Chapter 3
Methodology in context

This chapter will give an overview of the methodological steps I took to operationalize my research question. At its center is an account of how I constituted my field – "a crucial step in an empirical study" (Gupta & Ferguson, 1997), given that field work is at the heart of anthropology. First I had to define and limit my group of respondents in a context where refoulés, as introduced in Chapter 1, seemed to be "everywhere," as a consequence of the extensive, diverse, and long-lasting effects and experiences of deportations in Mali.

As a first step, therefore, I will describe how I gained access to the field, which constituted a distinct dialogical and rhetorical process (Lassiter, 2005, p. 15), in chronological order; as a second, I will outline the collection of data and the specific limitations to which this was subject, briefly sketching the places and cases that I researched during two field trips to Mali between 2014 and 2016 that lasted a total of eight months.[1] In addition, I will reveal the methods I used and the analytical steps I implemented to generate my results. This will involve showing how specific questions and main themes emerged. I chose to follow the general lines of grounded theory (Strauss & Corbin, 1990), which allowed me to openly generate knowledge from the ground. This I combined with an ethnography of conditions post deportation in Mali, including elements of participatory and action research. Ethnography, as an "undisciplined" approach (Breidenstein et al., 2013), gave me the freedom necessary to explore and develop deportees' narratives and (life) stories and to observe the practices that constituted their social worlds (cf. Lachenmann, 2010).

In all this, reflecting my own and others' positionality was vital, as "researchers and their subjects influence each other" (Strecker & La Tosky, 2013, p. 17) in such a way that there is no "beyond positionality." A certain immersion in people's lifeworlds turned out to be key for this research: I temporarily lived with deportees

1 Given this time frame, it is again necessary to highlight that the field and data presented date from before the last "wave" of humanitarian returns and *refoulements* from Libya, Algeria, and Morocco described in the previous chapter.

and their families and accompanied them during their daily lives. This went along with very specific power discrepancies, sometimes impossible to eliminate, which influenced how I did my research, which events, places, and cases I selected, how I did my analysis, and not least what I did not do. Critical reflection on how my positionality impacted my findings will thus figure prominently throughout this methodology chapter. I will conclude with the question of how conditions after deportation in a given time and place should be studied and describe the specific kind of ethnography that developed in the course of this research. In the end, however, this is an ethnography of a young white woman in southern Mali.

In the field: associations, activists, and deportations "everywhere"

This first phase of my field work, which took place between October and December 2014 mainly in Bamako, constituted an early stage in my project to research deportees' narratives, lifeworlds, and practices in Mali. It was supposed to provide me with an overview of the relevant actors and topics, to establish a contact network, and to start me off on data collection – mainly with deported migrant actors, but also with relevant institutions and NGOs. My aim was to enter the field by way of the AME and the ARACEM (*Association des Refoulés d'Afrique Central au Mali*), a self-organized group of Central African deportees, mostly from Cameroon. I set up my base in a transnational research center, where I could meet and talk with a number of Malian doctoral students. From there, still at some distance from the field, I started to reach out.

My primary aim was to conduct a multi-sited study,[2] my intention being to explore and capture the (social) field of post deportation in Mali in its varieties and forms in different localities. However, the protracted crisis in Mali and the issue of security impeded field work in northern areas, where deportees were reported to have been pushed back over borders or were stuck in transit (e.g., cf. Lecadet, 2013; Trauner & Deimel, 2013), thus fundamentally reshaping my field and my own capacity for mobility: the German Ministry of Foreign Affairs warned against "non-

2 The "multi-sited" approach emerged in the 1990s as a new way of doing ethnography. "This strategically situated ethnography [...] attempts to understand something broadly about the system in ethnographic terms as much as it does its local subjects: It is only local circumstantially, thus situating itself in a context or field quite differently than does other single-site ethnography" (Marcus, 1995, pp. 110f). Ghassan Hage, on the other hand, demonstrated the exhaustion that can result from multi-sited research. For him, following the global transnational Lebanese community all around the world was simply physically unbearable (Hage, 2005, p. 466f).

