Cameron’s Hollywood version of an avatar descending to Planet Pandora to save the native Na’vi. The gaze of Singapore’s skyline from the supertree’s platform, however, feels absurdly real. The canopy bar provides a glimpse into the 21st century’s hyperreal *maya* – the lungs of global capitalism.

And the natives, where are the natives? – That’s us.

Hundwil

12 May 2013. An explosion of green and repeating images of home.





An explosion of green meadows dotted with houses, barns and cows, the finest milk, an oversized church and the occasional human being. This is Hundwil, the place where I grew up. These images are all taken just around the house. Here, twenty years ago, I was sitting on the doorstep, smoking cigarettes and dreaming of the world out there.

Once I moved out, my mother started sending me self-made calendars with seasonal variations of these images – the church in winter, the meadows in spring, the old pear tree in autumn, the barns and cows in summer. She asked me every year whether I really wanted yet another one. I did, and I carried them with me to India, Nepal, Siberia, China and Tibet.

Whenever somebody was curious about the place I came from, I would show these pictures. I would explain that a good Swiss Brown would produce more than 100,000 kg of milk during her lifetime, that such cows would earn badges that peasants proudly displayed, that there were beauty pageants to celebrate their udders.

While I was living there, I was blind to most of these things. It was not my world; my father was a priest, not a dairy farmer. But from afar, it seemed obvious that the grass, the cows, the barns and the milk were what made this place – its landscape, its culture and its people.

My mother continued photographing the house and its surroundings until her old Zeiss Ikon Contaflex finally stopped working a couple of years ago. Whenever the light was good, she would take a picture: to remember that this was a beautiful place, to see it despite its omnipresence, to transcend its immanence – one roll of film after another.

Now, I find myself doing the same.

Moon over Munich

25 October 2013. Stuck between city and suburbia.

Donnersberger Brücke, 7 pm. The moon is rising behind the Munich City Tower. People in the expensive fitness studio across the railway tracks are being watched by those waiting for their train back to the suburbs.

A poster campaign by a local insurance company shows a young couple kissing happily. They have a thousand plans and no reason to worry because of their flexible pension plan.

My train is late. Stuck between city and suburbia on an early October evening that smells of snow, I am captivated by the colours of this uneventful moment. The dark blue sky, the yellow danger signs, the red of Deutsche Bahn. Random, fragile beauty in a city where I am still a stranger.



Fairytale

11 June 2014. As real as it gets.

“Look, a real princess!” my daughter whispers in my ear.

There she stands on a red and yellow cliff overlooking Issyk Kul, the deep blue lake between the two ranges of the Tien Shan in Kyrgyzstan. National Television is shooting a music video with a well-known singer.

They set up a camera crane and are doing rehearsals while waiting for the harsh light of the early afternoon to soften.



The sandstone formations that provide the setting for the scene are called *skaska* – fairytale. Just off the main road that leads along the southern shores of Issyk Kul, they are easy to reach and attract many visitors. We mingle with a group of Kazakh tourists and watch the scene unfold.

The slow pace on the film set, the extensive preparations, the boredom and waiting under the scorching sun are interrupted only by the short moments of focus and ritual when the camera runs. We, the bystanders, acknowledged but ignored, bestow our interest and curiosity upon the spectacle. All this suits the surreal landscape exceptionally well. A princess in a fairytale, indeed – as real as it gets.

Good location scouting, one could say. Or is it more the other way around? That the place lures people to worship it and praise its name around the globe?

Chläus

07 January 2014. New Year's Eve in Hundwil, Switzerland.

In Hundwil, the village where I grew up, a ritual is performed on New Year's Eve. Masked characters known as "Chläus" go round from house to house. They are dressed in elaborate costumes with hats that feature entire landscapes or stuffed birds, they sing songs without words and they dance, shaking the heavy bells they are carrying. If you ask anybody in the village what they are doing and why, you will learn they are chasing away bad spirits. Few believe in spirits these days; ghosts and spirits have disappeared from the lives of most people in Hundwil. But this does not matter. Ritual, I think, is much less about meaning than we may assume. It is first and foremost about being. Going round from house to house as Chläus is just beautiful, exciting and simply feels right. The Chläus are given mulled wine and some money in return.

Some of my former classmates arrive at the house. I do not recognise them.

“Happy New Year!”
“Who are you?”
“Hansueli.”
“Ah, Happy New Year!”
Happy New Year.



Caspian crossing

21 November 2014. Steaks from Paraguay for the Kazakh middle class.

We are on the way to Central Asia, crossing the Caspian Sea from Baku (Azerbaijan) to Aktau (Kazakhstan) on a cargo vessel. Aboard we meet a group of four Azeris working on a refrigerated goods train. Their cargo is frozen meat from Paraguay and their job is to handle customs procedures and deliver the goods safely to northern Kazakhstan. The nitty-gritty of globalised trade.



We spend a day anchored off the Kazakh coast. Two other ships are waiting to discharge, and the sailors say a storm is coming. Much time and nothing to do except watching the weather and hoping that we are allowed to dock before the winds pick up.

The Azeris invite us to their cabin and feed us fresh pomegranates, snacks and vodka.

“Isn’t there enough meat in Kazakhstan?” I ask.

There is plenty, “... but they eat a lot,” they reply. “And the growing middle class in the oil-rich country has developed a taste for fine steaks and tenderloin,” they add.

The four men are in their 50s and led different lives before becoming train attendants. One ran his own shop in Moscow, which paid for his two children’s university education. It was raided and seized, like so many other immigrant businesses. The other one was the director of a fish factory and was responsible for the protection of fish in the Caspian Sea during Soviet times. Now they earn about 240 US dollars a month. Not enough for today’s expensive Azerbaijan. Soviet times were better, fairer and more affluent, they all agree.

Steppe motel

4 April 2015. Tea and dinner in the Kazakh steppe.



Places to stay are few and far in between along the highways that cut through the Kazakh steppe. Weary after a long day of navigating potholes and driving desert tracks, we are happy to find this one near a fuel station outside Sagyz. The house is charmingly illuminated as if it were a film set. The signboard reads “Resting rooms,” rather than hotel – maybe out of modesty and maybe just to temper expectations.

Those who travel these lonesome roads usually have urgent business elsewhere. Some work on the oil fields near the Caspian Sea, others are migrant labourers on their way to Russia. Broken axles, loose sands, sudden storms or other troubles are frequent; everybody agrees that the steppe can be a dangerous place. People drive long hours to arrive somewhere safe.

Our room is tiny and the beds are so narrow that I fall out twice during the night. But the canteen is warm and the owner serves us tea and

dinner. The motel soon fills up, and those arriving late are sent away – the last ones at 3 a.m. They get back into their car and drive on till morning.

Prospekt Nastavnikov

28 December 2018. Winter light in Saint Petersburg.



There is something particular about Saint Petersburg during winter. With just a few hours of natural daylight, much of the city days unfold in darkness. Yet, around the clock, there is a glow to the city that I always found intriguing.

While we are walking to a restaurant with friends, it starts snowing heavily. I finally begin to understand: reflected by the snow on the ground, the light of street lanterns, billboards and cars illuminates the city from below. The ground is light and the sky is dark, inverting the natural order of things. Snowfall amplifies this inversion. This is what a stroll along Prospekt Nastavnikov, the Avenue of Mentors, teaches me tonight.



Cockpits

10 August 2018. Breeding roosters in the hull of a Boeing 747.

In Thantip Village, eastern Bangkok, a majestic Boeing 747 and several MD-82s found a resting place on an empty piece of land next to the road. A few European children and their parents – following, like us, online advice on what to do with children in Bangkok – are climbing around the enormous hulls. It is the first and probably the only time I explore the upper deck and cockpit of a Jumbo Jet. Allegedly, the defunct aircrafts were purchased to be dismantled and sold as scrap metal. Now, the family living on the premises has turned the picturesque graveyard of airplanes into a tourist attraction, charging substantial entry fees.

The bigger business, however, may lay in yet a different kind of cockpit: part of the 747's hull, cut in half to form two hangar-like structures, serves as a barn to breed gamecocks – the kind pitted against each other in cockfights. Cockfighting is both an old tradition and big business in Thailand despite efforts to outlaw it for good. I try to take portraits of

the proud roosters but I am not sure how much my interest in them is appreciated. Could I be a spy with a hidden agenda?





A thunderstorm is brewing. The sky behind the yard of pits and cocks turns dark, wrapping the hulls into dramatic light. An unexpected afterword to Clifford Geertz? Or have we just walked into an untold episode of *Lost*? The first heavy raindrops fall. The young woman who sold us tickets waves us off. A picture? – Yes. Smile. Goodbye.

We seek shelter in a nearby 7-Eleven. The sound of the convenience store's distinct bell, ringing each and every time the sliding doors open, wakes me out of this dream.

Fieldnotes

From Intimate Impressions to Academic Discourse

Magnus Treiber

Thrown into fieldwork

My PhD fieldwork certainly had a bumpy start – as is often the case. For me, the Eritrean capital, Asmara, had been a beloved place full of good memories from two tourist visits in the mid-1990s. In May 2001, after a few days of feeling excited to be back in the city, I realised that the Eritrean case was not the democratic post-revolutionary grassroots project I had somehow hoped for (Treiber/Redeker Hepner 2021). I had returned only a few months after the bloody border war of 1998–2000 between Eritrea and Ethiopia, which had resulted in the Algiers Agreement and a lasting ceasefire. Supportive members of Munich's Eritrean cultural association had provided me with a recommendation letter and contacted the Eritrean Embassy on my behalf. I was received at the airport by the family of a kind and well-respected diaspora activist, who allowed me to stay, for a time, in their house on the outskirts of the city. She had even put me in touch with her cousin, a university teacher in public health and a member of Asmara University's PhD committee. It was that committee that I would have to convince if I were to get research permission for my planned study on urban political culture and revolutionary emancipation at the grassroots level.

Soon my frustration grew, and I began to realise my initial naïveté. Certainly, I had to learn a lot – and in quick, drastic steps. I had to accept that Eritrea was a dictatorship; in fact, a few months later, the president

carried out his *coup d'état* from above. In that coup, he struck out at protesting students, long-time comrades from the days of the liberation struggle who were now part of Eritrea's government, journalists, all kinds of intellectuals and defectors and, above all, most of the people he claimed to represent. By this time, I had perceived that Eritrea's political culture was a deeply authoritarian one, founded fundamentally on fear. Following a protest letter authored by diaspora intellectuals – the so-called 'G13' – my diaspora support did not make much of an impression at all. The PhD committee's continuously repeated statement, "We are still discussing," was informally transmitted to me through my contact. It meant, essentially, "No-one here is willing to take responsibility for an apparently political topic, and we therefore prefer to wait silently until your three-month visa has expired." Furthermore, I tripped over academic intrigue of which I simply had no knowledge. For instance, when I tried to present my project idea to a local anthropologist, he started yelling at me in his university office. Calling my proposal unprofessional, he ordered me to leave and never return – an unforgettable encounter. Remarkably, though, this incident raised the interest of this person's long-time rival in the sociology department, who then declared his readiness to receive me. He awaited me in his office, along with two young Indian colleagues. While he silently chaired the evolving drama, I was once more yelled at and called ignorant and incompetent. A friend in the university administration laughed about both of my encounters and told me not to take it personally, but of course, I felt devastated. I had been introduced to local academic culture but not offered a seat. Apparently, the sociologist – we are still in loose contact – really tried to help me but could not gather sufficient internal support. Bewildered and insecure, I was about to give up. I felt particularly guilty because my parents had financed most of this first stay in Eritrea before I would secure a Hans-Böckler-scholarship the following year.

In addition, my host family's patriarch did his best to limit my movements in Asmara. He tried to make sure that I did not misbehave, until I could get my own room with other relatives of his in the Paradiso neighbourhood, closer to the inner city. From that point on, I would have my own key, at the cost of having no one to go to and no one with whom I

could talk. Unsurprisingly, I felt lonely and frustrated and often spent my evenings with only a stone-age laptop and the usual diet of dried dates, cookies, bananas and *raqi*, the local aniseed liqueur. However, I started to make interesting acquaintances and even rediscovered a friend whom I had come to know years before. When I got the chance to return to Munich for two weeks over summer, it felt like I was fleeing ‘the field’ and getting back to where I belonged. A colleague at the university welcomed me back the next day with a shout of “the fieldworker is back!” I answered many questions, but somehow I had to admit that my real successes were high scores in Minesweeper and Solitaire, a late-modern pastime that my more prominent forebearer Bronislaw Malinowski lacked:

Yesterday a week had passed since my arrival in Mailu. During that time, I was much too disorganized. I finished *Vanity Fair*, and read the whole of *Romance*. I couldn’t tear myself away; it was as though I had been drugged. Did some work, however, and the results are not bad for only a week, considering the terrible working conditions. (Malinowski 1967: 16)

While absent from what should have been my ethnographic field, however, I realised that I had indeed started a new social life elsewhere. Multiple relations had emerged and were evolving towards mutual curiosity, closeness, and responsibility. When I returned to Asmara, I was arriving in a place where I had friends who would welcome me back. My own pathway took an unexpectedly lucky turn, and just before my visa ran out, I received work and residence permits through the formally governmental – but relatively independent – Cultural Assets and Rehabilitation Project, to whom I offered to conduct an interview study on life in Asmara. Thus, my own problems grew to be less prominent, and I felt that I could pay deeper attention to the world around me. Eritrea had just come out of its border war with Ethiopia, and though the country was still in a state of shock, people felt that a political crisis was imminent. The fear of repression and renewed violence did not allow time for mourning.

25.09.2001.

The city is more beautiful than ever before. Almost empty, very sunny, warm, but not hot. Palm trees, whereas it has already started to rain at home. However, for one week, graveyard peace. Scary and dangerous. (Personal fieldnotes, own translation)

“I am a soldier”, a young man belonging to my host’s extended family told me laconically.

He did not try to explain much more, knowing that I had no idea of what he had been through, or of why he and his sick mother were hiding in a run-down shack. Slowly, I began to understand better. Strong impressions remain even after decades: a homeless mother’s baby crying at night in a dark backstreet just behind the Confederation of Eritrean Worker’s headquarters, the shocking news that Halima’s brother had died in unclear circumstances in national service or, that Mike was in prison, hands waving desperately from make-shift prison cells during a military raid for draft dodgers, the one bootleg CD with various Abba songs and Boney M’s “Daddy cool” (1976) that was played up and down in all of the city’s bars...

A close friend with a background in the Eritrean diaspora had come from Germany with her family for the summer holidays. On the evening of 10 August 2001, we were riding on one of Asmara’s red public buses together. Despite the darkness and the rain, we could see very well how the military police drove arrested students into Asmara Stadium, while desperate parents tried to throw blankets and food over the walls, not knowing what would happen to their loved ones. “*Dieses Land ist es nicht,*” my friend uttered. She was quoting a line from the song “Der Traum ist aus” (Ton Steine Scherben 1972), meaning, “This is not the country (we dreamt of).”

Diaries - grasping the immediate

In his book “How Lifeworlds Work”, anthropologist Michael Jackson looks back on his fieldwork in the late 1960s in Sierra Leone (Jackson

2018, Treiber 2021). Although fifty years had elapsed since then – and fifty years is a long time – Jackson was able to show that his then-ethnography was still of use. The long timespan allowed him to mirror ethnography's situational moments in a wider life course. This freed the aging but intellectually mature anthropologist from emotional entanglements with the immediate. When I look back at my fieldwork and take a new look at my own initial fieldnotes, I find them highly emotional, in that they express my own initial disorientation, distress, and loneliness – which does not surprise me. This phenomenon, as such, is nothing new, of course, even if it was an intimate and existential one for me. Had Bronislaw Malinowski been in a position to speak openly about his own difficulties and emotions instead of setting up a distanced and analytical method (probably as a result of feeling forced to mimic the natural sciences), anthropology's history across the 20th century would have looked very different. While his offensive formulations – which helped to plunge anthropology into a decades-long, if extremely productive, crisis – cannot easily be excused, half a century after the posthumous publication of Malinowski's diaries, we can acknowledge that diaries are always intimate texts that are not necessarily meant to be read by others. After all, in diary entries, personal notes are inseparably mingled with what would one day become 'an ethnography'. Diaries turn thoughts, impressions, and encounters into words (or sketches, drawings etc., Taussig 2011). They are inevitably fragmentary, grasping the ephemeral: their typical chronological order is invariably nothing but a first and very basic structure, open to whatever comes up, including personal judgements of others that we might not want to communicate openly (Schönborn 2007). All of this is also true for ethnographic diaries. While Malinowski's diary is a prominent example of a private, intimate and in parts therapeutic text, which was not originally meant for publication, others were written to document and leave something behind (see Germaine Tillion, who wrote as an inmate and eyewitness of a Nazi concentration camp, [1946]). Some also became the basis of professional autobiographies, such as Hortense Powdermaker's "Stranger and Friend" (1966), or of real-life satire, like Johan Voskuil's

“Het Bureau” (volumes 1–7, 1996–2000), which documents Dutch and European anthropology’s professional culture across several decades.

I don’t care for life with the missionary, particularly because I know I’ll have to pay for everything. This man disgusts me with his [white] ‘superiority’, etc. But I must grant that English missionary work has certain favorable aspects. If this man were a German, he would doubtless be downright loathsome. Here the people are treated with a fair amount of decency and liberality. The missionary himself plays cricket with them, and you don’t feel that he pushes them around too much. How differently a man imagines his life from the way it turns out for him! (Malinowski 1967: 16)

Like Malinowski, I wouldn’t disclose all my diary entries to the public eye. After all, a good part of these were about missing my then-girlfriend. Incidentally, Malinowski had had similar experiences.

As for homesickness, I suffer little enough from it and very egotistically at that. I am still in love with [...] – but not consciously, not explicitly; I know her too little. But physically – my body longs for her. (Malinowski 1967: 15–16)

Maybe bashful and appalled by his own thoughts, he immediately adds, “I think of Mother [...] sometimes [...]” (1967: 16). Hortense Powdermaker (1966) was more open and daring in her diaristic writing, eventually making public her initiation into anthropological fieldwork. Looking back into her fieldwork among the Lesu on Latangai (then New Ireland) in the late 1920s, she writes in the chapter “First Night Alone”:

That evening as I ate my dinner, I felt very low. I took a quinine pill to ward off malaria. Suddenly I saw myself at the edge of the world, and *alone*. I was scared and close to panic. When I arrived, I had thought the place was lovely. Everything seemed in harmonious accord: the black natives, the vividness of the sea and of the wildflowers, the brightly plumed birds, the tall areca palm and coconut trees, the delicate bamboo, the low thatched-roofed huts, the beauty of the nights with the

moon shining on the palm trees. But now the same scene seemed ominous. I was not scared of the people, but I had a feeling of panic. Why was I here, I asked myself repeatedly.

