Constructing the Field or Cementing It?

On Partnership, Method and the Hardening of Ethnography

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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 surround-ings, 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 pejora-tive 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 histor-ical contexts in which ethnographies come about has hardly dealt with

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1   See his YouTube channel: https://www.youtube.com/playlist?list=PL722949E70 B77BBFD

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 Blumenfield 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-

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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" ("Selbstauslegung 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 construcedness 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 objec-
tification," 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, point-
ing 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 practi-
cal" (ibid). Our point here is that an objectification or reification of a dif-
ferent 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 pre-
viously 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 tradi-
tional **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, intersub-
jective 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 episte- mological 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 cir- cumstances 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. Anthropol- ogists 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 proxim- ity 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 relation- ally 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-á-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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