urgent travel,"[3] including to Bamako, specifically prohibiting access to certain areas. Eventually, I conducted an ethnography that included several localities in rural as well as urban Mali (see the map below). In so doing, I first wanted to find out the potential differences between places in the rural hinterland and the city; and, second, to center on individuals' stories and their specific social embedding. As I have said, rural places of return in particular had barely been researched so far. In the end, what I wanted to do was to grasp the phenomenon through a heuristic case-by-case approach. This kind of development of my field created what Ruben Andersson (2014) would call an "extended field site" of situations post deportation in Mali, produced by transnational and global political interventions and constituted through the activities of associations and NGOs and, above all, by the presence, actions, mobility, and immobility of former deportees, (re-)emigrants and their close social contacts. The "Extended Case Method" of the Manchester School (cf. Gluckman, 1963) that this approach builds on thus allowed groups that previously had been considered separate to be brought together in an analytical conversation that reached well beyond the confines of the geographically bounded villages, which were the main object of anthropology back then.[4] Meanwhile, the security aspect kept impacting my field work.[5]

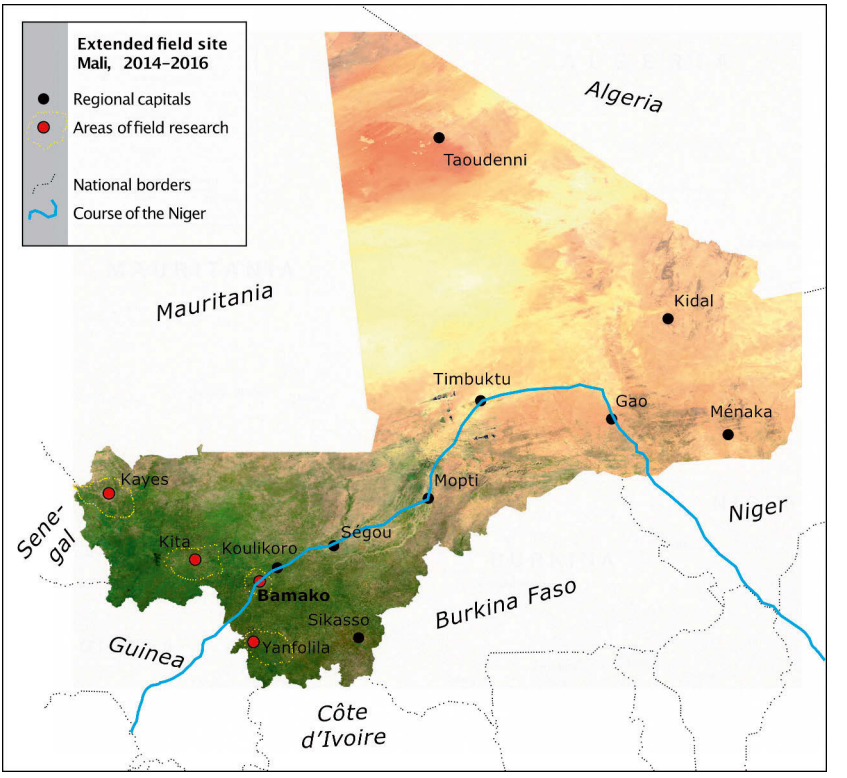
I first set foot in the post-deportation world through the AME's office, after sending emails from Germany (which remained unanswered) and eventually talking with their spokesperson on the phone a couple of days after my arrival. Everybody was preparing to travel to the African social forum in Dakar the next day. The small rooms were crowded and packed with pictures and posters for international and local events, the "*théâtre des expulsés*" (Canut & Sow, 2014; Lecadet, 2011), the Bamako–Dakar caravan taking people to the world social forum, and visits to France and Germany, as well as pictures of funding partners and other researchers. Toumani, the person responsible for receiving and assisting deportees, welcomed me cordially. I was soon taken to the Association's meeting room and introduced

3 Travel to the North would have been possible in UN company only, constraining an open ethnographic approach (that did not have the UN mission itself as its subject). Another trip to the Mauritanian border never became a reality either, as it as was simply too dangerous for me, as a white person, to be exposed to the traffic of every imaginable kind passing along this swath of land.

4 Ruben Andersson builds, among others, on the "extended case method" in defining his research into the European border industry (2014, pp. 283ff). In its original sense, the method was all about selecting a single conflictive event in a small social environment to observe over a long period of time.

5 With the attack on the Radisson hotel in Bamako in November 2015 in reaction to the Paris bombings, white people became an obvious target, which was also reflected in the anxieties of my Malian companions on my behalf. See online: https://www.theguardian.com/world/2015/nov/20/mali-attack-highlights-global-spread-extremist-violence, accessed 31 October 2021.

like one of their usual stagiaires (interns); a balcony led out onto an animated street in the neighborhood. The organization had a vast number of shelves full of deportee dossiers. My recording device was running almost constantly; I sat and listened, asked questions, and took notes. The former deportees had become "professional" in various senses (cf. Andersson, 2014, p. 258), and routinized in receiving international guests. The spokesperson even claimed to know me. This built some common ground: I was instantly involved in the organization's wide network.