There seemed to be no adequate reason: anthropology, curiosity, career – all seemed totally unimportant. *Why* had I come? (Powdermaker 1966: 53)

Justin Stagl mocks anthropology's fieldwork fetish and compares the anthropologist's existential crisis and personal learning process with a psychoanalytical voyage of discovery into one's own ego (2002 [1985]). The ethnographic novice will then have to successfully pass a phase of catharsis and endure uncontrollable and barely expected hardship before emerging as the triumphant hero (Stagl 2002 [1985], see also Baumann 2022, and Stodulka/Dinkelaker/Thajib 2019). This tactic, of course, would only work once the (male or female) anthropological Indiana Jones was back in an academic environment, claiming recognition and a successful career. To admit outright failure would have been unacceptable, of course, but the anthropological discipline has an academic culture that can render individual moments of crisis meaningful – albeit only in retrospect.

Anthropological fieldwork no longer has to be, or appear to be, a dire and mentally stressful experience. However, a certain challenging engagement can, admittedly, help open up new vistas. Fieldwork, as anthropologists do it, is inevitably intimate and personal – and crisis in the field is inevitably an immediate experience. We consider our being-in-the-world (Ingold 2011, Heidegger 2006: 2–15), our social and emotional entanglements (Kulick 1995, Dubisch 1995), and our resonances (Wikan 1992) to be instrumental to our research, to how we learn about and correspond with lifeworlds (Ingold 2017). We will always have to learn from scratch and *in situ*. After all, anthropology's starting point is not the top-down application of pre-defined models and theories, but the pairing of a fundamental lack of knowledge with curiosity and the desire to understand. In contrast to other disciplinary traditions in qualitative research, anthropologists do not have to hide themselves as acting and affective persons – less because of our own (negligible) importance than

simply as a result of dependence on the senses inherent in ethnographic learning (or what others might call “data collection”, Ingold 2017). Sensual impressions, however, are not truthful *per se*, as Adorno argues in his critique of classical phenomenology (1970), but they have to be subject to our interpretation and discussion. After all, the evolving fieldwork situation remains beyond “our firm control” (Amit 2000: 16), a fact that needs to be well reflected in our work. Peter Hervik has, therefore, tried to rehabilitate fieldwork’s decisive first impressions, taking them to a point beyond their anecdotal and self-legitimising character, understanding thoroughly that the immediate experience will soon be overgrown.

Today, I am no longer able to distinguish clearly between my general knowledge of Maya culture at the time of arrival and what I learned subsequently. My experiences of the first phase of fieldwork have to a certain extent been transcended by cultural models gained from shared social experiences. I can recollect feelings and understandings of the first-hand experiences, but I make sense of them in new ways, because the local knowledge that I bring into them evolved. (Hervik 1994: 86)

A field diary might be chaotic and disorganised, unfinished and unrefined, biased and full of emotions and contradictions. It can be a good friend in a time of crisis or (or as much as) a critical, omniscient and unforgiving self-other, which is what Canetti means when he speaks of a “cruel partner” (Canetti 1982, Wirz 2009). However, we need these notes and records of what we saw, heard, smelt, thought, and felt in quite an instrumental way. Beyond the immediate field situation, these notes will allow us to reflect – thereafter, when there is some distance – on the conditions under which we were learning and rationalise our attempts to understand. Thus, the field diary becomes a key document in our ethnographic archive, and it is subject to further study (Foucault 1973, Taussig 2011).

20.04.2004

[...] Yesterday evening, I saw military police hunting someone down. Seems to be a daily routine. [...] Due to Independence Day celebrations, lots of police in blue uniforms and MPs in camouflage can be seen. I am going to buy new trousers. (Personal fieldnotes, own translation)

In his “Vocabulary for Fieldnotes”, Roger Sanjek (2001) lists headnotes, scratchnotes, fieldnotes proper, fieldnote records, journals and diaries, texts and tape transcripts, letters reports and papers, etc. When I read his text, which is full of references, for the first time, I found it dry, far too detailed, even unnecessary. It took me some time to understand what it was ‘really’ about. Sanjek was attempting nothing less than the legitimation of anthropological knowledge as it is won during ethnographic fieldwork. Anthropologists are disciplined by the continuous work needed to elaborate more and more refined texts out of preliminary notes, and they are forced to summarise and sum up, to focus and structure. At the same time, emerging (and expanding) text production allows retrospective insights into past states of perception and knowledge as they occurred in our anthropological learning processes. Thus, diaries do not only invite self-dialogue and self-reflection in the specific moment of their writing, but they also do so over time – if we possess the right professional discipline and willingness to write. The diary becomes the cocoon for both product and process, the elaborated and refined ethnography, as well as the empirical and intellectual pathway towards it. Theoretical inspiration and evolving thoughts, methodological considerations and fieldwork experiences, early attempts to formulate findings and conclusions – all this can be traced back retrospectively. Yes, anthropological fieldwork is no longer objective and scientific in the narrow sense of the word. However, it is not just a personal story. It documents our epistemological development and hermeneutical learning process, it backs up our academic contributions, and it justifies the existence of anthropology as an academic discipline.

How, then, should I understand my own entry, the one where I had to buy new trousers? It would have been easier to omit this entry, so as to not disqualify my work by opening it up to ridicule. Two decades later, I can only loosely recall the situation. Looking back, I guess, I was out of my depth, unable to get along with what I was witnessing, but still a participant in the daily life that was going on despite everything, as people longed for some normality amid the exceptional. Furthermore, this statement can be read as a need to stay, to carry on and go out, to see people and interact with them despite the politically tight atmosphere that prevailed. This sounds strikingly laconic and unemotional, but maybe I was simply unable to say any more – an obvious sign of my own powerlessness. In later years, I would record people telling me about torture, loss, and fear. In contrast to my earlier field diaries, I would take much greater care to note and document what I was told, but I would also comment less and let the words speak for themselves. My later field diaries are thus far more systematic and structured than their predecessors, offering fewer immediate impressions and, consequently, having reduced room for self-reflection. Of course, by then, I was far more experienced and I also knew many more people, whom I had met during shorter visits to my respective field sites. However, this might also have protected me from what I was told.

From intimate impression to academic discourse

Eventually, fieldnotes have to grow into academic contributions. They allow us to trace our finished work back to its beginning and show where our conclusions come from. However, two main problems remain, neither of which can be easily resolved: the accessibility of fieldnotes, and their interpretation.

If fieldnotes are the key to understanding ethnographic learning processes and academic writings, how can they be used? Should they be freely – or at least in limited ways – accessible? And, if so, how – in times of quick technological change? It is unrealistic to demand total transparency, since very few anthropologists would fully comply, fearing

harsh criticism, and sometimes consequences, for one's professional career. I do not see another way other than to appeal to anthropologists' common ethical responsibility, and to do so in the name of our common interest in the general transparency of academic debate and the construction of our arguments and conclusions. Fieldnotes do not have to be fully accessible, but anthropologists should allow partial access to them, in order to offer partial insights into their role. In this way, anthropologists can explain how they reached a certain conclusion, how they developed a certain argument further and even, perhaps, how they came to revise that argument thereafter. Fieldnotes are attempts to grasp surrounding lifeworlds from a necessarily personal perspective, and so they can never be objective and purely academic. Textual work is required to grow fieldnotes into academic (and academically interesting) publications. Notes have to be refined, organised, and properly edited in several steps (as Sanjek has shown). Looking back into this process, and providing, as they emerge, selected insights to others, will enrich our debate and render it more dialogical and less personal, less pseudo-heroic.

Who, then, has the right to judge? Those, who have been subject to the study, and their successors? Academic colleagues? The public? Just the author? Or all of us? I would not dare to give a general answer. In any case, our critique should accept that fieldnotes inevitably have a *preliminary* character. Of course, fieldnotes can be most interesting, particularly from a historical point of view. Fieldnotes mirror more than just personal encounters and immediate reflections. With some historical distance, we can expect to find in fieldnotes and diaries anthropological approaches and intellectual fundamentals, as well as wider political discourses, effects and the fragmentations of a certain era. It is probably easier to criticise what was written in a private, lost and lonely moment in a distant past than it is to capture ideological backgrounds and intellectual shortcomings in one's own work. However, it is an undeserved privilege that we, today, are able to contextualise earlier anthropologists' perspectives beyond what they could see and guess at the time.

Self-reflection and autobiography – Dilthey’s starting points – are not primary and are therefore not an adequate basis for the hermeneutical problem, because through them history is made private once more. In fact, history does not belong to us; we belong to it. Long before we understand ourselves through the process of self-examination, we understand ourselves in a self-evident way in the family, society, and state in which we live. The focus of subjectivity is a distorting mirror. The self-awareness of the individual is only a flickering in the closed circuits of historical life. *That is why the prejudices of the individual, far more than his judgments, constitute the historical reality of his being.* (Gadamer 2004: 278, emphasis in the original)

If it is true, then, that others might see more than oneself in one’s own fieldnotes – now, but even more so in future – what will they find? There may still be some sort of heroism, although its currency of success has changed. How do today’s academic capitalism (Münc 2014) and competition, the scramble for grant money, impact factors, careers and new buzzwords sediment into our very personal learning processes and our attempts to note these down? In the immediate crisis, we understand well that fieldwork is not an aim in itself, that we have also come for academic merits and a potential career: “*Lenfer c’est nous*”, ‘hell is us’ (Lévi-Strauss 1968: 422).

Still, it is our learning process, an inevitably personal one, that allows us to make a valuable contribution to the academic discourse and, perhaps, beyond. Fieldnotes documenting this process are fundamental to our academic contributions; they show how our arguments are built on an empirical base and they accompany our developing thoughts. They also show us as an unfinished learner and a “Child of Our Time” (Horváth 1939).

I think we had best soft-pedal the self-righteousness: We do what we do, create what we create. We should take pride in doing it as well as we can. But it’s not a bad idea now and then to take a look in that mirror we are so anxious to turn on others and to face some of the tensions in a role that we often need to explain and sometimes need to defend. (Wolcott 1995: 153)

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Section Four: More-than Politics

Intimate Suspects

Politics of Fieldwork and Intimacy under State Surveillance

Pascale Schild

Introduction

Unexpected and intimidating encounters with state surveillance are certainly not what anthropologists would wish for their fieldwork. In many research contexts, however, anthropologists must devote considerable time to complying with the regulations and demands of intelligence agencies. Moreover, they must endure and emotionally navigate the uncertainties and anxiety that come along with experiences of being shadowed. The usual reason for state surveillance is that (foreign) anthropologists arouse suspicion and are seen as potentially dangerous figures or even spies. In this chapter, I seek to explain how suspicion relates those who monitor with those who are monitored. I want to trace the ambivalent and intimate workings of mistrust and suspicion in the everyday relationships and interactions between the anthropologist and her 'shadows', using my own example as a woman researcher in the 'disputed territory' of Pakistan-controlled Kashmir.

Martin Sökefeld – my PhD supervisor – and some of his other PhD students also felt the presence of 'agencies' or 'agency *wale*', as they are called by people in Pakistan and Kashmir, during their fieldwork in Gilgit-Baltistan (Grieser 2016; Sökefeld/Strasser 2016). Interactions with intelligence officers may represent the opposite of what this *Festschrift* for Martin explores as anthropologists' 'hobbies' in the field. However, I

would claim that from an epistemological perspective, Martin did not understand his fieldwork hobbies, such as hiking, photography and truck art, as radically different from his encounters with state surveillance in the field. Rather, in his work, he fundamentally thinks of both the pleasant and the less pleasant moments of fieldwork, as instances in which the ‘multi-sided ethnographer’ co-creates knowledge with the people he interacts with, whether they are interlocutors he chooses or ones who choose him. For the multi-sided ethnographer, there are hardly any events or feelings that take place ‘beyond research’.

The blurred lines between work and leisure (or ‘hobbies’), professional and personal life, are crucial to understanding the ideal and practice of ethnographic research as near-complete immersion in a certain social and political context. At the same time, however, it is this immersion that often brews mistrust and suspicion. As Martin Sökefeld and Sabine Strasser note in their introduction to one of the few publications on anthropological fieldwork “under suspicious eyes” (2016: 159), the practices of participant observation often do not correspond to the prevailing image of a ‘scientist’ and their professional work:

In many countries, social science is largely identified with conducting surveys, and as such a ‘scientist’ that does not work with questionnaires and spends most of her or his time simply hanging out with people does not appear to be scientific at all. In an environment deeply infused with mistrust, the conclusion is not far-fetched that the self-proclaimed scientist who apparently does not really do science is, probably, a spy. (Sökefeld/Strasser 2016: 164)

Given such an image of science, ethnographic fieldwork is barely recognisable as ‘work’ but rather conforms to how people, observing and being observed by an anthropologist, would expect a researcher to spend their ‘free time’. What may seem merely a curiosity and perhaps strange and amusing to some of our research partners must almost inevitably appear suspicious to the agents and citizens of a ‘security state’ like Pakistan, where military-shaped nationalism constructs the nation as being under threat (Ali 2013; 2019). But there is yet another

reason. In many ways, ethnographic fieldwork and participant observation resemble the secretive practices of intelligence agents (Verdery 2012; Sökefeld/Strasser 2016). The similarity between anthropological research and intelligence work is aptly depicted by John Borneman and Joseph Masco:

Both involve looking, listening, eavesdropping, taking notes, recording conversations, snapping photos, and establishing trusted confidants. We call it participant-observation; they call it spying. We seek informants; they seek informers. Both intend to understand and create a representation of someone else's reality. (Borneman/Masco 2015: 781)

However, while this similarity explains the suspicion that anthropologists may arouse in a certain political context, it does not tell us much about how this suspicion shapes the lives and practices of both anthropologists and the intelligence agents monitoring them. As I argue in this chapter, suspicion creates and maintains a social relationship between anthropologists and 'their' intelligence officers, forging an uncertain bond of intimacy between them. In other words, suspicion and the blurred lines between professional and private life relate anthropologists and their 'shadows' to one another as intimate suspects rather than socially distant opponents. As an anthropologist in Pakistan-controlled Kashmir, I was not the only one whose boundaries between professional and personal life were blurred (in the name of my research). In a similar way, one of my 'shadows', an intelligence agent from Pakistan, surrendered the separation of his professional life from his and his family's private life (in the name of his duty, my safety and the safety of his country). This is how we invaded each other's lives and how 'the other' was experienced as both suspicious and familiar, vulnerable and threatening, caring and unpredictable.

Uncertain and intimidating encounters with state surveillance are not merely scary interruptions or, at best, annoying sideshows to anthropological research. Rather, as anthropologists we need to think through such encounters to deepen our understanding of political power rela-

tions and how they shape not only our interlocutors' lives, but also our own practices as researchers. By pointing to my ambivalent and complicated role and positionality as an anthropologist in the field, and its wider political context of 'military nationalism' and (post-)coloniality, my relationships and interactions with intelligence agents show how I was both exposed to and part of local power relations. At times, the uncertain presence of intelligence agencies in anthropological research also points us to the ways our research partners may experience our presence as anthropologists in their lives.

Blurred lines: 'Work' and 'free time' research

"Come and visit my home, whenever you are free (*mere ghar a jao jab free ho*)". I frequently received such invitations from my women neighbours, who were also my research partners, when I passed their houses on my way to the city centre, where I had 'some work' (*kuch kam*) to do, for example meeting and interviewing a local state official. My neighbours distinguished my work from my free time in a way that seemed at odds with how I saw this distinction myself, i.e., as largely non-existent. The time spent with families in their homes was as much part of my research as my frequent visits to state authorities in Muzaffarabad, the capital of Azad Jammu and Kashmir (AJK), one of the two parts of Pakistan-controlled Kashmir (the other being Gilgit-Baltistan). Tracing local and translocal politics of relief and reconstruction in the everyday lives of survivors of the 2005 earthquake, my work in AJK drew on research both within and outside the political domains of disaster government and bureaucracy (cf. Schild 2019; 2022). The processes of reconstruction and the distribution of relief by state authorities were often not transparent to people (including me) and were surrounded by speculation and rumours. My research with women and their families was aimed at understanding how this non-transparency of the state and political authority in AJK manifested in people's practices and struggles to rebuild and reshape their homes and lives after the earthquake. Moreover, I sought to place and understand my interlocutors' lives and struggles in the context of the so-

cial and political uncertainties extending beyond the disaster and related to Kashmir's (post-)colonial past and present and Pakistan's rule in AJK.

I had spoken several times about my work with all my neighbours, and they were aware that learning about their everyday lives was an important part of my research. However, while we were “hanging out” (Geertz 1998: 69) together, my role as researcher often faded into the background, not only for my research partners, I believe, but also for me. While my professional role extended into (other people's) leisure time, the reverse was also true: I sometimes found myself in a leisure mood and in the role of a friend while hanging out with neighbours ‘doing research’ (or what else was I doing?). Only at some points during the visit or later – for example, when I took my notebook out of my bag to write something down, or when I had returned home in the evening and completed my fieldnotes about the day's events – did I feel myself turning into something like a ‘professional’ researcher again.

It has been noted many times in anthropology that the boundaries between work and leisure, professional and personal life, are often blurred in and through ethnographic fieldwork. Classical fieldwork, practiced and idealised as immersion in a place and a social context (cf. Malinowski 1922), involves the researcher as a whole person, replete with all their feelings, emotions, privileges and vulnerabilities (Behar 1996; Davies 2010; Thajib et al. 2019). In the field, it is often not clear where an anthropologist's work ends and their free time begins – any more than their analytical mind can be neatly separated from their positionality and embodied research practice (Berry et al. 2015).

And yet, as researchers, we experience boundaries between work and leisure that structure our practices and everyday lives in the field. In this way, the invitation “come and visit my home, when you are free” shaped my work and life as an anthropologist, creating a sense that it was ‘free time’ I was spending when hanging out and doing research with women interlocutors in their homes. Over time, I therefore came to experience and think of this part of my work as my ‘free time’ or ‘private life’ research.

This shows, I believe, that ethnographic fieldwork is not only about the adaptation and immersion of the anthropologist in a social context, but also about the context immersing into the researcher and their body.

The way in which I experienced the boundaries between work and leisure highlights my embodiment of local power relations and gendered norms of *purdah*, i.e., the separation of women from men, the home (*ghar*) from the outside, the city or the bazaar, and private from public. Women in AJK usually do not leave their house or neighbourhood (*mohallah*) alone and unaccompanied by a male relative, or, if they do, it is not without a good reason such as going to work or school (and certainly not for leisure), and they always return home before sunset.

Image 1: A group of women sitting together in the morning sun and chatting at a neighbour's home.



Photo by Pascale Schild

As both a researcher and a foreigner, I was able to transgress many of the gendered norms to which my interlocutors were exposed. The position of the foreign researcher gave me greater freedom as a woman in AJK. This was the privileged position of the white woman (*gori*), one that is closely related to that of the coloniser, namely the British (*angrez*) in South Asia. However, my privilege did not always position me outside local power relations. People's expectations of my role as both a woman and

a researcher/student shaped my everyday practices, and I both adapted to and embodied these expectations. As a result, I ended up regularly visiting my women neighbours and friends, not only to do research, but also to spend my 'free time' with them, to relax and emotionally recover after work when I returned home to the *mohallah* from the city centre, where I had been talking mostly to men in public offices.

My embodied adaptation to local gendered norms and related boundaries between work and leisure, home and the city, points to a specific culture of surveillance that women – and foreigners in particular – are exposed to in AJK. Anna Grieser (2016) experienced such gendered surveillance in Gilgit-Baltistan, albeit in a much more extensive way. By enforcing patriarchal norms, and thus restricting the mobility of foreign women researchers, intelligence officers “aim[ed] at both monitoring the foreigner *and* controlling the female” (Grieser 2016: 190). In AJK as well, forms of transgression and difference arouse suspicion and are closely watched, not only by local communities and the police, but also by various Pakistani intelligence agencies operating in the region. I adapted to the local norms and practices of *purdah* because I did not want to attract any more attention, especially from men and intelligence agents, than I already did as a foreign, unmarried woman in AJK. I dressed in *shalwar qameez* and tried to do my work outside the neighbourhood as straightforwardly as possible. When I walked in the street, I hastened my steps and lowered my head, and when I spoke to strangers, I avoided eye contact and limited the conversation to what was necessary. In this way, I tried not only to meet the moral expectations imposed on women, given their role in society, but also to protect myself from harassment and assault that might result from disregarding the local patriarchal order. The more I immersed myself in the place where I worked and lived, the more I experienced 'my' home and neighbourhood as familiar and safe, in contrast to the uncertain outside and suspicious anonymity of the public, the city and the bazaar.

The opposition between the home and the public is a cultural construction reproducing gendered norms and inequalities. The idealisation of the home as the caring and safe place for women and children ignores the silent suffering and violence, including harassment and

assault, women often experience in this place (Schild 2021). And yet, if there is one space where women can rest, feel safe and have fun with others, it is in each other's homes. This is how I came to experience my interlocutors' homes, too, and where I went to spend my free time, to hang out with neighbours and friends, to chat, joke and laugh together, to watch TV, to take a nap, to care and share in the lives of others, to drink tea and have lunch or to go out visiting other neighbours' or relatives' homes. Research turns into leisure, and leisure into research. In the shared moments of pleasure and excitement, but also of boredom, sadness and worry, emotional bonds of affection are created that also frequently turn research partners into dear friends. This makes it even more impossible to discern where the 'work' of anthropologists ends and their 'free time' begins. The point, however, is perhaps not so much that boundaries between work and leisure do not exist in ethnographic research but that they are fluid and constantly shifting – clearly drawn in certain situations and left blurry in others.

Pakistan's military nationalism and politics of surveillance and suspicion in AJK

While an ethnographer's role is multi-faceted as well as contradictory and difficult to grasp, not only for themselves, but also for their research partners, to whom they may both become a friend and remain a stranger, it is often also a suspicious role. This is especially true for a 'disputed territory' like AJK where foreigners (but also local Kashmiris) are seen as potential spies and are therefore closely monitored by various special branches of the police and civil and military intelligence agencies, the most prominent and powerful of which is Pakistan's military Inter-Services Intelligence (ISI).

Pakistan's surveillance in AJK illustrates the ongoing conditions of coloniality that structure politics and power relations in the region. AJK is part of the Kashmir dispute between Pakistan and India, and it is under the control of Pakistan. The region's relationship with Pakistan embodies the historical predicament of the Kashmir dispute, i.e., it is am-

bivalent and reveals both the political marginalisation of AJK within the Pakistani nation-state and the importance of this region in sustaining Pakistan's military-shaped nationalism, with India as the aggressive and dangerous enemy that constantly threatens the existence of the Islamic nation. Pakistan's "saviour nationalism" (Ali 2019: 16) in relation to Kashmir holds that, in contrast to the parts of Kashmir across the 'border' – the military *Line of Control* (LoC) – AJK is 'free' (*azad*) from Indian occupation. However, this 'freedom' has come in the form of great political and military dependency on, and domination by, Pakistan (Snedden 2012).

AJK – like Gilgit-Baltistan, the other part of Pakistan-controlled Kashmir – is neither an autonomous state nor a province of Pakistan but a disputed, and therefore highly militarised, region shaped by processes of "postcolonial colonialism" (Sökefeld 2005: 939). In these borderlands and domains of ambivalence, Pakistan's political and military rule in the name of 'national security' is revealed by, among other things, a distinctive culture of surveillance and suspicion (Ali 2013; 2019).

Historically, the emergence of AJK is linked to politics during colonial times, which resisted autocratic rule in the former state of Jammu and Kashmir (J&K) and related discrimination against Muslim subjects. In the turmoil of the partition of British India in 1947, Kashmiri Muslims in the southwest, the south of today's AJK, started an armed uprising against the Maharaja of J&K, who had ruled under the auspices of the British and was yet to decide about the future of the state. To end autocratic rule, the political leaders of the Kashmiri uprising deposed the Maharaja and formed the rival 'Provisional Azad Government' of J&K. The Maharaja turned to India for military support to suppress this local struggle for *azadi* (freedom) and subsequently declared J&K's accession to India. The resulting war between Pakistan, supporting the Kashmiri freedom fighters, and India ended in 1948 with a UN-negotiated ceasefire line that later became the LoC, dividing J&K into Indian-controlled J&K and Ladakh, and Pakistan-controlled AJK and Gilgit-Baltistan (Snedden 2012; Hayat 2020).

Although the 'provisional' AJK government at times favoured accession to Pakistan, the Pakistani government has always refused to recognise AJK as a province of Pakistan, as the region's integration into

the nation-state would undermine Pakistan's claim to the entire J&K territory (Snedden 2012). At the same time, neither Pakistan nor the UN recognises AJK as a sovereign state and alternative government for J&K. The UN has referred to AJK only as a provisional 'local authority', while Pakistan, with the consent of the local Kashmiri leaders, has taken control over all matters of the state relating to defence, foreign policy, negotiations with the UN and Gilgit-Baltistan. To this day, Pakistan claims to be protecting AJK from the Indian threat and supporting the local state temporarily until the people of Kashmir – through a UN-led referendum – determine the political future of the entire J&K state, including AJK. In practice, however, Pakistan has integrated and dominated AJK through different forms of bureaucratic, constitutional and military domination (Snedden 2012; Hayat 2020).

Moreover, in its role as (temporary) protector of AJK, Pakistan often uses this claim as a justification for its political and military presence in the region. Indeed, without its financial support and political protection, the Azad government – established initially as a war council – would not have been able to survive and run its own local state beyond the ceasefire. At the same time, however, local people are suspicious, with their loyalty to Pakistan being constantly called into question, mostly because of the possibility of propagating Kashmir's independence (cf. Snedden 2012). Ultimately, this suspicion of Kashmiris has led to Pakistan's continued control and surveillance of the local state, society and politics in AJK through intelligence agencies, with agents hailing mostly from Punjab or other parts of Pakistan, and locals serving as informers. The culture of suspicion and surveillance that Nosheen Ali (2013; 2019) notes for Gilgit-Baltistan essentially exists in AJK as well:

The military-intelligence regime accomplishes its rule in the region not only by rendering its citizens suspect and using this suspicion to further its control, but also by promoting suspicion among citizens and hence disrupting local political solidarity and resistance against the military regime. Apart from suspected subjects, it thus also produces suspicious subjects. (Ali 2013: 109)

Apart from creating mistrust among Kashmiris (and towards foreigners), as anyone could be a potential spy and informer, Pakistan also resorts to other mechanisms of domination and control. By 'lending' officers to AJK's administration, for example, the government in Islamabad can appoint loyal Pakistanis to the most influential political positions in AJK, including the chief secretary, who acts as the intermediary between local government and the administration (Snedden 2012). Moreover, AJK's politicians and officials are bound by the constitution to ensure loyalty to the political ideology of Kashmir's accession to Pakistan. The requirement to sign a declaration of loyalty has regularly silenced and prevented candidates from pro-independence groups from contesting elections and seeking state employment in AJK (Snedden 2012; Mahmud 2021).

Foreigners are exposed to a similar regime of suspicion and surveillance, as well as additional regulations and monitoring. Unlike Kashmiris and Pakistanis, for example, they must obtain a non-objection-certificate (NOC) from Pakistan's Ministry of Interior to travel to AJK. When I travelled by public transport, the bus was usually stopped at the border crossing between Pakistan and AJK. Officers checked the vehicle for suspicious passengers, and as a white woman, I often attracted the attention of the vigilant officers, who would ask me to get off the bus and follow them to a nearby barrack. There, they checked my passport and papers, including my visa and NOC. They asked me why I was travelling to AJK and where I was staying in Muzaffarabad. Then they took my picture and wrote down my details in a thick notebook. I was also sometimes checked on the street or at my home in Muzaffarabad. Since various civil and military intelligence agencies operate in AJK, it remained mostly unclear to me whether it was always the same agencies who checked me, why they kept checking me and the same papers again and again, and whether the checks were spontaneous or planned in order to intimidate me and the people with whom I worked and lived. Usually, the officers explained that it was all for my safety and that they were only doing their duty.

In AJK's ambivalent relationship with Pakistan, the lines between protection and control, care and suspicion are often blurred. Moreover,

the country's national security concerns regarding foreigners are multiple and complicated and relate not only to the enmity with India, but also to the imperialist politics of the US. These concerns range from abductions of tourists by Islamist groups, to operations by foreign intelligence agents in the Kashmir dispute, Afghanistan/Pakhtun-US relations and the so-called 'war on terrorism', all of which undermine Pakistan's national sovereignty. While I was doing my fieldwork in AJK, on January 27, 2011, Raymond Davis, who worked for the US Embassy in Pakistan and was later exposed as a CIA agent, murdered two young men on a motorbike in the streets of Lahore. However, amidst large public protests, Davis was released and returned to the US, thus avoiding prosecution by Pakistan's judiciary. Only a short time thereafter, on May 2, 2011, a special US Navy Seals commando tracked down Osama Bin Laden and killed him and four other people at his hideout in Abbottabad in Pakistan. The multiplicity of security concerns makes it impossible to discern whose safety is being maintained when foreigners like me are monitored: mine, that of the people of AJK or that of Pakistan and/or its military-intelligence regime.

Anthropologists, intelligence agents and research ethics

The uncanny and opaque presence of intelligence officers in my fieldwork in AJK sheds a rare, penetrating light not only on my research partners' everyday lives under suspicious eyes, but also on my conflicting and uncertain role as an ethnographer in the context of both territorial conflict with India and the 'war on terrorism' by the Global North. As a security state, Pakistan, through its military, dominates AJK, but at the same time it is dominated by US imperialism and global geopolitics. Therefore, both my presence as an anthropologist and that of agencies (due to my presence) must at times be experienced by interlocutors and other people around me as opaque and potentially threatening. Who is this person? What is she doing? And why?

As anthropologists, we have more in common with intelligence agents than we would like to admit. Martin Sökefeld often hinted at this

affinity when he jokingly claimed that anthropologists would also make good intelligence agents. One might counter that the former are bound by research ethics and obliged to be open and honest with their research partners and do them no harm (AAA 2012), yet history shows how colonial and imperial regimes have repeatedly used anthropologists and their knowledge to dominate local communities. It is no coincidence that the debate on research ethics in anthropology took shape in the late 1960s, when it came to public attention that the US military had planned to enlist anthropologists for research supporting counterinsurgency operations in Latin America (Sökefeld 2022; Sökefeld/Strasser 2016). The knowledge created by anthropologists is both potentially valuable and potentially dangerous for a “delusional” (Ali 2019: 6) state with high military spending for its national security, depending on who they work (and spy) for. It is therefore hardly surprising that foreign researchers in Pakistan are monitored by intelligence agencies. For instance, what I uncover in my research could be destabilising for Pakistan’s rule in AJK. But my knowledge could also be of value to the ISI and be used to track down and arrest local Kashmiri critics of Pakistan. At the same time, the everyday practices and personal intentions of intelligence agents working on the ground and at the lower levels of bureaucratic hierarchies may influence surveillance in unexpected ways. In other words, as anthropologists, we often do not know how our knowledge is – or will – be used, nor whether our publications are read at all.

From the perspective of research ethics, however, my obligation to be transparent with the people I interact with for my research turned out to be very complicated when I was confronted with intelligence agents who questioned me about my research and interlocutors. I especially recall the visit of two officers who pretended to be journalists and visited me at my home in Muzaffarabad for an interview about my work. At first, I wanted to be as open with them as possible and to share my knowledge with the local Kashmiri community they claimed to represent. Moreover, I did not want to hide anything, so as not to be suspected of being a spy or to reinforce such a suspicion. When the visitors took out their pens and notebooks and started asking questions in English, I soon realised that I was not interacting with journalists. They did not make much ef-

fort to hide the fact that they were agents but asked very detailed questions about whom I had visited and when, and what people had said about the earthquake and reconstruction. I explained to the visitors that I could not reveal my sources, which I was sure they as journalists would understand. I then spoke mainly about technical issues of disaster reconstruction, and I avoided mentioning politics and local conflicts over Pakistan's dominant role in reconstructing AJK (Schild 2015; 2019). It was certainly an advantage that I had only recently arrived in Muzaffarabad, and I was able to tell them that I had just started my research and to ask them counter-questions about their experiences of the earthquake and its aftermath. The 'interview' turned out to be very short. What remained with me, though, was the unsettling feeling that I had made myself unavailable – and therefore suspicious – to intelligence agents, because I had chosen not to be transparent with them but to protect the identity of my (other) interlocutors and research partners.

While the debates about ethical standards for anthropological research are important, the standardised procedure for the ethical clearance of research projects increasingly imposed on anthropologists by their universities is often not helpful. My example of interaction with covert intelligence agents underpins Martin Sökefeld's recent call to "Keep research ethics dirty!" (2022). Indeed, we need reflexive research practices that are ethical in a more holistic, relational and politically engaged sense, given the unpredictability of fieldwork encounters and the uncertain and blurred boundaries of ethics and politics in most research contexts.

Intimate suspects

At the beginning of my research, I was visited at my home in Muzaffarabad every two or three weeks by intelligence officers. However, such encounters became less frequent five or so months later. While it might be that I was simply unaware of the surveillance, I believed that the more time I spent at the homes of neighbours and other research partners rather than in public, male-dominated domains, the more the intelli-

gence agencies lost interest in my research. As a woman and an anthropologist working on and with women and their families, my knowledge would perhaps be considered less valuable and, potentially, less threatening, so I was no longer worth being closely monitored. However, it turned out that I was wrong. The agencies did not lose interest in my research at all; rather, my 'private life' as a researcher increasingly presented them with unexpected difficulties and finally compelled them to take a rather drastic step.

Towards the end of my research in Muzaffarabad, my neighbour Ambar told me one day that they would finally be renting out their prefabricated house her husband, Asif, had received as earthquake relief. The building was unoccupied and located on a small plot next to the house where Ambar and Asif and their four children lived. When the new tenants arrived, Ambar invited me, along with their relatives living nearby, to a small inauguration ceremony. I learned that the tenants were a family from the Punjab in Pakistan. The husband was on duty in Muzaffarabad and had brought his wife and children with him. I first met Zahur and her two little daughters, and a few days later I met her husband, who introduced himself as 'Tanvir'. I was shocked when I immediately recognised him as an ISI agent I had previously interacted with several times. 'Tanvir Sahib', as I now called him, was one of the officers responsible for my surveillance and security in AJK.

Tanvir Sahib's moving with his family to Muzaffarabad and into my nearest neighbourhood was certainly not a coincidence but a deliberate move by the ISI to improve their surveillance of me by invading my 'private life' as a researcher. The boundaries between home and the outside, private and public, 'work' and 'free time' research as I had come to experience and draw them, in the way my research partners did, turned upside down.

In retrospect, I believe that ethnographic fieldwork, and with it the blurred lines between 'professional researcher' and 'private person', made me a suspicious figure and presented intelligence agencies with the problem of how to monitor me when I was moving – outside the public domains of society – in and between homes and along the narrow and rather hidden paths and within the intimate networks of women in

the neighbourhood. However, the step taken by the ISI also reveals that I was not the only one for whom the boundaries between professional and personal life were blurred. After Tanvir Sahib and his family had become my neighbours, I frequently interacted with Zahur, whose home I had to pass when I was visiting other neighbours and research partners. Most of the time, we only made small talk, but against the backdrop of her husband being an ISI agent and claiming to be responsible for my safety, her otherwise trivial questions became suspicious to me. How are you? Where are you going? I am not sure if she reported my answers to her husband, but she probably did. Tanvir Sahib and his family became part of 'my' neighbourhood and 'private' research network, which enabled him to track my movements and social relationships outside of public domains. At the same time, he made his presence, as an agent closely monitoring me, *felt* in the domains that I previously had experienced as relatively safe and protected from the male gaze of the public and state surveillance. In other words, with Tanvir Sahib and his family as my next-door neighbours, I was exposed to suspicious eyes that no longer excluded my 'private life'.

In *Discipline and Punish*, Michel Foucault (1977 [1975]) traces the intimate and embodied workings of surveillance in the lives of those being monitored, pointing out that it is the mere knowledge of the possibility of pervasive surveillance that brings people under disciplinary and increasingly self-disciplinary power. Explaining this panoptic experience, Sökefeld and Strasser note:

Surveillance often operates in a way that it makes itself known, at least to some extent. Also, in many cases, secret services are not *that* secretive, and so we often know or at least sense that we are being watched, and this necessarily changes our way of acting in the field and the manner in which we view social relations. (Sökefeld/Strasser 2016: 165)

The knowledge of being watched by my intelligence agent-turned-neighbour brought new feelings of uncertainty and suspicion into my life as a researcher and related experiences of home, friendship and leisure:

who was a neighbour and friend, who was an informer and agent? But even more so, I worried about the safety of my research partners and was confronted with ethical questions. Had my research partners been threatened and put under pressure by the ISI? Was my presence in their lives dangerous to them? Should I continue to visit and interact with my neighbours and friends?

While suspicion and state surveillance have the power to penetrate the bodies and intimate lives and relationships of those being monitored, intimacy and in-depth knowledge of the 'other' can also produce and reinforce suspicion and mistrust. In "Specters of Treason" (2010), Tobias Kelly and Sharika Thiranagama point out that "forms of surveillance increase not only the sense of what can be known but also the sense of what is not known, amplifying the feeling of suspicion and the impression that betrayal is always possible" (Kelly/Thiranagama 2010: 15). The drastic move by the ISI to send an agent to live with his family in my neighbourhood is perhaps a result of such self-reinforcing tendencies of surveillance and knowledge about those being monitored.