Toumani soon agreed to provide me with the contact details of individual deportees. "Yes, yes, yes, of course! There are always people like that here." And, indeed, after a very short time a young man, Lamine, was standing in the doorway, timid, but friendly. Deported from France two days before, he had been referred to the organization at the airport needing support with his "false" *laisser-passer*. It was his second visit to the NGO. He appeared disturbed and confused. We were introduced and were able to sit next door and talk. He wanted to go back "right away!" While we were still in conversation, another young man entered the organization's

office space. A staff member had called him in to have a chat with me. This man had been deported from Spain and Libya previously, and from a multiplicity of other places at other times, as I shortly learned. Now, he had been back for two years. In no time I found myself in the middle of my research topic; it was going almost too fast for me to follow. There was a "restless" pulse, I could sense, accompanied and contrasted by a kind of stasis. It would become constitutive of my field work.

Similar things had been reported by studies on migrants in transit (for more on this subject, see Chapter 4). When two days later the organization presented me with a "selection of representative deportees" – people with characteristic as well as salient trajectories, as they described them – as potential interviewees, I became hesitant. I was not after a ready-made post deportation reality or the image that the NGO wanted to present of itself; nor did I want to replicate what others had been doing. I had used the same entry points as others, but apparently, I had to go beyond the organization's realms in order to gain a less filtered view.

ARACEM, mentioned above, was my second point of entry to the field in Bamako. Unlike the AME, their working world was quiet; the spacious patio of the organization's building was almost empty. A broken, upside down chair in the corner, and a set of slightly faded housing regulations marked the change from busier times in the past, a reminiscence of former deportees from the days when the routes to the north were open.[6] One of the ARACEM's Cameroonian coworkers, Dave, a recent deportee from Morocco, was an excellent guide to the world of Cameroonian transit migrants and those dumped back in Mali from the routes to Algeria, Morocco, or Libya. Many of them lived in an empty garage, which they called their "*ghetto*," located in a run-down market "*Halle de Bamako*," just next to the ARACEM office and the central bus station "*Auto Gare de Sogoniko*" (with buses to and from the north – cf. also Lecadet, 2013): most of them were somehow trying to eke out a living through little daily jobs. They were strangers, much more so than Malian dportees (see also Chapter 4), who needed to survive and were trying to continue on their way north as soon as possible. Entering their transit space in Bamako allowed me to better shape the field by delimiting the lifeworlds of the Malian deportees from theirs. From this base, still in October 2014, I started my data collection process.

Shifting positionalities in action research among associations and activists

The participatory research I started from within the organizations, contained elements of action research. All of the organizations' staff had previously been de-

6 Rooms were located around the patio: one dormitory for men, one for women. The other entrance led to the office spaces of the association, consisting of two rooms and a small bathroom.

ported and were now engaged in caring for others arriving in a similarly miserable situation, providing orientation and support and engaging with them politically as well. As far as I was concerned, getting involved with the organizations implied acknowledging, and up to a certain point going along with, their reality as the "what was there" in terms of representing and shaping conditions post deportation in Mali.[7] At the same time, it meant being a researcher, activist, and "*stagière*" – being able in the latter capacity to pass my time on the organizations' premises and take notes, but also to report on my activities, when I was out in the field, and, above all, to conduct interviews with state and international officials.

My supposed belonging to several associations was a sensitive issue. On the one hand, taking different positionalities and building relations with a variety of actors and organizations meant gaining different access to and different perspectives on my subject matter; on the other, it required juggling my loyalties. Collaborating with a white European researcher implied potential benefits and advantages – in the civil society sector funds and donor relations provided legitimacy and enabled survival at the same time (cf. Sylla & Schultz, 2020).[8] Critically, the AME and ARACEM were partially funded by the same donor. After causing some irritation to the organizations by belonging to both, I increasingly came to exchange information between them or pass on greetings from the one to the other. Throughout my entire fieldwork I reported on my activities when I entered their premises. This was, not least, about building trust relations. While the AME was nationally and transnationally quite well established (cf. Lecadet, 2011), it remained a small association with a handful of staff and a three-room office.[9] Since then, the situation has changed substantially in the aftermath of the "refugee crisis," creating another form of competition and reshaping these organizations as described in the previous chapter.[10]

More than that, I participated in organizing events in a network among various African, mainly Malian, and European activists. There were significant overlaps and interrelations among the self-organized groups, which aroused internal conflict and competition, accompanied by gossip and mistrust – it seemed a bit of

7 For a recent discussion of using a "returnee identity" for political recognition in Benin City, Nigeria, see Shaidrova (forthcoming).