However, I would like to reveal another dimension of intimacy that is linked with surveillance as a social relationship that constitutes new forms of knowing and interacting between those who monitor and those who are monitored (as well as those around them). In anthropology, the concept of 'intimacy' connects 'the personal' and 'the political' and seeks to explore "the uncanny fold in which inner and outer life are disjunctively blended" (Mazzarella 2017: 199); that is, both the "intimately impersonal" and "impersonally intimate" (*ibid.*). The concept explains kinship, neighbourly relationships, friendship and community beyond the personal/political and public/private divide and refrains from attributing only pleasant emotions and feelings to intimate relationships (Berlant 2008; Sehlikoglu/Zengin 2015). While intimacies create and involve suspicion, enmity and hatred no less than affection, trust and mutual care (Kelly/Thiranagama 2010), I would like to reverse this perspective and claim that surveillance not only penetrates, but also creates intimate relationships.

My surveillance brought with it new intimacies and social relationships for me and 'my' intelligence agent, as well as for his family mem-

bers and my research partners, neighbours and friends. Tanvir Sahib and his family became part of 'my' home and *mohallah*, with Zahur participating in everyday relationships and the social exchanges among women neighbours and friends. She visited Ambar and her relatives regularly and asked them for small favours, whilst their children played together every day. In response to my mistrust of Tanvir Sahib, which I obviously could not always hide, Ambar explained that Tanvir *bhai* (brother), as she called him, was only doing his duty (*is ki duty hai*). In an important way, he and his family were just 'normal' neighbours. Therefore, not only does surveillance penetrate intimacy, but intimacy also invades surveillance, thereby making the strange familiar, the distant close and countering suspicion not always with even more (intimate) suspicion but sometimes also with trust, care and friendship.

Epilogue

When I returned to AJK in 2012 on a tourist NOC, much had changed, and I was no longer allowed to stay with a local family but had to stay at a hotel. Tanvir Sahib was still on duty in Muzaffarabad. His family, however, had moved back to the Punjab soon after I had completed my fieldwork and left. He came to meet me at the door of my hotel room almost every evening, asking me about where I had spent the day and at whose house. Since he had gained an in-depth knowledge of my research relationships, I could mention just the names, sometimes even only those of women friends, and he knew where I had stayed and whom I had met. After all, he had closely watched and researched me during my fieldwork.

My accounts of the day, I believe, were then compared and/or supplemented with those of other officers who shadowed me during the day on their motorbikes but sometimes lost track of my friends and me when we were walking along the more informal paths through the hills on the outskirts of the city. On one occasion they called me on the phone, and since I could not tell them where exactly I was, I handed the phone to my friend Shazia. I could hear the officer asking her rather desperately, "*Oh beta, tum kidher ho* (Oh child, where are you)?" Shazia tried to explain our

location to him and mentioned the name of her late grandfather whose house we had visited, but the official did not seem to understand. We laughed about the incident many times later on, imagining the agent getting lost in the *mohallah* with its many narrow and hidden footpaths while trying to find us and get his motorbike back to the main road.

Image 2: The steep and narrow paths of a neighbourhood on the outskirts of Muzaffarabad.



Photo by Pascale Schild

Perhaps we were naively lucky, but the incident did not have any serious consequences, for me or for my friends. However, after that phone call we had a call from Shazia's mother, whose brother had met several officers looking for me on his way home from work. Shazia's mother told us to come home immediately. My friends were ambivalent about the 'agency *wale*', and most people – including Shazia and her family – did not seem overly scared of intelligence agents. They sometimes called them harsh and unyielding people (*sakht log*), but often they also excused and normalised their intrusive practices by claiming they were only

doing their duty (for my safety) and that I should not worry (cf. Grieser 2016).

The intimate and uncertain experiences of state surveillance in AJK are deeply intertwined with local politics and power relations, as well as the region's ambivalent relationship with Pakistan.

One very late evening, Tanvir Sahib visited my hotel room and knocked loudly on the door. I was already asleep and woke up startled; I was the only woman guest and all alone. This night-time intrusion and intimidation by the ISI agent was certainly part of monitoring practices shaping surveillance as a panoptic experience for those who are monitored, namely 'anytime, anywhere'.

One day later, I again became aware that Tanvir Sahib was closely monitoring me when I was stopped on the street in the afternoon by a Special Branch officer from the local AJK police. The officer was about to question me and check my papers when suddenly Tanvir Sahib came out of a small teahouse not far away. He asked me to leave and began to harass the other officer, who eventually left on his motorbike.

While the ISI made its powerful presence felt, the incident also showed that there was rivalry between intelligence agencies over responsibility for my surveillance and safety. Later, I even learned that Pakistan's ISI was certainly not all-powerful but had to confront local resistance and related moves to hold its officers accountable in AJK.

When I met Zahid Amin, a well-known local politician in the city, I somehow ended up telling him about Tanvir Sahib's previous late-night 'visit'. I did not expect Amin to do anything about it, but when Tanvir Sahib visited my hotel later that day, he was deeply offended that I had complained about him. He explained that Zahid Amin was an influential person (*bare admi*) in Muzaffarabad and that I was completely wrong to protest against him, since he was only protecting me. He pointed to the incident with the Special Branch officer, claiming that he had argued (*larai*) with the officer on my behalf and that I should complain about that officer instead. He then insisted that I write down the officer's name. At that very moment, I no longer saw Tanvir Sahib solely as 'my' intelligence agent but as a 'normal' man and a former neighbour who probably feared for his job and who had to provide for a family – one that I knew well! I

honestly hoped that my (unintended) complaint had no consequences for him personally.

Pakistan's political and military presence in AJK intimately shaped my relationship and interactions with an intelligence agent who was responsible for my surveillance and safety and to whom and to whose wife and children I and the people around me inevitably became close as neighbours. In this way, not only did suspicion come to penetrate my 'private life' and intimate relationships with my research partners, but intimacies were also planted into relationships of suspicion, turning them into familiar and sometimes even comical encounters, albeit not necessarily any less uncertain or less threatening ones. The anthropologist and 'her' intelligence agent became intimate suspects, bound together through political intimacies of everyday life and neighbourly practices, as well as through intimate (post-)colonial politics of mistrust and enmity entrenched in military nationalism and global geopolitical power relations.

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Agencies, Friendships, Nationalism and Anthropology

Fieldwork Experiences of Collaborative Research in Gilgit-Baltistan

Azam Chaudhary

Introduction

This paper is an attempt to put together some of the experiences I had during my research in and around Gilgit-Baltistan, particularly as a research collaborator with German academics, from 1982 to 2020 on the topic of surveillance of anthropologists during fieldwork. The idea to write about surveillance crossed my mind after reading two articles by Martin Sökefeld and Sabine Strasser (2016) and Anna Griesser (2016) in a special issue concentrating on ethnographers' experiences of different forms of state surveillance. I decided to write on this topic because I felt that the 'native perspective' of the surveillance was missing or only partially covered by these authors. Local perspectives included many different elements, such as the views of secret agencies doing surveillance, the views of administrators responsible for issuing NOCs, university administration, faculty, local people and especially my personal experiences of surveillance as a collaborating anthropologist in research projects with Sökefeld.

This article mainly builds on my collaboration with Martin Sökefeld, but it also draws on my work with other German researchers in Gilgit-Baltistan, and it is this second part of the experience that relates this article to the theme of this book, namely the multi-sided ethnographer.

From 1982 to 1995, during the *Rock-Carvings and Inscriptions along the Karakorum Highway* project, I took on many roles and duties, such as administrator, negotiator, facilitator, translator, research assistant and researcher. My identity was as a Punjabi, from downstream Pakistan, a Sunni Muslim working, eating and living with Germans (including in Germany) and someone who knew their language. These roles and positions also helped me to examine multiple aspects of suspicion against researchers, including foreigners and Pakistanis.

The broader question for this paper is as follows: why are anthropologists suspected and monitored, what impact does this monitoring have on their research and do anthropologists need to rethink their research methods if they are indeed being monitored? This paper also debates why no objection certificate (NOC) or research permits are issued to anthropologists despite accusations of spying? Another related question is: why do many anthropologists continue their fieldwork despite severe difficulties, and why do they sometimes even secretly continue research without permits regardless of knowing the consequences for their interlocutors, including their research collaborators?

I am of the view that suspicions of spying against anthropologists are neither new nor always baseless, especially when we look at the history of their involvement in spying activities. The nature of suspicion and surveillance of anthropological fieldwork depends upon a number of factors, such as who the anthropologist (foreign, or native, male, or female) is, the location of the research site (border or central area) and the type of population in the locale (multi-ethnic/multi-religious or homogenous). Gilgit-Baltistan as a place of anthropological research provides strong reasons for suspicion, and the insistence of anthropologists wishing to continue their research in difficult circumstances further strengthens these suspicions. Similarly, I believe that denying research permits to researchers is not an individual act, as it affects relations between countries in terms of cooperation. Therefore, it is not easy to do. Moreover, it is similarly not easy for anthropologists to change the location of their research and accept failure. Changing of location would require new research project and funding on the one hand and on the other hand, I think there seems to be a belief among anthropologists that the more

difficult/dangerous, the more distant, the more 'primitive' the field, the better the anthropologist.

In order to provide the reader with a glimpse into my experiences of work, association and research collaborations with Germans, I start this article by providing some background information. Thereafter, I describe my own different roles and positions that have caused suspicion against me in Gilgit. This is followed by some anecdotal first-hand experiences from fieldwork about surveillance and its consequences for my future research, career and life. The attitudes of people other than agencies, in relation to research and associated permits for foreign anthropologists, are then discussed. In order to explain fully why anthropologists are shadowed by agencies generally, and in Gilgit-Baltistan particularly, the special political and historical context of Gilgit-Baltistan, ethnographic methods and the history of anthropologists as spies are discussed.

My relevant background

The aim of this recounting of a number of important collaborations and projects is to provide a brief overview of my experiences with German academics and universities. In all these projects and collaborations, I have been responsible for performing several jobs: translator, interpreter, organiser of different events and activities, securing (in part) NOCs, sometimes even arranging boarding for visiting students and, in some cases, part of the negotiations for collaboration. This gave me the opportunity to meet and understand the points of view of different Pakistani actors about German anthropologists and researchers. The following description is based on my personal experiences of those years of work and collaboration.

In 1982, I became a member of the Pak German Study Group, a multidisciplinary team consisting mainly of Germans, engaged in the socio-cultural exploration of Gilgit-Baltistan, at that time called 'Northern Areas of Pakistan'. In 1989, I moved to Germany for higher studies and remained there until 1995, but in all those years, I also remained an

active member of the Pak German Study Group by working part time in its head office in Heidelberg and by participating in annual research expeditions to Gilgit-Baltistan. In 1996, I joined Quaid-I-Azam University, Islamabad, as a teacher and served the university until 2019, during which time I collaborated with German academics from different universities, including Free University of Berlin, University of Tübingen, Ludwig-Maximilians University Munich, University of Heidelberg and University of Duisburg-Essen.

The first project in which I also served the longest time (1982–1995) was *'Rock-Carvings and Inscriptions along the Karakorum Highway'*, funded initially by the German Research Foundation and later on by Heidelberg Academy of Sciences. The project head was Karl Jettmar, an anthropologist from Heidelberg, and after his retirement Harald Hauptmann, an archaeologist from the same university. Every year, a team of mainly German – but sometimes also French and British – scholars took part in a research expedition. My key responsibilities included interpreter, administrator, translator, informant and facilitator, and on occasion, I accompanied the team leadership in their negotiations with Pakistani bureaucrats and counterparts in order to secure no-objection certificates (NOCs) for research in Gilgit-Baltistan. The second project I collaborated on, albeit in a piecemeal manner and only in short bursts, was *'Culture Area Karakorum'* (CAK 1989–1995), funded by the German Research Foundation (DFG) and led by Irmtraud Stellrecht from Tübingen University. It was within the framework of this project that I first encountered Martin Sökefeld.

I have had a long academic association with Martin, and our joint ventures included summer schools, joint research projects, workshops, seminars, etc. The first joint activity was the research project *The politics of reconstruction: a study of the social and political consequences of the October 2005 earthquake in Pakistan and Azad Kashmir*. This assignment lasted for four years, between 2009 and 2012, and was a multinational (German, Swiss, Pakistan) research endeavour in which one Swiss and one German PhD scholar and four Pakistani MPhil students were engaged. Another project in which both of us collaborated was *Coping with change in Gilgit-Baltistan*, a project funded by the German Academic Exchange Ser-

vice (DAAD 2011–13). Ludwig-Maximilians University (LMU), Munich, National Institute of Pakistan Studies (NIPS) QAU, and Karakorum International University (KIU), Gilgit collaborated in this project and arranged research training workshops for students from Germany, NIPS and KIU, both in Islamabad and Gilgit. In addition, we organised a summer school in 2009 at the National Institute of Pakistan Studies (NIPS), Quaid-I-Azam University, Islamabad, in which about eight German students and an equal number from NIPS participated. This proved to be a very successful pursuit, judging from the fact that most of the German students subsequently did research for their MA thesis in Pakistan; some of them married Pakistanis and have even had children from these marriages (see Grieser et al. in this volume).

Other than that, I also collaborated with University of Tübingen. A group of six students and two professors (Stellrecht and Hardenberg) from Tübingen visited NIPS in 2008 and participated in joint seminars and short research exercises. I also visited Tübingen twice for one semester each as a visiting teacher. Similarly, two groups of students and teachers from Duisburg-Essen (Germany) and Morocco visited NIPS as part of the joint project *Peaceful change and violent conflicts: Middle East and the West-Muslim relations*. This was a three-year multinational project involving universities from Pakistan (NIPS), Germany (Duisburg-Essen), Morocco (Rabat) and Iran (Tehran), funded by the German Academic Exchange Service (DAAD). Several international seminars, workshops and summer schools were organised (two in Germany, two in Morocco, two in Islamabad and one in Tehran) as part of this project. A senior lecturer/researcher (Wolfgang-Peter Zingel) from the South Asia Institute of Heidelberg University, Germany, taught at NIPS (fully funded by DAAD) for two semesters (Spring Semester 2011 and Spring Semester 2016). Similarly, two senior German scholars (Andrea Fleschenberg and Sarah Holz) taught at NIPS, both fully funded by DAAD.

My multiple sides/roles in Gilgit-Baltistan

I now wish to describe the different roles I played during my research and collaboration in Gilgit-Baltistan, and through this example, I want to highlight the different meanings and roles of a local/native anthropologist versus people in the field and how it can lead to different kinds of suspicion. I have already written about the different roles and duties I performed in Gilgit-Baltistan, initially as a research assistant for a German team doing socio-cultural research, but mainly documenting rock-carvings and inscriptions. In between, I did three months of fieldwork for my Master's degree research in Chilas. I also worked as a translator (between Germans and local people), and I helped conduct interviews with local elders, administrators, etc. for German researchers. Furthermore, I facilitated negotiations between the local administration and the German team. For the people of Gilgit-Baltistan in general, and of Chilas in particular, I was part of the German mission, not only living and eating with them, but also as someone who knew their language and manners, i.e., I was very similar to them.

From another perspective, I was seen as being one of the local people: a Muslim, a Pakistani – a sort of native. This was expressed, for instance, when fixing wages for people who worked for the project or when negotiating the prices of whatever we bought in Chilas. I heard them when our team members from Chilas introduced me to a local person. Additionally, I was also a Punjabi from the lowlands, mostly local people called people like me 'from Pakistan', a negative identity, also for the Pakistani government, including armed forces present there i.e. a non-local exploiting them. This was especially significant when discussing politics, i.e., at election times or when local issues relating to development funds or foreign policy were discussed. Local people like the Chilasi were not all one and the same; naturally, there were differences. The local population was divided into *malik* (original inhabitants) and non-*malik* (migrants), and in this regard I was considered a non-*malik* sympathiser. When we went to Gilgit, or at times of conflict between Chilas (Sunni) and other parts of Gilgit Baltistan, especially Shia, the Chilasi considered me as one of their own. For instance, during an armed conflict between

Sunni (mainly Chilas and Kohistan) and Shia in and around Gilgit, they expected us, including German members of the team, to help them with resources and cars.

In Gilgit, the security forces, including security inspectors and some heads of administration, originated from the Pakistani Punjab. Local Gilgitis were also divided, in that some were happy that soldiers and secret agency people were there to prevent armed conflicts between Shias and Sunnis, whilst others thought they were partisans or actually themselves the reason for conflict. Security force personnel themselves expressed disappointment with the locals, because it was a difficult area for them due to not knowing the local language, the food was different and the area was difficult for them to live in.

International workshop in Gilgit

We travelled to Gilgit to participate in the second leg of the research-training workshop organised as part of a research collaboration between the National Institute of Pakistan Studies (NIPS), Quaid-I-Azam University Islamabad, the Karakorum International University (KIU) and the Institute of Anthropology, Ludwig-Maximilians University of Munich (LMU). I was the collaborating partner from NIPS, Martin Sökefeld represented LMU and Vice Chancellor herself represented KIU. A few days before the workshop, Martin was informed that KIU would not be hosting the workshop, even though it had been agreed a long time previously. The participants were students and faculty from KIU, NIPS and LMU. After the initial training workshop for students in qualitative research at KIU about a month beforehand, participants started to collect data in and around Gilgit on their own topics of interest. The second workshop would take place towards the end of the activity, after about four weeks. All of the students and the KIU faculty simply stopped communication, without giving any reason. Some faculty members who did not want to be identified told us privately that they were afraid of the VC and agencies. There were only rumours, and nobody, except perhaps the vice chancellor of KIU, really knew the true reason.

Martin Sökefeld had already met the top administration in Gilgit before my arrival, and both of us met the police chief in Gilgit, following which, and after clearance from both sides, we decided to hold a workshop at the hotel in which we were staying. Some officers from the intelligence agencies visited us in the hotel to ask questions about who would be participating and what topics the workshop would cover. I explained everything to them in Urdu, and they would leave – only to return again soon thereafter to repeat almost the same questions. On inquiry, they said it was their duty. The city police chief told us shortly before it was due to take place that he could not allow it to go ahead. The same message was relayed to the hotel manager, who was very angry because it meant losing a lot of business.

We met at the breakfast table the next morning and decided to visit German workshop students/participants residing in another hotel later that evening; in fact, they invited us for dinner. We also discussed the possibility of informally holding the meeting in that hotel or even somewhere out in the open. As soon as we reached the hotel that evening, police were already waiting for us at the gate. They stopped us and asked why we had gone there. It was surprising that they knew in minute detail what we had been discussing, and so we tried to guess who could have told them. The Deputy Superintendent of Police personally warned me of dire consequences if we did not stop everything, so we went back to our hotel and left Gilgit the next day.

The consequences of collaboration

Sökefeld and Strasser observed that “while in most cases threat to the researcher will be limited to the danger of being expelled from the field, [...], the participant may suffer much more existential consequences that include threats to his or her employment, freedom or even life. Sometimes, the researcher is only indirectly affected by surveillance and threats that directly target her or his partners” (2016:166). I remained under observation for the next few years.

I was a professor at the National Institute of Pakistan Studies and a collaboration partner in a project that included holding a conference. Four students from NIPS were also part of the research and the intended conference. After the first workshop, I travelled back to Islamabad to attend to some urgent university tasks, and I was accompanied by my wife and son on the return journey, in order to participate in the conference. A few days after my return from Gilgit, two officers from the secret agencies visited our Institute, met the director and inquired about me. The NIPS director informed me about this and warned me to be careful. A few days later, they visited again, and this time they interviewed me personally. There were rumours in the Institute that agencies were observing me in connection with alleged involvement in suspicious activities in Gilgit. Some people even warned me that I could lose my job or that I would not be promoted. I did not share any of this with my family, who were already very scared. I stopped almost all contact with Germans for the next several years, and many remain uncontacted to this day. This was the last time I would visit Gilgit for research.

The bits and pieces of information I could gather in the coming months and years included the alleged indecent behaviour of a female German PhD scholar doing fieldwork in Gilgit, which was said to be the catalyst for this fiasco. However, she was not present in Gilgit for the workshop. The KIU administration knew about her case when the workshop was being planned, and they agreed to collaborate. There were also rumours about one speaker invited to the workshop who was not acceptable to the agencies and administration, but nobody told us anything about this. Some of my students from Gilgit told me later that the secret agencies had asked VC KIU to distance herself from the workshop therefore, she has directed the faculty and students to keep away from the workshop. It is also important to mention that, as already alluded to herein, I have worked and collaborated with German researchers for many years, and except for perhaps indirect indications of surveillance, there has never been any direct or indirect threat to me.

Secret agencies are not alone in suspecting anthropologists of spying

Over many years of collaborating with German academics, especially anthropologists, I have learned that it is not only the state and its different ‘secret’ agencies that suspect anthropologists, but also local university staff, their Pakistani research counterparts and local people. Griesser (2016) observed the following about research in Gilgit: “Correspondingly, though most Gilgitis are fond of contacts with foreigners, many of my interlocutors appreciated the suspicion of intelligence agencies against foreigners” (Griesser 2016: 183).

Many different groups of local people (intelligence agencies, bureaucrats, academics and locals) may have very different reasons, including very personal ones, to suspect or sympathise with anthropologists. Before coming to why all these different people suspect anthropologists of spying, let me share two small experiences that may help us understand this issue. In the first case, DAAD agreed (actually, NIPS agreed) to grant two positions at NIPS. Initially, DAAD wanted to establish a branch in Pakistan and they needed a space that NIPS agreed to on my recommendation. Second, DAAD agreed to a proposal, (actually, an idea given by DAAD), to sanction a long-term professorship at NIPS. The faculty and director of NIPS, in private discussions, had doubts about both of them. NIPS always questioned their whereabouts (questions such as why they go to Peshawar, Lahore, Gilgit, etc.), why they contact certain students (especially why they would invite students from certain areas to their homes) and why certain topics had been chosen by students working under their supervision for their research. Some of these debates were held in my presence, others not, because I was partly held responsible for their presence.

The second case revolved around a teaching position offered to me by a university. In 1995, after completing my PhD at Heidelberg, I was about to return to Pakistan when a senior German professor contacted me and asked if I would be interested in becoming one of two teachers for the soon to be established Department of Anthropology at a university in Pakistan. Since I was desperately looking for a job, I agreed. Mar-

tin Sökefeld was to be the other teacher. I went to that city to meet Amjad¹, who was a research counterpart of the German professor and was supposed to head the department once established. According to plans, I was to be introduced to the Vice Chancellor of the university by Amjad. I stayed there for a couple of weeks without meeting the Vice Chancellor, but during my stay it transpired that Amjad was not convinced about the establishment of the department. He would often ask me questions. Why are Germans interested in establishing this department in Pakistan? Why here and not in Rawalpindi or Islamabad. Why do they want to invest money, even though no request has been made by the university?

Sceptical collaborators

A research project may come into being via many different routes in Pakistan. It may start after a meeting between representatives (minters) of two governments for cooperation, funds are allocated and the universities may be given the task of implementation. Sometimes, it is a result of collaboration between two universities, departments or professors in certain areas. It may sometimes come to fruition after a professor has won a research grant from a funding agency, or it may be an independent researcher intending to do fieldwork in Pakistan. In almost all cases of collaboration, however, funding originates from Western countries and the money goes to professors hailing from those countries.

A foreign anthropologist intending to do research requires an NOC to carry out fieldwork in Gilgit-Baltistan, and generally, the local university initiates/forwards an application in this regard. A memorandum of understanding (MOU) between two universities is helpful for securing a NOC. Individual researchers can also send applications for a research NOC directly to a Pakistan Embassy, for instance in Germany. The relevant Pakistani university, after approval has been issued by authorities,

1 Fictive name.

such as a dean, a vice-chancellor, etc., sends the application to the Ministry of Interior (MOI) for issuance of the NOC. The MOI then sends the application for clearance to different intelligence agencies. After an NOC has been sent out by the MOI, the Pakistan Embassy in Germany can issue a visa. After reaching Gilgit-Baltistan, the researcher has to report to the local administration, who can also demand a local letter of permission for research or may simply verbally instruct the researcher in terms of how they must conduct themselves.

We can divide the different people involved in issuing an NOC and conducting research into three groups:

- State administrative/functionaries: bureaucrats, secret agencies and police
- Dean, Vice Chancellor, collaboration partner, professors and students from the concerned university
- People living where the research will be conducted

The first group suspects anthropologists because it is their responsibility to keep an eye on foreigners in Pakistan, especially in a sensitive area like Gilgit-Baltistan. Bureaucrats from the Ministry of Interior are responsible for issuing NOCs after secret agencies and police give clearance to do so. The ethnographic methods of anthropologists make them an object of suspicion. Many people, especially the aforementioned authorities, do not know – or know very little – about anthropology, and so hanging around, interviewing people, making notes, etc., especially in a locale like Gilgit, which is known for communal conflicts, make them wary. University administrations, faculty and students sometimes may be jealous of colleagues involved in research projects, but they may also know the history of involvement of anthropology in CIA-funded research endeavours, for instance. Local people may be different from locale to locale. In Gilgit city, agencies may consider certain groups as potential accomplices of foreign anthropologists, whilst others may collaborate with agencies against the anthropologists.

A frequently asked question is, why do (German) anthropologists always travel to Gilgit for their research? The other question they raise is,

why do they spend money on this research if, ultimately, there is no direct gain for them? For instance, the people in Chilas, where I worked most of the time, wondered why a German would live in such a miserable place (e.g., spending hour upon hour in the hot field. 'Are they writing a book or trying to understand how people live?' 'Don't they have anything better to do?' 'There must be something they're not telling us'. They never trusted the research team. Even on my last time in Chilas they asked, 'What is all of this about?'

In Gilgit I felt a bit differently, especially when I accompanied my German team in the field and I talked about the local social or political situation. I often had the feeling that they did not like to talk openly and freely in my presence until they had convinced themselves that I was 'absolutely harmless'. I have already written that being a Pakistani from the lowland, a Punjabi, was a negative identity. In other words, the locals seemed to trust Germans more than a person with my identity. But then it also depended upon who the local person was – a Shia, a Sunni, an Ismaili, a Chilasi, etc.

Gilgit-Baltistan as a field of research

There are multiple reasons why anthropologists are shadowed by secret agencies in Gilgit-Baltistan. Before coming to these points, however, it is important to mention that anthropologists are not monitored in Gilgit-Baltistan only, even in Gilgit-Baltistan not all anthropologists are monitored², at least not monitored equally. Furthermore, the policy of monitoring has frequently undergone changes over time, owing to the deviations in political and regional circumstances. For instance, after the Raymon David case 2011 (a CIA agent who killed two men in Lahore, and whom the US government accepted as being a CIA agent working for them) and the Abbottabad incident (a CIA-led mission in which Osama

2 This was also observed by Griesser in Gilgit: 'One question that obviously, immediately arises from fieldwork and surveillance is why some are "singled out for suspicions" and others not' (Griesser 2016: 182).

Bin Laden was killed), suspicion of foreign or local anthropologists increased dramatically.

Gilgit-Baltistan as a research location is particularly sensitive because, historically, it has been a part of Jammu and Kashmir, a disputed territory between India and Pakistan. At least two wars have been fought between India and Pakistan on the “Kashmir issue,” and there is continuous unrest along the line of control, i.e., the temporary border. Besides India, Gilgit-Baltistan shares borders with Afghanistan, ex-Russian states of Central Asia and especially China, with a huge 3,000-km Chinese infrastructure network project (CPEC) for which Gilgit acts as a lifeline. Furthermore, Gilgit is also sensitive because a number of ethnic, religious and political groups reside there and are often at war with each other, especially during Muharram – the month of mourning for Shia Muslims. It is also very important for Pakistan due to its high mountain ranges and glaciers, which are the source of a permanent flow of water into major dams and along rivers. As a result, there is a significant military presence in the area.

Ethnographic methods that cause suspicion

The subject matter found in anthropology, and its research techniques, are another strong reason for suspecting anthropologists. The more popular topics in this regard include marginalised groups such as prostitutes, transgender, beggars, peripatetic, religious or ethnic minorities, political dissent groups, regions with insurgency, conflict, border areas and peripheral zones. Sökefeld and Strasser also shared this observation: ‘Anthropology is increasingly interested in all kinds of “security zones” such as border areas, laboratories, hospitals, refugee camps, prisons, and industrial plants’ (Sökefeld & Strasser 2016: 160), while Verdery observed that “[...] our methods are indeed in some ways strikingly similar to the practices of spies and agents, “simply hanging out with people” [...] are involved in a comprehensive data gathering operation that goes way beyond our formally-stated research questions[...]’ (2012: 17). The basic goal instilled in an anthropologist during their training is

'grasping native's point of view, his vision of his life' (Malinowski 1961: 25) by building rapport, gaining trust, being close to the natives and at the same time remaining unobtrusive. All of these factors offer sufficient grounds to arouse suspicion.

History of suspecting anthropologists of spying

According to Nancy Howell, 25 per cent of the social anthropologists included in her sample had to deal, at one time or another, with this suspicion, and in her view, spying is 'a difficult charge to defend against when one is there in search of information' (Howell 1990: 97). During colonial times, local people suspected anthropologists, as they considered them part of the effort to colonise. Evans-Pritchard's research among the Nuer is a good example at hand: 'Nuer are expert in sabotaging[...] [they] defy the most patient ethnologist to make headway against this kind of opposition. One is driven crazy by it' (Evans-Pritchard 1979: 13). After the end of the colonial period, local people suspected anthropologists for various other reasons, including being sent by their own state to monitor them, to impose taxes on them, to suppress uprisings, to pressurise a religious or an ethnic minority and to impose a law, depending on the region, the community and the state. Clifford Geertz's research in Bali (1973: 412–13), Lincoln Keiser's fieldwork in Kohistan (1991: 32) and above all Pnina Werbner's research of Ghamkol Sharif near Kohat Pakistan, where her ethnography was burned (2003: 193), are a few other examples.

On the other hand, the state may suspect anthropologists of being agents of a foreign power who are supporting an ethnic minority group against the state, a separatist political group striving for independence or a religious minority suspected by the state. In places where the state is weak (peripheral and border areas), or it is not liked by the local people, anthropologists may also make use of this to build rapport with their interlocutors.

Another aspect of monitoring not greatly discussed is the observing of the local anthropologist in a collaborative research team as well as local people working for anthropologists. The interlocutors of anthropol-

ogists and their local counterparts are found on both sides of the suspicion divide. I know of research collaborators who have spread rumours and stated, 'I do not understand why the government gives them permits to do research'. Going back to the previously discussed Gilgit conference that did not take place, we could not figure out who was spying for the police, but they knew exactly what we had been discussing. Grieser (2016) and Sökefeld and Strasser (2016) have written that their interlocutors had been followed and somewhat harassed by security agencies. On the other hand, they also expressed their fears that sometimes their informants were also perhaps agency informers.

Anthropologists as the accomplices of intelligence agencies

'Anthropologists have been active on both sides of the "surveillance divide" [...], in many cases anthropologists have been accomplices in intelligence work' (Sökefeld & Strasser 2016: 161). They have spied and done all kinds of clandestine research in service to their nation by cooperating with their governments and agencies up to and including the First World War. In 1919, Boas condemned anthropologists involved in spying by writing that they 'prostituted science by using it as a cover for their activities as spies'.³ His colleagues actually excoriated him, and the American Anthropological Association (AAA) censured him for pointing this out (Sökefeld & Strasser 2016: 161).

Similarly, during World War II, approximately half of America's anthropologists (including prominent members Ruth Benedict, Gregory Bateson, Clyde Kluckhohn and Margaret Mead) contributed to the war effort by working for the Office of Strategic Services (OSS), Army and Navy intelligence and the Office of War Information. The AAA also secretly collaborated with the CIA. In the early 1950s, its executive board negotiated a secret agreement with the CIA under which agency

3 Some information in this portion has been extracted from an article of David Price 'Anthropologists as Spies: Collaboration occurred in the past, and there is no professional bar to it today' published in *The Nation*, November 2, 2000.

personnel and computers were used to produce a cross-listed directory of AAA members, showing their geographical and linguistic areas of expertise along with summaries of research interests (Price 2000).

Due to the unresolved issue of anthropologists spying and after the 'Project Camelot' for which social scientists including anthropologists were recruited by the US military for doing research about counter-insurgency in Latin America, in 1971 the first draft of the Statement of Ethics (no secret research, first responsibility towards the people they study, etc.) for anthropologists was presented and adopted with a majority vote in an AAA meeting. Almost two decades later, during the Gulf War, proposals made by conservatives in the AAA, namely that its members should assist allied efforts against Iraq, provoked only minor opposition (Price 2000). In the words of Fluehr-Lobban, 'Ironically, just when anthropologists thought they had "decolonized" their discipline, anthropology is being called to the aid of an empire in crisis and decline' (2008: 18).

The clear condemnations of clandestine research mentioned in 1971 'the principles of professional responsibility (PPR)', was removed from the AAA's code of ethics in 1999. Nowadays, the stipulation 'no secret research, no secret reports or debriefings of any kind should be agreed to or given' has been replaced by the guideline that anthropologists are 'under no professional obligation to provide reports or debriefing of any kind to government officials or employees, unless they have individually and explicitly agreed to do so in the terms of employment'. After military and political failure in wars against Iraq and Afghanistan, the 'human terrain systems' project came into being, whereby anthropologists were 'embedded in military teams operating in both countries and the collaboration which never actually stopped has been revived on an unprecedented scale' (Sökefeld & Strasser 2016: 162). The AAA's Commission, formed to deliberate on the engagement of anthropologists, neither opposed nor encouraged engagement in its report (Fluehr-Lobban 2008: 18). The net result is that every anthropologist is a possible spy for his/her country or for her/his paymaster, i.e., what Boas predicted in 1919: 'In consequence of their acts, every nation will look with distrust

upon the visiting foreign investigator who wants to do honest work, suspecting sinister designs’.

Conclusion

Writing about the local perspective on the surveillance of fieldwork as a research collaborator with foreign (German) anthropologists, as “a sort of” native/local anthropologist, is like looking from the fence, i.e., a place where I can see, or at least try to see, both sides, if not many different sides. Due to our research collaborations, I experienced German anthropologists being shadowed and personally being observed, including my research career and perhaps even my life being. In this regard (a research partner educated in German universities, who knew German language and was a good friend of at least some of them) I saw it as unfair and that agencies were cruel or at the very least misguided. This surveillance impacted our research, foreigner or not, and we all had to navigate its pitfalls.

On the other side of the fence sat many different types of people, such as secret agents, administrators (of both universities and ministries), faculty members, university students and the people of Gilgit. Based on their particular position, they were divided in their opinions about surveillance. Secret agencies and administrators are responsible for the security of the country, the area and its people. In their view, there are enough reasons for them to suspect anthropologists in an area like Gilgit, which is a major trouble spot with a multitude of religious, ethnic and political issues. Foreigners generally – and anthropologists particularly – are suspected of being involved due to “questionable” behaviour and research methods. I have already said that university administration, faculty and students all had their own different reasons for being suspected, ranging from why a foreign country was spending money on training of our students and sending paid faculty members to Pakistani universities in order to open new departments. Jealousy over colleagues receiving payment, research funds and facilities and for being collaborators with foreigners was other reasons for creating

hurdles in the way of NOCs. Similarly, the people of Gilgit-Baltistan not only were sceptical of Punjabis, their own government and Germans, but they also sometimes sided with foreigners against their government and people from lowlands.

The question why NOCs were issued to them in the first place despite the general history of involvement of a significant number of anthropologists in spy activities, the sensitivity of Gilgit-Baltistan as a research area and serious credibility issues with some anthropologists. In my interaction with some intelligence officers, they confirmed such reports. In response to my question why an NOC had been issued, they stated that their duty was to keep an eye on the situation and to report anything back to their officers. The issuing or not of an NOC was the responsibility of the government. In response to a similar question put to a senior intelligence officer, I was told that surveillance operations also acted as a deterrent, and NOCs would be stopped only after hard evidence was found.

We further believe that issuing NOCs is part of a complex system of international relations, i.e., a network of economic, political and diplomatic relations – a sort of “postcolonial colonialism.” It is not merely a matter of choice for countries like Pakistan, or at least it is a choice only to a very limited extent. Even when very clear evidence has been presented, no or little action has been taken against citizens of strong countries. For example, US citizen Raymond Davis, a CIA agent, who shot dead two Pakistani men in 2011, was safely deported back to the US. The other strong incident that took place, based on spy activity, was the case of killing of Osama Bin Laden by US Navy SEALs a few months later. There were also discussions that some NGOs or individuals had been found involved in spying but they escaped any punitive action.

I believe that it is good that NOCs are issued to researchers, because not all anthropologists, not even the majority of them, are involved in spy activities. Anthropologists may and often do have different opinions and different points of view compared to local people, governments and agencies. Moreover, they also have these differences in opinion in their own countries and against their own national governments and national agencies. This does not make them spies, it is the genesis of

social sciences – and social sciences thrive on these differences. Continuity of anthropological research is not – and should not be – a matter of choice. It must continue, not only because developing countries are dependent on developed countries for resources, including for higher education, but also because most anthropologists bring important, constructive and critical perspectives to the debate on topics such as the marginalised, minorities, the oppressed, political opponents and, above, all the perspectives of local people.