8 Lack of funds and an often existential dependency on donors from the global North have created uneven relationships and major dilemmas that have been thoroughly discussed as the pitfalls and downsides of development aid (see, e.g., Macamo & Neubert, 2012).

9 In 2016 the organizations were in a phase of strategic reorientation. Central points were: "want[ing] to be ourselves…," but nevertheless following a "strategy of moving together" (field notes, 03-1-2016).

10 ARACEM has become much more involved in humanizing the external borders of the EU since the "refugee crisis," being confronted with a massive new increase in deportations, above all in the form of transit and humanitarian returns.

a snake pit sometimes. In particular, the craving for funds and for relations with promising prospects created serious ruptures and embezzlement on a large scale. Outrageous rumors were circulated, and nasty speculations aired in order to belittle other parties or to create new or reinforce old loyalties. I tried to stay out of these conflicts and later withdrew from my activist engagements, as things became complicated. Moreover, my focus was on the *refoulés* themselves.

It was essential to differentiate between the points of view of activists and organizations and my own. Susann Huschke's term, "activist's blinders" (2015, pp. 61f) fits this well: it points to a certain unavoidability, or at least the danger, of seeing things from an activist perspective when intensely engaging in such circles. At the same time, this kind of immersion – one could describe it as "going native" up to a point – is part of a "usual" ethnographic approach of "cultural inclination, adaptation, transformation" as well (cf. Lachenmann, 2010, p. 10, transl. S.U.S.), and has to be constantly counterbalanced by a sufficient degree of alienation. More than that, actively engaging in one's field and doing ethnography for purposes beyond those of publishing has become self-evidently part of activist or action research (e.g., Reason & Bradbury-Huang, 2012). In meetings with (Malian) administrative bodies, for instance, remarkable scenes occurred as well. Different groups of people, either directly or in more subtle ways, expressed their need for and expectations of additional funds and projects from Germany or contacts with people there. In the end, all of us – the organizations, the *stagiaires*, the researchers, the deportees, the politicians, the journalists, and the activists – formed part of the absurd "bordering Europe" industry that Ruben Andersson (2014) aptly describes, and of the deportations industry in particular. Nevertheless, we took different roles and perspectives, which I had to differentiate and thoroughly reflect in order to document and later analyze my data in as balanced a way as possible. Thus, my involvement required caution in specific areas, which led to a quasi-never-ending negotiation that at the same time defined my field (cf. also Breidenstein et al., 2013).

Accessing and researching with former deportees

To encounter deportees' lifeworlds in a more unfiltered way and to understand how they coped with and were impacted by having been forcefully returned, I had to take more independent steps. Increasingly, I kept looking – sometimes proactively, sometimes passively – for relevant contacts and "potential deportees," broadly following the idea of a snowball system (Atkinson & Flint, 2001). In Mali (post-)deportations seemed to be everywhere, pace Collyer (2018, p. 111), who states that research on deportations faces major issues as returnees are hard to identify among the larger population. Beyond the organizations and arranged meetings with institutional contacts and others, my entry points were random conversations with

other researchers, friends of friends, the security guard and the shop owner next door, taxi drivers, and more and more with people in the streets.

Brahima, for instance, one of the main respondents of this study, I met accidentally at Bamako's *Grand Marché* in 2014 when buying credit for my mobile phone at a tiny, lopsided stand. The vendor, responding to a request from me about my research, spoke up loudly enough to drown out the high-volume activity of a busy road crammed with cars and people: "Ah, we have a Spaniard here! – Brahima!!" A somewhat shy, but friendly-looking young man appeared. Quickly, Brahima and I made contact, exchanged a few words, and I came back for an in-depth interview a couple of days later. Several visits and conversations followed over the next one and a half years and after that occasionally via telephone. Others showed me their passports on similar occasions; some told me about their brothers, friends, or acquaintances who had been deported or had potentially left again: "I have been deported too! I've been in Spain/France/Morocco …" was frequently the starting point for such random encounters.