Another often discussed issue with reference to surveillance is related to anthropological research methods. Martin Sökefeld has dealt with this topic quite extensively, and I agree with his and Strasser's view that 'After initial exploration we might come to the conclusion that we have to change our topic and/or site of research, [...]. Thus considerable flexibility and readiness to alter timings, sites and questions are significant elements for a methodology under surveillance' (2016: 167). However, I disagree with Sökefeld and Strasser when they say that '[...] research under surveillance will necessarily not be open. [...] we have to consider, [...] to whom we can disclose what. [...]. The use of a voice recorder is probably not advisable' (2016: 168).

I am of the opinion that agencies frequently suspect us because of misunderstandings, but behaving like suspects makes us even more suspect. We need to avoid suspicion by not hiding and instead by explaining our research. Anthropological methods should not be compromised, but if suspicions become serious, we should think carefully about changing the research topic and site. I am further of the opinion that the monitoring of anthropologists should continue, especially because it is a hurdle and a deterrent to spies with a military agenda. Surveillance is almost part and parcel of anthropological research, but it should not turn into harassment.

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Qurbani

Nearness and Altruism as an Engaged Ethnographic Practice

Usman Mahar

Prelude

The Urdu expression '*kisi tareefka mohtaj na hona*' is often rhetorically employed to articulate how someone does not need an introduction. What I find interesting is not that introduction often ensues this remark but the use of the word *tareef* (praise) instead of *taruf* (introduction). Thus, introducing someone is akin to praising someone, and this is how we often experience and carry out introductions in Pakistan. Often, praise as a practice of introduction is carried out to the point that it might even seem disingenuous to many in Germany. However, such a cynical reading of a Pakistani practice of praise perhaps fails to understand that some of us only introduce certain people and not others for a good reason.

Martin Sökefeld is indeed someone who does not need an introduction (*tareefke mohtaj nahi*), but some praise is nevertheless in order. In March 2019, Martin welcomed me to the Institute of Social and Cultural Anthropology at the University of Munich (LMU). I still remember how he introduced me to my new colleagues and the administrative workings of the institution I would be a part of for the coming years. He did so as the head of the department and, more importantly, as my PhD supervisor, my *Doktorvater* (doctoral father), as this relationship is known in the German-speaking world. In the following weeks and months, he patiently inducted me into my roles as a researcher, public

scholar and engaged anthropologist in my initial field site, the city of Munich. It would not be wrong to say that my subsequent relationship with him is simply captured by the affective split between two terms that signify the same formal role: the English term ‘PhD supervisor’ and the German equivalent *Doktorvater*. Often delicately but sometimes sharply, he highlighted to me the various aspects and sides of becoming an ethnographer. He nudged me not only to write and present my first conference paper, but also to engage in multiple other activities that were essential to my development as a public anthropologist. For instance, early on, he invited me to join demonstrations advocating for the right to work regardless of people’s residence status and to partake in public discussions on so-called ‘voluntary returns’ organised at Bellevue di Monaco¹ (henceforth Bellevue) – a cultural, political and residential centre in Munich that also serves as an important meeting space for activists. Thus, my initiation into the field of anthropology at the doctoral level was not only about collecting ethnographic data, but also about connecting with my research and field site in varied ways.

Indeed, ‘fieldwork’ and ‘ethnographic’ methods are sometimes used to denote the collection of a certain form of qualitative data, particularly by social scientists outside the field of anthropology. Sociopolitical engagement is often discouraged in this form of fieldwork, in order to achieve ‘objectivity’ in data collection. While there is nothing wrong with this cross-disciplinary methodology and practice, as James MacClancy argues, “it is rather different from (and frequently less than) the anthropological approach of intensive interaction with a particular group of people, including learning their language” (2019: 6). What has come to be known as an *engaged anthropological approach*, however, goes even beyond this and argues for a deeper and multifaceted sociopolitical commitment to one’s field and interlocutors (Ortner 2019). Such an approach advocates and practices anthropology that critically and affectively grapples with important issues of our time, often beyond the scope of the re-

1 Located in the heart of the city, it offers its space (three contiguous buildings) and resources for a range of sociopolitical projects and activist efforts geared towards a diverse and inclusive Munich (for refugees, migrants and citizens).

search. In other words, it is a move away from politically and emotionally detached anthropology towards involvement, activism and feeling in the field. The usual critique of this approach and its cognates is the alleged loss of objectivity – and hence not being ‘scientific’ enough. Sherry Ortner regards such fears as unfounded when she writes:

To take an engaged stance does not in any way conflict with an adherence to the principles of accuracy, evidentiary support, and truth which are the basis of any kind of scholarly or scientific work. The only difference is that the biases of work that does not define itself as engaged tend to be hidden, while the biases of engaged anthropology are declared upfront. (2019: NP)

Martin and I at a public discussion on the irregularised migration of Pakistanis (Bellevue di Monaco), Munich 2021



Screenshot from the Bellevue di Monaco YouTube channel

At a methodological level, Martin himself once pointed out to me how subjectivity is not the Achilles heel we sometimes portray it to be – in our reflections on ‘shortcomings’ in the methods section –but a strength we should unabashedly profess as a productive catalyst for any ethnographic endeavour.

Introduction: *Qurb\ani* as a kind of engagement and a side of the ethnographer

While this edited volume highlights many different aspects of being an ethnographer, in this chapter, I discuss the altruistic side of engaged ethnographers. In their 2010 article, SETHA Low and Sally Engle Merry take the “Statement to the Profession” that came out of a 1993 AAA panel as a point of departure to discuss ‘engagement’ in anthropology (cf. Forman 1995). They argue that “anthropologists are engaged in a variety of ways, but, as indicated by the history, they do not necessarily agree about what constitutes engagement much less about the form that it should take” (2010: 207). In their effort to come up with a typology, Low and Merry (2010) first called to attention six different forms of anthropological engagement: sharing and support, teaching and public education, social critique, collaboration, advocacy and activism. They assert that the aim is not to establish “a set of mutually exclusive categories” but “to describe the range” and underscore the diversity and overlapping in engagements (ibid: 204). Keeping this claim in mind, I try to further this diversity of engagement not only conceptually, but also in terms of how I, as a scholar from the so-called ‘Global South’, practice and affectively experience engagement.

In the remainder of the chapter, I explore and develop a type of anthropological engagement that overlaps and intersects with a few of Low and Merry’s categories by building on two personal examples from the field (ibid). I call this fluid type of engagement *qurb\ani* (*qurb* meaning closeness; *qurbani* meaning sacrifice) and do so for a few reasons. Let’s start with the most obvious path to understanding a new use of established terms: semantics. In terms of affective terminology, the Urdu word *qurbani* (literally sacrifice; broadly altruistic behaviour informed by locally adapted Muslim practices of charity, but not only) and its cognate *qurb* (meaning nearness; in affective terms, a means of approaching someone) are both important here. Omar Kasmani describes the latter (i.e., *qurb*) as a “feeling of closeness [...] always evolving, never stable feeling of being close in terms of time, place, or relation” (2022: 21). He suggests that *qurb*, due to “its visible, interiorized, spatial, and temporal

dimensions” of closeness, also implies being in reach “of understanding and conception” and, in that sense, is fundamentally different from *nazdik* the Urdu word for spatial or physical proximity (ibid).

I conjoin *qurb* and *qurbani* with a reverse slash on purpose, to borrow the simple idea in computer file management systems to indicate the ‘path’ taken. That is to say, the path to *qurbani* is through *qurb*. Through the concept of *qurb\ani*, a coalescing of *qurb* and *qurbani* in our field/work, I wish to achieve two congruent tasks: namely, dig deeper into specific kinds of affective and altruistic engagement we practice as anthropologists in our nearness to our field and interlocutors whilst simultaneously theorising the broader politics of such engagement. In other words, I wish to advance *qurb\ani* – a mode of affectively engaged altruism – as an ethnographic endeavour (methodology) whilst pitching it as a critical alternative to neoliberal forms of charity, ‘sacrificing’ one’s economic capital or ‘doing good’ more generally (politics). I see this as a befitting call in an era when (tech) billionaires are fuelling the so-called ‘Effective Altruism’ movement and other forms of neoliberal fixes to tackle the problems of inequality (Bajekal 2022). Sometimes simply referred to as ‘EA’, Effective Altruism is a neo-utilitarian social movement that argues for dispassioned charity. It encourages donating a certain portion of one’s income to projects that purportedly make the ‘biggest’ impact in terms of the value of money, usually in the Global South. In a sense, it is the opposite of *qurb\ani* as I frame it. Quite problematically, EA assumes a data-driven way of ‘giving up’ a portion of one’s income, and ‘doing good’ can be ‘objective’ and ‘emotionless’ when various private interests are involved. My challenge to this assumption rests on the premise that altruism is more than the sacrifice of money for ‘benevolence’, ‘humanitarianism’ or ‘doing good’ and that its often-hidden politics need to be brought to the fore (cf. Fassin 2012). While I throw down the gauntlet below and problematise global ‘humanitarian’ charities of all kinds trying to be ‘effective’, in short, my claim is that without *qurb* (nearness), there can be no *qurbani* (altruism; self-sacrifice). This is particularly relevant for movements that claim to have found an ‘effective’ and ‘objective’ mode of being altruistic and saving lives (cf. Fassin 2018).

Challenging neoliberal forms of charity that function under the rubric of ‘Effective Altruism’ is imperative, because not only do such seemingly apolitical approaches to doing good reproduce many of the inequalities they seek to address, but they are also based on a model not very different from the ones racist colonial powers used on their ‘civilising’ missions. Through their ‘effective’ approach to the distribution of resources, they justify diverting resources from ‘inefficient’ local organisations and governments to more ‘efficient’ international charities that often have their headquarters in the Global North (Acemoglu 2015). In this manner, they slowly chip away not only at a community’s capacity and skills to engage with local issues in the Global South, but also the value of public goods more generally. At an abstract political level, the proponents of EA are trying to veil the politics of charity with a facade of objectivism. However, we know that every measurement involves a value judgment, no matter how mathematical the process of measurement may be (ibid). Even an ‘objective’ number becomes subjective, since the story that we tell about it is just one of many, as a friend working as a biomedical engineer at ETH Zürich pointed out to me recently. As I later thought about this observation about the construction of objectivism, Nikolas Rose’s discussion on the notion of “style of thought” (Ludwik 1979) came to mind. Taking the example of biomedicine, Rose points to more fundamental issues of power in truth regimes:

A style of thought is a particular way of thinking, seeing, and practicing. It involves formulating statements that are only possible and intelligible within that way of thinking. Elements—terms, concepts, assertions, references relations—are organized into configurations of a certain form that counts as arguments and explanations. Phenomena are classified and sorted according to criteria of significance. Certain things are designated as evidence and gathered and used in certain ways. Subjects are chosen and recruited. Model systems are imagined and assembled. Machines are invented and later commodified to make measures and inscriptions such as graphs, charts, and tables [...] linked up with complex practical arrangements such as experiments and clinical trials. A style of thought also involves [...] intimate knowledge of [...] relations of power and status. [...] [It] is not just about a

certain form of explanation, about what *it is* to explain, it is also about what *there is* to explain. (2007: 12)

Such critique seems to miss the wealthy individuals and benevolent 'objective' minds of today who seem to mimic the white civilising mission of their predecessors. Like the previous eras of objectivism since its dark eugenic past, the 'science' of Effective Altruism relies on those with money and power, often rich (white) men in the Global North, to decide what cause is worth fighting for, what is the right mode of addressing the issue and who are the right people to carry out the actions needed (McGoey 2023).

In contrast, I propose a form of altruism steeped heavily in the affective and engaged turns: *qurb\ani*, a sacrifice that intrinsically relies upon nearness. Altruism, I argue (with my reliance on *qurb\ani*), has to be engaged and rely on collaborative forms of giving while tackling the structural causes of inequity, not only its symptoms. As I affectively envision *qurb\ani*, it stands for sacrifices that rely on nearness, which in turn informs our approach to sacrifices. That is to say, altruism should be socially and politically grounded in the contexts in which we seek to do good. Moreover, altruism must be engaged, or it will remain a handmaiden of capitalist extraction or, at best, memetic of mercantile civilisation missions of European colonialists (cf. Dalrymple 2019).

To illustrate my point, I first draw on the example of my participation in The Long Run, a grass-roots initiative that started as a small 'refugee running group' in Munich over a decade ago but has since grown into a community of diverse people who gather around many different activities, from sports to arts and cultural events. Subsequently, I draw on an example of a social entrepreneurship project in Lahore that I support in various ways. Towards the end, I juxtapose the practice and outcome of *qurb\ani* as 'engaged altruism' with 'Effective Altruism'.

My *qurb\ani* model is fundamentally built on practising it. I joined The Long Run soon after beginning my research with irregularised migrants in Munich, with whom I had just started my fieldwork at a return counselling centre and who had an *Ausreisepflicht* (obligation or rather order to leave). A few months prior to my 'following' people who returned

to Pakistan through so-called ‘voluntary return’ programmes, I saw my participation in the group as an opportunity to understand the pre-return lives of irregularised migrants (see Mahar 2020b, 2023). Engaging with the group, I thought, would allow me to meet them outside the return counselling setting (where I was sometimes seen as a part of the state-sanctioned return mechanism). Moreover, I would get to know migrants from different countries for a comparative understanding of irregularisation and (in)voluntary return (Mahar 2020a). Soon, however, I was engaged beyond that strategic aim for my *multi-sited* ethnographic research (Marcus 1995) with irregularised migrants and discovered my *multi-sidedness* as an ethnographer. Similarly, I got involved in a social entrepreneurship project in Pakistan, initially for the strategic aim of a comparative analysis regarding perceptions and aspirations of local (unskilled) youth in terms of migration and return. However, I soon realised my involvement exceeded methodological strategising, and my commitment to the project was social, political and altruistic in its nature. Upon submitting my PhD and receiving a Swiss salary, I also committed to financial support in both engagements (e.g., a friend and I donated €500 to The Long Run for a speaker system that was needed for our workouts, and a couple of hundred euros for training in Pakistan).

In both of these instances, I discovered that I was not solely engaged in ‘giving and sharing’, ‘collaboration’ and ‘activism’, to re-quote a few forms of engagement identified by Low and Merry, but much more – and affectively so (2010: 204). In addition to collaborative practices through which I provided my expertise to these two projects, there were opportunities to offer different forms of capital I had available, indeed even economic capital² (Bourdieu 1986). Since this asynchronous or indirect reciprocity is based on personal relationships built during fieldwork, mutual feelings of support, human intuition, collaborations, myriad sacrifices and long-term social and political engagements in the field, I call it *qurbāni* (one could say *engaged altruism*) and see it as different from Effective Altruism in the same way as ethnographic description and reflex-

2 Particularly upon the submission of my thesis and having more disposable income.

ive sociological analysis is different from descriptive statistics and objectifying theoretical models (Bourdieu and Wacquant 1992). However, what does this look like in practice? And, more importantly, in what ways does *qurb\ani* as an ethnographic practice go beyond the usually accepted forms of engaged research? Through my participation in the two aforementioned projects, I discuss *qurb\ani* as an ethnographic exercise more concretely, and I theoretically develop this side aspect of being an engaged ethnographer.

Practicing *qurb\ani* in Germany

The Long Run brings together citizens and non-citizens with diverse experiences and backgrounds around various cultural, social and sports activities. Soon after joining, in cooperation with Paul Huf, a founding member, I initiated a 30-minute workout session, which has now become part of the weekly schedule in addition to the jogs that I am responsible for organising, as well as a range of other activities. As such, the Long Run helps foster connections built upon interactions during sporting and cultural activities, thereby giving equal status to non-citizens, unlike more goal-oriented voluntary work that sees Germans and migrants or refugees enter the unequal power relation between the helper and the helped. These occasions provide opportunities whereby non-citizens are not dependent on citizens and engage in new forms of relationalities that can lead to “de-migranticization” (Dahinden 2016) at the affective level – in some cases, even providing non-citizens with the opportunity to help citizens, thereby reversing the usual roles (Safuta et al. 2022).

Ahmed, for instance, a former competitive martial artist in the Ugandan Tae Kwon Do team, trains those interested in the sport in addition to offering a course for children at Bellevue. This is not to say, however, that practical ‘skills’ are valued over other forms of participation. Different people find different ways to contribute; for example, Hafez engages the rest of us with poetry and enlightens European citizens about the literature and arts of the Middle East. He bridges affective distances

and differences with verses that connect the Syrian experience to that of German participants on some days and to that of Pakistanis like me on others. Through poetic giving, he often saves an evening that would be otherwise spoiled by the actions of a common antagonist, the *Ausländerbehörde* (Foreigners Office), in Munich. He often lightens the mood at the end of a long run when issues with German authorities are discussed. German citizens like Ursula, a scriptwriter, and long-term Munich residents from other parts of Europe, like Jon, a patent judge, have established friendships with non-citizens like Ahmed and Hafez. Thus, when people like Ursula and Jon support their non-European peers with wearisome issues of bureaucracy, language and the modes and codes particular to 'Bavarian' life, it is not with the aim of helping a 'poor migrant' but a friend, a fellow runner or trainer.

While I participate in many of the activities and invest my time in organising some, I also contribute as an anthropologist by helping group members critically reflect on what they do. I see this as *qurbāni*. Instead of only contributing to public and academic discourse, I find it valuable and important to engage the communities we do research with epistemologically and to share our knowledge with them. In other words, along with the established paths of disseminating knowledge through academic institutions and public discussion, I use my skills to communicate, transmit and circulate knowledge directly in a relevant community through engaged work – sometimes even at the cost of sacrificing academic knowledge generation (see Stephen Lyon in this volume). In that vein, I often discuss certain actions or 'help' offered by our European friends, at times quotidian modes of interaction that may reproduce unequal power relations. Certainly, it is always the case that based just on my physical appearance, I am assumed to be a refugee/asylum seeker by migrants and citizens alike. Often, new German volunteers or members of the journalistic community lose interest when I don't have a harrowing story to tell, which is often expected of non-white members of the group. Similarly, there are subtle problematic notions about 'the Muslim migrant' that effectively circulate. When possible, I gently take such observations to task and discuss them with the others. In doing so, we try to collaboratively identify ways to

strengthen and highlight the everyday and ‘mundane’ ways in which The Long Run can challenge the reproduction of unequal citizen-migrant relations.

There is no doubt about the fact that unequal power relations exist between citizens and non-citizens and that the nation-state system affords certain privileges to a group of humans considered to be at ‘home’ within a certain territory through the category of citizenship (Sharma 2020). However, in addition to struggling against such an exclusionary system in the long run, how can non-citizens (migrants, asylum seekers, refugees, etc.) be helped with their immediate needs in the short run? The Long Run, through different activities, tries to help members understand the artificial separation between natives and migrants, citizens and non-citizens at the affective, somatic and political levels while providing non-citizens with immediate and concrete support. Towards both these ends of subjective good, in the short term and the long term, I *spend* the various resources I have at my disposal and sacrifice others. This spending or investment is an important component of my theorisation of *qurb\ani*, as it distinguishes itself from the kind of investment and spending that takes place within EA communities.

Practicing *qurb\ani* in Pakistan

In 2019, during my fieldwork in Pakistan, I met Ali³, a migrant who had returned from Malaysia. At the time, Ali worked at a café in Lahore. Though not directly related to my research, which focuses on returns from Europe, we talked on several occasions about migration, return and the lack of opportunities for Pakistani youth. His story of migration and returning as a skilled barista were the bases of my interest in his views and opinions. Our love for speciality coffee and my student job as

3 Pseudonym.

a barista in Heidelberg helped strengthen our friendship further. It was a *sonay pe sohaga*, as the Urdu idiom for ‘icing on the cake’ goes⁴.

Ali shared how he had overcome the lack of capabilities as an unskilled youth with a rural peasant upbringing and changed the trajectory of his family. However, this was not enough for him, and he was interested in doing something for others. Thus, when he wanted to start a café with an important social dimension that resonated with me, I decided to help in every way possible. I employed all forms of capital at my disposal, by using my networks amongst privileged Pakistanis to not only collect funds, but also build social bridges for him. Due to our reliance on rent-seeking investors to finance the project, however, our vision of a cooperative had to be strategically – but only temporarily – shelved. We are still adamant about our vision for a cooperative that will give ownership to its employees and restrict the disparity between the maximum and minimum employee pay to a factor of six.

In 2022, just before leaving at the end of my second fieldwork stint there, I aided Ali in opening one of the first cafés of its kind in Lahore. Due to my support in getting the project off the ground, I am an unofficial ‘co-founder’. According to Ali, the project is as much mine as his, and while we do not have any legal agreements, I am proud to be a ‘co-founder’ in the symbolic sense. This perception was important for Ali for practical purposes, too. He felt that when I reached out to certain stakeholders or potential investors (e.g., my class fellows and friends from Aitchison College, a British-era private school), the response was quite different from what he would get (see Armytage 2020; Mahar 2021). Once I got involved, how he was treated within these circles was also quite different. In a sense, I became his gatekeeper to a rather closed fraternity which usually only conducts business within its network and often exploits people like Ali (see Mahar 2021).

4 The expression alludes to pouring a chemical on gold which helps dislodge its impurities and hence makes it shine immaculately but can be translated to English as “the cherry/icing on the cake.”

*Trainees learning how to taste espresso notes with Ali,
Lahore 2022*



Photo by Naheen Mahar

The principle of the café is simple. It serves locally roasted speciality coffee and simultaneously runs a barista traineeship programme for unskilled Pakistani youth. In addition to free barista training, covering training material costs (such as milk and coffee) and paying international certification fees, it pays a liveable monthly salary to trainees from the first day, much like an *Ausbildung* or vocational training programme in Germany (unlike in Germany, however, it is sadly a private and not a public endeavour, like many social projects in Pakistan). As the trainees progress out of training, they quickly find jobs in the hospitality market in Pakistan and various Gulf states with Ali's help, who now has several

of his students working there. The project can currently support six trainees at a time who retain employment until they find a job, following which a new trainee is hired. This rotational principle of the project, designed by Ali and me, does not please some of the investors and those providing the physical space for the café in exchange for profits. The rotation of the baristas and the repeated training of new ones impact the(ir) profitability and, in their opinion, should be discarded in favour of a traditional business model. For us, however, it is not a business but a small experiment, a microcosm that strives to garner a capabilities approach, in line with Amartya Sen (1999) and Mahbub ul Haq, the prominent heretic economist who first argued for the Human Development Index rather than national income (GDP) as a measurement of growth and development that was hitherto entrenched in 'western citadels of learning' such as Cambridge and Harvard (Mahbub ul Haq 1998: NP). Like in Germany, in Pakistan, too, I *spend* the various resources I have at my disposal on this subjective good and theorise this as *qurb\ani*.

Practicing *qurb\ani* as an anthropological alternative to Effective Altruism

As alluded to earlier, Effective Altruism (EA) is a philosophical and social movement that sees itself as 'doing good' in what its practitioners and funders consider the most effective and rational manner of spending unequally distributed wealth. The veiled assumption here is that a private model of wealth redistribution is more effective than a public model. According to Peter Singer's (1972) essay on the topic, which is widely considered to be the intellectual seed of the contemporary movement, let us consider the ethical and utilitarian premises of EA. In this seminal work on the ethical dimensions of altruism, he argues his point through an example of a little child drowning in a pond. His starting premise is that it is morally questionable not to help the child, even if that means ruining your clothes. In simple terms, his core thesis is that not sacrificing one's material belonging to save a life is immoral. That is nothing new,

but from here, he extends the argument into a more radical one by stating that the metaphorical ‘pond with a drowning child’ is the situation of many in the Global South, whilst the ‘expensive clothes’ are our luxuries in the Global North that we could easily sacrifice: not doing that, according to Singer, would be evil.

Let’s, for a moment, not engage with the idea that the supposed bystander with the duty to save is not only that, but also often part of the cause of the kind of suffering and inequality being discussed. Let’s also denote the paternalism of rendering people in the Global South as ‘drowning children’ and discuss Singer’s call for ‘effective’ altruism. The paper clearly outlines his ethical argument, but to understand his utilitarian contention, we must resort to one of his more recent examples, popularised through a TED Talk (Singer 2013). Singer poses a rhetorical moral dilemma for his audience: Assume you have 40,000 dollars that you can either donate to providing one guide dog for a blind person in the Global North or to a charity in the Global South that could cure the blindness of between 400 and 2,000 people with trachoma. In the end, Singer (*ibid*) concludes that the answer in such a scenario is clear regarding “what’s the better thing to do”. In his opinion, it would be irrational to help one blind person over hundreds, potentially thousands. In line with that thought, he urges his students to strive for lucrative careers in order to allocate a large portion of their income towards ‘effective’ altruism (of the EA community), in opposition to what I would call affective altruism.

Examples provided and the arguments put forth by other prominent proponents like Singer make three key assumptions about EA: 1) the irrelevance of nationality, ethnicity, distance, etc., 2) the amount of change or ‘good’ brought about is pertinent to altruism being effective and 3) consequences, not intentions, matter (cf. MacAskill 2016). Philosophically speaking, who could disagree with such a virtuous disposition towards doing good in the world?

Proponents of EA want to make the world “a better place” through “evidence-based practices” (Pincus-Roth 2020), which, they argue, can be done using data to make decisions rather than intuition and empathy, which they consider biased (cf. Bloom 2018; Alter 2023). However,

in practice, making altruism more effective by using our *heads*, not our *hearts*, and hence deciding how money can do the ‘most good’, is more complicated. This begs the question, how is the money being made in the first place, and how are the decisions to do the ‘most good’ doing good to the do-gooders? Let’s take a concrete example. The Effective Altruism community offers platforms such as “Give Well” and “80000 Hours” that help people decide how to donate their money and time in the most effective way (Effective Altruism 2022). Upon a closer look at the charities, one notices that many of them operate out of the Global North. In combination with its often young, white and male donation base, this poses pertinent questions about why the white and wealthy should be the ones deciding where and how money should be spent in a crudely unequal world created through their own practices (Ackermann 2022).

Another big EA blind spot is the lack of attention given to the structural causes of inequality that it wishes to address through ‘effective’ philanthropy or charity based on ‘hard science’ and data. In fact, many of the actions of ‘benevolent’ capitalists help perpetuate inequality by addressing symptoms thereof through private and neoliberal mechanisms— and not by addressing the root causes (Acemoglu 2015). Its supporters, often tech billionaires like the now-disgraced cryptocurrency tycoon Sam Bankman-Fried, profess to help suffering subjects worldwide ‘effectively’ but without questioning the system that enabled their absurd accumulation of wealth. Questioning structural inequality would perhaps be intrinsically problematic for a cause that solely argues for charity as a way to make the world a better place. Imagining a world that does not require the charity of the benevolent capitalist would lead to the redundancy of the movement. As McGoey (2023) argues, at times, the utilitarianism of some proponents goes well beyond sanity and circularly assumes that a rich person’s life is worth more simply because it has the potential to save more poor lives. He goes so far as to call EA one of the most problematic ideas of the century.

However, the conceit of the rich, and philanthropy as a way of deflecting criticisms of amassing unimaginable wealth, is not a new idea. A more recent notion is how such neoliberal approaches are being emulated in contemporary forms of ‘humanitarian’ government. Didier

Fassin (2012) shows through his critique of humanitarian reason that everything, from unwanted migrants to disaster victims and international conflicts to the misery of the poor, is currently managed through humanitarianism. He calls it a “paradigm of politics of compassion” (ibid: 1) and writes further down the monograph:

The suffering of the unemployed man, the refugee and the disaster victim is not simply the product of misfortune, it is also the manifestation of injustice. Humanitarian reason, by instituting the equivalence of lives and the equivalence of suffering, allows us to continue believing—contrary to the daily evidence of the realities that we encounter—in this concept of humanity which presupposes that all human beings are of equal value because they belong to one moral community. (ibid: 252)

If, as Fassin maintains, through humanitarian reason, injustice is contrived as misfortune at best wilfully ignored and at worst taken advantage of for political purposes, all whilst the benevolent can be relieved of “the burden of this unequal world”, then Effective Altruism is its unbridled form (ibid: 252). *Qurb\ani*, instead, relies on a form of giving that involves the recipient as a collaborator – not a recipient object of the benevolent mercy of capitalists (or ‘humanitarian’ (non)government officials) but an agent in deciding how those helping or giving should do so. Instead of running experiments on people to determine the greatest utility as per the donors, *qurb\ani* values collaboration and small-scale efforts to take immediate ameliorative measures but remain committed to changing the political or social dimensions that lead to the suffering or the issue in the first place. Perhaps in comparison to EA, *qurb\ani* is slow, inefficient, and, as the coffee project with Ali demonstrates, on a small scale. However, as I realised over the course of my engagement with the two projects, what they lack in utilitarian or ‘effective’ terms, they make up for in affective terms. Furthermore, what such a form of engagement supposedly loses in terms of ‘objectivity’, one may argue, is epistemologically less valuable compared to the access to complex and affective realities it allows the ethnographer.

On a frosty Thursday evening in January, two Afghan youths joined us on our weekly run, and as can be seen in the photo below, I was clearly happy about them being there. The winter running group shrinks to a fraction of its size in the summer – when the weather is more inviting – so two additional runners joining made me cheery (see photo below). The new runners were also newcomers who had just arrived in Germany about two months previously and were being supported by some of our members with legal and bureaucratic issues and other seemingly mundane but important aspects of adjusting to a new place.

*A weekly jog with some Long Run members, Munich
2023*



Photo by Paul Huf

*Along the River Isar at 3:42 am with The Long Run,
Munich 2022*

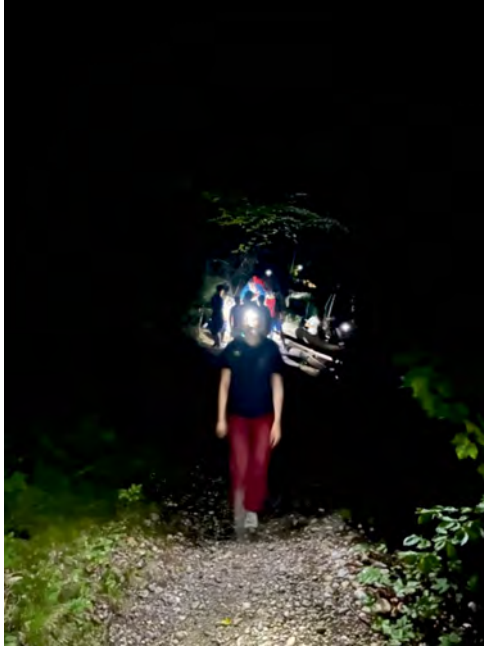


Photo by Usman Mahar

As we started our jog along the River Isar, I asked them if they liked to run. Their innocent answer made my heart sink as the younger of the two answered, “Yes, yes, run, run, from Afghanistan to Iran to Greek-istan [sic]”, while the older one continued to regale me with the rest of the route of their ‘run’ to Germany: “Now here, run, run, run”, the younger concluded with a joyous and triumphant laugh that made me smile. I often have such encounters while participating in The Long Run activities, and beyond openings into the stories of people, it is the sharing of somatic and affective situations that makes my experiences valuable and allow me to understand people’s everyday lives, which not only helps me

comprehend and write about their issues, but also engage with them as part of a community.

Let me explain through another example. Once a year, The Long Run organises a 32 km night walk called “Walk into the Light” to bring together migrants who need jobs and people who can potentially offer work. However, by partaking in this event as an ethnographer, I was not only walking-with, but also sharing and co-creating my affective, theoretical and practical reflections (see Sabine Strasser in this volume). For instance, how this exercise, amongst other activities of The Long Run, acquaints privileged members of society, European citizens and other advantaged members of society like me with somatic, psychological and affective states, which many of our irregularised non-European peers may have experienced on their long and arduous journeys to Europe. At the same time, for the non-Europeans, it symbolises the grit of European peers who are willing to walk along for as long as it takes.

Conclusion

As anthropologists, we often seek to give our interlocutors a voice through our ethnographies. We count this as an important aspect of reciprocating or ‘giving back’ for their time, patience and support. Engaged anthropologists go a step further and consider their social, political and activist involvement in the field and with their interlocutors as an ethical necessity (cf. Scheper-Hughes 1995). In that vein, I have discussed and theorised more concretely how we, as ethnographers, are uniquely positioned to turn our nearness into altruism when it comes to the communities with whom we work. I propose that we proudly and openly commit to how the many forms of nearness established in the field can be built upon through altruism as ethnographers. I call this process and its practice *qurbʿani*. In addition to being methodologically productive for our fieldwork and our ties in the field, it offers us insights into how to challenge problematic movements such as Effective Altruism that seek to establish what are ‘effective’ and ‘ethical’ ways of ‘doing good’. Here, I would like to lean on Martin’s recent commentary,

in which he unshrinkingly takes a similar approach to pose a challenge to formalisms around ethical clearances, maintaining that ethics in the field are a complex matter and are always “coupled with our political ideas and goals” (Sökefeld 2022: 523). Moreover, he convincingly argues that real ethical issues mostly arise in the field, and a formal ethical clearance meant to safeguard institutions can sometimes curtail the anthropologist’s freedom and flexibility to engage in solidarity with our interlocutors.

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Epilogue

From the Field, With Love

*Anna Grieser, Anna-Maria Walter, Jacqueline Wilk, Sohaib Bodla and
Clarissa Leopold*

Dear Martin,

As you are well aware, many of your German students fell in love in Pakistan, either during or after fieldwork, got married, had children. What do we make of such long-term entanglements, of blurring boundaries between the private and the professional? For your *Festschrift* on the multi-sided ethnographer, some of us got together to discuss our German-Pakistani marriages. But what a difficult task, in terms of both finding time in between shuffling kids, careers and married life and moving between two or more countries and then settling on what can and cannot be said! When considering writing about our relationships,¹ everyone's immediate reaction was that this topic would be the obvious choice, albeit it should not include anything private. So, what to discuss, if nothing personal?

Long-lasting relationships with partners from the field seem to have been unthinkable for most of the last century (Dubisch 1995). Plenty has been written on sexual relations in the field since the significant *Taboo* (Kulick & Wilson 1995; see also Goode 1990, Coffey 2018) was broken in the 1990s. The researcher couple is also not a new phenomenon (cf. the Bohannans, the Geertz, Mead & Bateson, the Rosaldos, Wikan & Barth,

1 Interestingly, it already poses an epistemological problem to label our marriages as binational, bicultural, inter-, etc. and evokes feelings of uneasiness in doing so.

and others; see Girke 2020), and the research project in which you conducted your own doctoral fieldwork in the 1990s also resulted in long-term relationships and marriages (between German project staff), including your own. And why not? After all, most people undertake their graduation and doctorate at an age at which people typically enter long-term relationships, if not marriage. Nevertheless, while the first debate zooms in on brief, sexual relations between researcher and interlocutor, the second one, on researcher couples, remains remarkably silent on the matter of intercultural relationships and almost exclusively presents the anthropologist as having a partner or a family 'at home', possibly bringing them to the field (see Burger and Burger in this volume) – a balancing act that certainly also applies to 'native' anthropologists.² Another established trope is the researcher's lasting relationships with key informants (see Lyon in this volume), with some even taken in as kin (cf. Häberlein 2020; Haug 2020) – a practice also quite common in Gilgit, where a local man adopted you as an *unilo poch* (milk or foster son) during your doctoral research. While these fictive kinships certainly entail social obligations and demand forms of solidarity, the anthropologist stays somewhat autonomous and moves 'in' and 'out' of the field – as well as between research objectives, interlocutors and research partners – without much concern.

But what happens when an anthropologist falls in love and marries someone from the field? Although we know of quite a few such – admittedly rather traditional, heteronormative – connections, conjugal relations with (former) interlocutors continue to be relatively underrepresented in the academic literature. In *Into the dark heart of ethnography: The lived ethics and inequality of intimate field relationships*, Katherine Irwin (2006) discusses her marital bond with a man who was also her key interlocutor and lays bare the issues attached to their affiliation. As Irwin argues, structural inequalities continue to exist, and while trying to overcome them, she and her husband possibly even reinforced them

2 Lately, reflections on parental duties during fieldwork have featured more prominently in the anthropological debate (cf. Braukmann et al. 2020; Cornet and Blumenfeld 2015).

through their relationship. In her essay *Anthropology that warms your heart: On being a bride in the field* (Cuba), Anna Cristina Pertierra (2007) points to the fact that it remains a constant and intense challenge to disentangle one's private and professional self. Is this blurred boundary the reason why so many of us struggle to admit the obvious absence of objectivity as well as the permanent struggle in carrying out the various expected social roles? Similarly, Michaela Haug (2020) describes how becoming part of an Indonesian kinship system brings shifting positionalities with it, from daughter, to daughter-in-law, to mother and, over time, also to grandmother – a rich entanglement that not only opens up possibilities for insights but also forecloses other contexts. Notably, most who publish works on their relationships are women, and though rich in detail, a broader comparative analysis remains to be done.

In our case, with more than four couples who established lifelong bonds by (getting married and/or) having children together after having met during fieldwork, the question of an emerging pattern arises. Ilva Ariëns and Ruud Strijp, as early as 1989, noted that anthropological fieldwork, like any professional environment, offers possibilities for amorous relationships. Of anthropologist couples asked to share their experiences, however, very few came back with a positive reply, but those who did so worked in the Middle East – the strong social and legal ostracism of intimate relationships in Muslim societies seems to demand scrutiny. So, here we are, after fieldwork in Pakistan. Let's now take the opportunity to take a closer look at our shared experiences.