Most of them were doing little jobs in the market or at the side of a street, like selling mattresses, dates, or telephone cards – getting by as best they could. Some had returned a long time ago and were now doing better – or doing even worse; others had returned recently. The latter seemed as if they were in an immense tension between restless "hyper mobility and waiting" (Streiff-Fénart & Poutignat, 2006, p. 137; transl. S.U.S.). Some expressed a craving to leave again, come hell or high water, and accompanied this with a gesture of pointing to their forehead – "you know I have something in my head, I have to go on" was an emblematic expression that I will further develop in the next chapter.

Following returnees, I did my "urban" research in several districts of Bamako frequented by deportees and the people socially close to them. Besides the premises of institutions, these were the offices of the self-organized groups of former deportees, the "*Halle de Bamako*" market, the *Grand Marché* in the city center, the youth center (*Maison des Jeunes*), a number of returnees' workplaces, restaurants and cafés, as well as the private houses of deportees' families, often their uncles or cousins as their parents were still in the village.

The following box provides a summary of the characteristics of my interviewees and a first overview of their backgrounds and the main patterns of their lives. (Differences between the urban and the rural contexts will be further detailed throughout the book.)

> The large majority of interviewees were male. Their ages ranged between 20 and about 50. In Bamako the majority of them were aged between 25 and 40, though in some villages there were also quite a number of older deportees in their fifties. Those I interviewed in Bamako had mostly been deported from European countries,

but some from Morocco and Libya as well. All of them originally came from rural areas, from North, South, or mostly Southwest Mali.
Those I met in rural villages (see below) had mostly been deported from Libya, Mauritania, Morocco, or Algeria, but some from European countries too. (Furthermore, there were deportees from Gabon, Guinea Conakry, the Central African Republic, the Congos, etc., whom I engaged with as additional respondents mostly.)
The level of education of the people I spoke with varied from illiterate to holding a high school diploma, with a tendency toward the lower levels of education. Particularly among those who had returned to village sites, the level of education was relatively low. Most respondents were, or felt, constrained in their capacity for mobility. Differences considered significant related to migratory and deportation experiences, whether one had earned money or not, and most notably to one's age and generation. Still, all of them considered themselves to be refoulés, even if they had experienced different journeys and deportations or repatriations (by land, sea, air, or even via humanitarian evacuation from Libya 2011–2014, as covered in the introductory chapter).
Those living in the village were mainly married – most, though by no means all, since their (last) deportation. Among those living in the city, there was more diversity in terms of family status as well: some were married, but the wife usually lived in their father in law's village; others were not (yet) married.

Certainly, there were considerable structural inequalities and asymmetries, since I came from a very different position, being white, young, highly educated and thus supposedly privileged, mobile, European and a woman. Susan Coutin speaks of "irreconcilable incompatibilities" in studying deportations from a "white" researcher's point of view.[11] Even if I principally agree with that, being a young white woman at the same time spurred me on to discover former deportees and relevant contacts. More than that, I met a lot of openness. It seemed that encounters between deportees and myself provided them with a certain relief. Contrary to what one might assume, people often had difficulty in speaking about a traumatic or shameful past. While some reported their nagging memories very frankly to me, they also indicated their social alienation and estrangement through comments such as: "I've not forgotten. Often, thinking about it makes me want to stay by myself. I stay away from the others." Accounts might remain indirect; things were kept silent about, or took longer to come out, in order to retain face and some respect.[12]

11 Ines Hasselberg even warns of a "pornography of suffering" (2018, p. 32) in researching excluded, abject, traumatized subjects, which is what deportees may be.

12 I will discuss the role of silence after deportations in Chapter 5.

In other cases, ironical or bitter laughter about arbitrary deportation practices, for example, that randomly distribute deportees throughout West Africa, could alleviate stress by incorporating deportation experiences into everyday life. Some shouted their anger and frustration about experiences of exclusion and administrative abuse, embodying a feeling of unjust treatment and betrayal. Others presented themselves as isolated or victimized but saved their most vehement objections for experiences of criminalization. For me, it was sometimes difficult to stay calm and "endure" in such conversations, to accept people's lamenting and suffering without interfering and reminding them of the structural effects of their situation. Such interventions could, however, clearly influence the course of our conversation and my findings. At the same time, our conversational interactions need to be seen in terms of the narrative styles and self-construction of the deportees, without discrediting the suffering they experienced (see Chapter 4). There were moments when I wondered if people made up their stories. How could deportations be literally "everywhere"? People repeatedly telling me that they would prefer to travel and live in Germany were an obvious indicator of discursive, somehow opportunistic adaptations of content and narrative strategy.