Although our field trajectories were quite different, the most striking commonality attached to all of our relationships is that they went beyond the innocuous flirt or short-term sexual encounter and had serious, life-changing consequences, such as marriage, migration and children. However, this was often difficult for others, including family and colleagues, to accept, and even you, Martin, initially wrestled with the thought of perhaps having failed as a supervisor. Moreover, some of our families struggled with the geographic and cultural distance and the often seemingly quick decision to get married. When one after the other announced their intention to marry, one of the German fathers, for example, expressed his momentary resentment by suggesting that some-

one should “hang the *Doktorvater* (PhD supervisor) by the testicles” for having sent his students to Pakistan and not prevented such intimate entanglements.³ While marriage is the prerequisite for any kind of socially sanctioned romantic and sexual relationship in Pakistan, it is exactly the opposite in Germany, and so the idea of getting married without knowing each other for a couple of years, or having shared a daily routine, sounds somewhat suspect to most people socialised in Europe. Furthermore, in Pakistan, some of the future mothers-in-law had a not insignificant fear of relationship loyalty, be it because a “white girl” (*gori*) would not stay married for long, be it because she would eventually not enter into paradise with the (Muslim) family (even if she did turn out to be faithful to her husband after all).⁴

Why is the idea that ethnographic fieldwork can result in a partnership with someone from the area so difficult to accept – and why do a few anthropologists still deem it a professional failure? In times of ‘native’ anthropologists and anthropologists working ‘at home’, why is a relationship between an anthropologist and someone from ‘the field’ still labelled and sneered at by colleagues as “going native,” i.e., a form of abandoning the dubious distinction between Self and Other (Tedlock 1991; Sluka and Robben 2012)? In *The Vulnerable Observer*, Ruth Behar (2022 [1997]) puts her finger on the paradox within participant observation, namely that one is supposed to “get the ‘native point of view’ [...] without actually ‘going native’” (ibid: 5). She grapples with the question that the established manner of scientific objectivity renders us anthropologists vulnerable to criticisms of failing to apply the scientific criteria of verifiability and transparency, since all our ethnographic knowledge is based on highly biographical and subjective fieldwork moments. On the one hand, it is long understood that not only is the place of fieldwork highly situational and contextual, but so is the person of the fieldworker, which is why it is deemed imperative to reflect on one’s positionality as well as personal

3 This is a vivid example of the reactions of German family members, friends or ex-partners, revealing the persistence of patriarchal structures.

4 The issue of conversion (as well as applicable laws in the case of separation) remains one with which some families continue to struggle.

relations (cf. Carsten 2012; Haraway 1988; Okely 1996). Additionally, reinforced by feminist approaches, methodology syllabi nowadays increasingly promote the patchiness of ethnographic entanglements (see, for example, the website patchworkethnography.com). On the other hand, it still seems to be difficult to admit without reservation that the field-worker is not only a researcher, but also a human being and that no one is able to simply collect and analyse qualitative data without bias. As this edited volume illustrates, the boundaries between private and professional life tend to be fuzzy and blurred.

Notwithstanding this point, though, why is it that so many of us became entangled in serious relationships? In our discussions for this contribution, we discovered that the beginning of all our interactions involved extensive talking, both online and offline, in Pakistan, Germany and places in between – for hours upon hours. We discussed everything with our partners: politics, social norms, academics, poetry, music, emotions and more. Conducting ethnographic fieldwork in different parts of Pakistan left a serious mark on all of us; however, it was increasingly difficult to speak about and make sense of these experiences with people who had never been to these places or who were perhaps somehow resistant to academic curiosity.

Nevertheless, we continue to wonder to what extent our attraction to and the perception of our partners were affected by different exoticisms,⁵ hopes, dreams and interests. Some may have been interested in escaping the confinement of how things are done ordinarily, others drawn to share their privilege and help someone who was struggling. Having someone who could explain the subtleties and ambiguities of the field, or with whom to share the beauty of their cultural heritage, such as reciting Rumi and Bulleh Shah or discussing Marx and Nietzsche, might have been equally attractive. All of us have delighted in the feeling of transcending worlds and the excitement of explaining our backgrounds to each other with an infinite amount of content to explore and discuss. With this in mind, are our marriages purely personal, or were all of us “not only marrying the partner but marrying a culture” (as one of

5 Possibly, even orientalist biases influenced us to some extent.

us phrased it)? Ultimately, we (like to) think that it was our curiosity, excitement, deep exchange and endless conversation that made us fall in love with each other.⁶ The attempt to reconcile cultural differences, and the resulting ambivalences within our relationships, was however both a challenge to becoming a couple as well as a binding force, thereby adding to the already intense emotionality of a burgeoning relationship. Our marriages, then, were perhaps as much a result of the need to legalise them as of the thrill of doing something out-of-the-ordinary, of pushing normative frontiers. Being equipped with anthropological training, we felt able to transcend (perceived) cultural boundaries. ‘Us against the rest of the world’ feelings can apparently elevate a common infatuation to the sphere of the magical and thereby intensify the attraction. By entering a long-term partnership, we all chose to hold on to the intense experience of immersion in Pakistan – be it caused by adventurous travels, exciting fieldwork, the warm experience of family or the notion of deep, affectionate commitment.

The structural difficulties that mixed couples face dawned on us only after having sealed our commitments, and with time passing. Although we fulfilled legal (Islamic and German bureaucratic) expectations, we have learned that living as binational couples with diverse linguistic, socio-cultural and economic backgrounds can pose serious problems in relation to navigating different worlds, moving between places, juggling expectations and needs, bureaucratic requirements and social pressures. While Irwin (2006) writes that the structural inequalities of mixed couples become more attenuated “when marginalized men attempt to ride into middleclass [sic] worlds on women’s coat-tails” (ibid: 169), we argue that difficulties apply in any case. Borders, discrimination, adverse child custody rules and other factors always hit the one who happens to be the outsider, no matter where and no matter their passport, gender or class. All of us have struggled with the lengthy processes involved in obtaining and maintaining visas, family reunification or official documents for different countries. All of us were

6 To be fair, the illegitimate sexual encounters experienced by most of us also came with a certain thrill.

exposed to the intrusive scrutiny of our personal lives, compulsory visits from lawyers and security agencies and ongoing surveillance. To have a Pakistani marriage recognised in Germany, some were subjected to the pervasive suspicion of a sham marriage, and even after successfully resolving these issues, unpleasant and often racist encounters at the German *Ausländerbehörde* (Foreigners Office) were – and remain – a regular occurrence. The daily lives of those who have moved to the other country are affected by discrimination and feelings of isolation. For some of us it has taken years, whilst for others it continues to be a struggle to figure out where and how to live, to find a supportive community for our families and to secure employment for both partners.

Our intimate relationships have also forced us to confront our own stereotypes, unconscious biases and colonial baggage. No matter how genuine the intention and emotional involvement, such relationships raise questions about power and privilege, as well as the potential for perpetuating colonial dynamics. Many of us regularly engage in discussions about patriarchy and racism within our relationships. As white Europeans/European anthropologists, are we not automatically continuing a colonial project? As Pakistanis/Pakistani anthropologists, are we perpetuating the old power dynamics by engaging with Westerners? As couples, are we able and willing to fulfil the expectations set by ourselves, our families and societies? How do we navigate child-rearing and all the issues that arise with different pedagogical concepts and beliefs on what is best for our children? Is the pressure to successfully transcend cultural differences even greater in the light of our anthropological training? Or can the relationship between anthropologists and their partners be a way to disrupt and subvert traditional power dynamics and structural inequalities? After all, when anthropologists form relationships with individuals in the field, they establish a connection that extends beyond the immediate research focus.

Your own approach in the field, Martin, follows more established ways of building rapport and meaningful relations. As a foreigner in Pakistan, you are careful not to pass judgment or intervene in local ways of doing things, and thus you remain emotionally more detached and keep a ‘professional’ distance that perhaps appears to establish a some-

what more ‘objective’ stance (cf. Irwin 2006, Sluka and Robben 2012). Moreover, you engage with interlocutors in a very polite, authentic and empathic manner, yet you follow your own – male, German, academic, anthropologist – logics and ethics. Just imagine how your research persona, your contacts, worldviews and writings would have been taken over by family entanglements had you married there, too. What do you think – would you be able to render an even better and more intricate picture of this part of the world, or would it simply be a different, equally partial one?

“To throw one’s self into the field, *body and soul*,” as described by Irwin (2006: 157, emphasis in the original), has slowly gained momentum as a research methodology since the interpretive, postmodern, feminist, affective turns over the last decades (see also Jackson 2012; Pinto 2014; Stodulka et al. 2019). Instead of striving to remove the inevitable dimension of emotional subjectivity from our work and leave out private entanglements in the established manner of “quiet political correctness” (Varley 2008: 134), a holistic approach through which we use our whole selves, our bodily perceptions and emotions as instruments facilitates insight (Walter 2019). When research partners become life partners, the field and everyday life merge. A good example is Emma Varley’s (2008) very private and open account of her marriage into a Gilgiti family, which she uses to demonstrate how she evaluates protagonists through biased local prisms and personal (dis)regard. This holistic approach is not possible solely through wedlock, as the example of your good friend Monika Schneid, and her continuous engagement in girls’ education in a valley near Gilgit town, shows (also see Lyon’s chapter in this volume). However, marriage in the Pakistani context comes with quite all-encompassing social effects and seeps into any aspect of one’s perception as well as personal identity. While such strong positionality, i.e., being associated with a certain family, offers deep insights, it simultaneously excludes other avenues. And, most importantly, fieldwork is never just over.⁷ Consequently, these connections allow for a deeper understanding of socio-

7 This may, however, be increasingly true for many anthropologists who continue their field relations by means of electronic communication: for many anthro-

cultural dynamics and have the potential to disrupt, challenge and deconstruct (post-)colonial continuities.

We all recognise the value of long-term involvement in South Asia and the different facets we have been able to discover due to the various roles we have embodied over time. Just as we see in your commitment to Pakistan (and particularly Gilgit-Baltistan) over more than three decades, we continue to rotate back and forth between Germany, Pakistan and sometimes third countries, often pulled back by work arrangements, at other times by private interests. Even though these journeys have not always been easy or delightful, they have been rich experiences none of us would want to miss. Dear Martin, we want to take this opportunity to thank you for your indirect role as matchmaker – a position you might not have wished for or even imagined before, but one which you have grown to be enthusiastic about in the face of at least eight ‘grand-children’.

And when we read one of your most recent publications, we realised our experiences have also come to mirror in your teachings: In *Keep research ethics dirty!* (2022), you speak out against the growing praxis of obtaining ethical clearance through research boards before embarking on anthropological fieldwork. You believe, along with Didier Fassin, that “moral and ethical dimensions of human action are empirically and normatively impure” (2015: 177). For example, to fill a standardised questionnaire before going into the field would therefore equate to a “manual of confession to be filled in for the soul-searching of not yet committed fieldwork sins” (Sökefeld 2022: 522) and would also be counterintuitive, given the messiness of fieldwork. Ethics statements in anthropology (e.g., those of the Frankfurter Erklärung, AAA, ASA) are rather vague in this regard, but they do demand that researchers continuously reflect on their positionality and power dynamics with interlocutors and acquaintances in the field. Although there are no official guidelines advising against intimate relationships with someone in the field, given the overall level of ethical scrutiny, they do hint at the notion.

pologists the old dichotomy of being in the field and leaving it no longer applies (see the special section by Hughes and Walter 2021; Sluka and Robben 2012).

And despite all kinds of turns in anthropology, an informal moral code (still) tends to delegitimise relationships with partners from the field. Nevertheless, as illustrated in the various contributions provided for this edited volume, the boundaries between private and professional life are always fluid and blurred; fieldwork is as messy and unforeseeable as life itself, and yet it deserves careful examination. Such scrutiny may again remind us of the absence of objectivity and the unavoidable partiality of any ethnographic venture and product. While marriage in the field offers a deep understanding of certain aspects very likely denied to the standard visiting ethnographer, it nonetheless certainly prevents other insights. Moreover, the need to transcend cultural boundaries, stereotypes, biases and socioeconomic inequalities requires perpetual effort. Yet, the idea of doing something out-of-the-ordinary, of pushing normative frontiers through something as ordinary as a marriage, is quite fascinating. Consequently, writing about our German-Pakistani relationships seemed not only the obvious but also the necessary choice. Would a cautionary questionnaire really have prepared us against throwing the (post-)colonial understanding of *us* and *them* overboard?

With love from all of us.

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Appendix

Authors and Editors

Lisa Burger and **Tim Burger** studied social and cultural anthropology in Munich, where they continue to live with their children. Lisa has worked ethnographically on rationality and djinn conceptions in Morocco and has a keen interest in fieldwork methodology. Tim specialises in economic anthropology and has studied agriculture, state formation and value on the Azores and in Indonesia. He has recently defended his doctoral thesis at the University of Cambridge. Lisa and Tim are also editors of the book series “Edition Trickster”.

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Punjab and Germany on a diverse range of topics, including religion, politics, law, kinship, transgender and environment.

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Anna Grieser completed her Dr.Phil. in social and cultural anthropology under the supervision of Martin Sökefeld at LMU Munich with a dissertation on water management in Gilgit, Pakistan. Since then, she has worked on community-managed water and hydropower infrastructures in Gilgit-Baltistan for the Aga Khan Rural Support Programme and, as a

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Clarissa Leopold holds a Magister degree in anthropology from LMU Munich. Her M.A. research supervised by Martin Sökefeld centred on human rights activists and documentary filmmakers in Pakistan. Currently, she works as a Project Manager in international development, with a particular emphasis on the education sector and gender-related issues. Clarissa and her husband, whose paths first intertwined in Lahore twelve years ago, are now proud parents of two children. They currently reside in Cologne, and as a family they are engaged in a continuous journey to discover a place they can truly call ‘home’.

Stephen Lyon is a cultural anthropologist who has carried out ethnographic research in rural and urban Pakistan for more than 25 years. His research focuses on the intersection of cultural systems and political conflicts, and he has employed a variety of traditional and innovative methods for data production and analysis, including computer modelling and long-term participatory observation. Stephen is the author of numerous articles and books on kinship, computing and social or-

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Usman Mahar is a postdoctoral research fellow at the Center for Governance and Culture, University of St. Gallen. Under the tutelage of his *Doktorvater*, Martin Sökefeld, Usman fused critical border studies and affect theory in his PhD to shed light on the undeniable power of emotions, feelings, motivations, moods and other affective processes in the everyday lives of Pakistanis subjected to unequal mobility, irregularisation and removal. His current research delves into irregularised migrants' access to rights and services in Austria as part of the Horizon Europe-funded project "Protecting Irregular Migrants in Europe" (PRIME). He is a contributor to *The Other Side of Hope*, a UK-based literary magazine edited by refugees and migrants and "The Long Run," a Munich-based migrant solidarity group.

Beatrice Odierna is a PhD student at the Institute of Social and Cultural Anthropology, LMU Munich. After receiving a Master's degree in Middle Eastern diaspora studies (University of Edinburgh), she worked for about four years as a youth worker in different refugee accommodations. Drawing on experiences in the fields of Social Anthropology and Social Work, her PhD research focuses on "Processes of Subjectivation and Self-Formation of 'Young Female Refugees' in Germany". The PhD is part of a project funded by the German Research Foundation (DFG), for which she worked as a research associate between 2020 and 2023. Currently, Beatrice works as a research associate in palliative medicine research at the LMU University Hospital.

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Pascale Schild is a social anthropologist working on disaster government, the state, politics of reconstruction, resistance, political solidarity, peace and conflict and ethnographic vulnerability, with a regional focus on Kashmir and its transnational diaspora. Her research has appeared in *Citizenship Studies*, *Peacebuilding* and *Contemporary South Asia*, among other journals and edited volumes. Pascale completed her PhD at LMU Munich under the supervision of Martin Sökefeld. She is currently a visiting researcher at SOAS University of London and an associated researcher at the University of Bern. For her research on transnational peace initiatives and practices of solidarity with the Kashmiri freedom movement, she was awarded research grants from the Walter Benjamin Kolleg at the University of Bern and the Swiss National Science Foundation (SNSF).

Sabine Strasser is Professor of Social Anthropology at the University of Bern, Switzerland, and is particularly interested in migration, refugee and border studies as well as in feminist theory. Currently, she is co-editing the EASA book series with Annika Lems and Jelena Tošić. Recently, she co-edited (with David Loher and Georgeta Stoica) "The Politics of Precarity", which explores neoliberal academia under

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Magnus Treiber teaches anthropology at the Institute of Social and Cultural Anthropology at LMU Munich. He has a longstanding research focus on migration and political conflict in the Horn of Africa and has done extensive fieldwork in and beyond the region. He is most interested in the interrelation of ethnographic encounters and learning processes in broader political contexts, and their subsequent theoretical reflection. In this context, he recently published "Tentative lifeworlds in Art Deco: Young people's milieus in postwar Asmara, Eritrea, 2001–2005" (Journal of Eastern African Studies 2021) and together with Tricia Redeker Hepner "The Immediate, the Exceptional, and the Historical: Eritrean Migration and Research Since the 1960s" (Canadian Journal of African Studies 2021).

Anna-Maria Walter did her PhD research on the anthropology of emotions, gender relations and mobile phones in the high mountains of Gilgit, northern Pakistan under Martin Sökefeld's supervision at LMU Munich. Her monograph "Intimate Connections" was published by Rutgers in 2022. As a postdoctoral researcher for the University of Oulu, Anna-Maria has worked on conceptions of the self through social media use, digital anthropology and field methodologies, the socio-ecological

dimensions of Alpine ski touring, perceptions of mountain landscapes more broadly as well as indigenous knowledge of glaciers. She met her husband at the wedding of Jacqueline and Sohaib (two of the co-authors of this chapter). After years conducting a long-distance relationship, the couple first moved to England and are now settled in Munich, Germany, with their two children.

Jacqueline Wilk and **Sohaib Bodla** met each other at a dinner in Islamabad during their Master's researches. After obtaining her M.A. in socio-cultural anthropology from LMU Munich, Jacqueline started her professional career in international development, working with German political foundations in Pakistan. Currently, she is working with a social welfare organisation in Munich. Sohaib completed his M.Phil. in anthropology at QAU Islamabad and is currently pursuing his PhD on the topic of Political Subjectivity among Marxist Activists in Azad Jammu and Kashmir at LMU under the supervision of Martin Sökefeld. His research interests include military, nationalist movements and activism. After their wedding, Jacqueline and Sohaib lived in Pakistan for five years and are now settled in Germany with their two children. They talk a lot with each other but still struggle to communicate at times.