The openness and the reserve equally possible in research encounters need to be seen in relation to the "common ground" (cf. Drotbohm, 2012) that emerges between the researcher and deportee through a connecting international experience: my interlocutors "had been there" or at least had been "on the way," while I came "from there" and was with them "here" now. My co-researcher Birama Bagayogo (2019)[13] reasoned that, as I was an outsider, there was nothing to hide. Engaging with a European woman and a Malian man from the capital – as was the case in our collaborative research – seemed to diminish an often-widespread competition between people. There was less danger of the gossiping and mistrust which often occurs in narrow social spaces like village sites, or in city neighborhoods, or among self-organized groups and activists of the kind described above. Moreover, this feeds into what has been discussed as the positive effect of being a researcher from outside the field (Diawara, 1985), and the researcher's (the anthropologist's) position as a "stranger," uninhibited by social convention and commitment (Simmel, [1908] 1958); cf. Jónsson, 2007, p. 16). It might, moreover, lend weight to the observation that former deportees want to share their stories (Lecadet, 2011, p. 17ff).

Despite being a woman, I rarely met female deportees – only in rare cases through the organizations. I was also told about some. Most deportee women had been returned together with their husbands, particularly from Libya. In addition

13 Birama Bagayogo was a 33-year-old Malian anthropologist, with whom I collaborated starting in summer 2015, and who assisted me on numerous occasions during my second field phase, October 2015 until March 2016. I will detail this collaboration in the next section.

to the fact that there is less (independent) international female migration, women seem to be deported less often than men and female deportees are less publicly visible. That may relate to the fact that female migration has long been viewed ambiguously. Women's international migration in particular may still be suspected of having a possible connection to sex work or the development of new sexual attitudes, both of which are easily stigmatized (Lesclingand & Hertrich, 2017, p. 66).

I will briefly describe one incident, as it demonstrates how positionality imbalances can intersect in research with deportees, influencing people's lifeworlds to some degree and research findings most of all. A former deportee from Belgium, whom I had known from the beginning of my field work, became firmly convinced that he and I were going to marry. We paid a visit to his village in the region of Kayes after spending some time making plans to do so. The respondent wanted to show me the place, and I was curious and grateful for this research opportunity to accompany somebody "back to the village." We went there together taking Birama's cousin with us for co-research. During our stay I had mixed feelings, which turned out to be well-founded: back in Bamako, the respondent confronted me with his ideas about getting married. It turned out that during our visit that he had told the entire family about his/our marriage, even about plans to migrate to Germany.

Obviously, the respondent and I had been in frequent contact in the course of my ethnographic research; I was interested in him and cared about him – but as a researcher, not as a woman. With the help of Birama's cousin, I tried painfully to clear up this enormous misunderstanding between the respondent and myself. His family was deeply disappointed and his aunt in Bamako demanded extra clarification. She was irritated about this "unexpected development." It seemed that our encounter had converted a long-cherished desire to leave into a supposedly attainable reality for them, making all of them fall for the idea. The respondent was deeply sorry when he realized his mistake. At the same time, this incident can be read as another arbitrary outcome of the political and moral economy of conditions post deportation, another result of the global social inequalities and injustices manifested in the deportation regime (cf. Khosravi, 2018, p. 6). It proved the existence of asymmetries in our research situation and, importantly, of differing gender conceptions as well. The respondent's aspiration to leave again was revived and he continued to talk about leaving again, but now he was focusing on other means.

Extending the site to the village. Researching post deportation in the rural hinterland

The major aim of my second stay in the field, from October 2015 to March 2016, was to continue following the cases and people I had met in 2014. I planned to concentrate on the themes developed from the data I had acquired so far (see fur-

ther analysis, pp. 91ff). More than that, though, I saw it as of central importance to explore rural Mali further, the places of origin of the deportees and the places to which they would potentially return after their forced removal, thus substantially extending my field beyond Bamako and deepening it.[14] It was also an opportunity to shed light on the little researched post-deportation context in rural areas. I continued to be fascinated by the tension between stasis and the urge to leave again, as well as by the "issue in the head" that it embodied, which seemed to originate from and be condensed in the designation "migratory adventurer" and, most notably, "failed migratory adventurer." However, the intention to focus on deportees "on the move," those who had recently returned and were about to leave again, altered substantially as I continued my research. Increasingly, I realized that conditions post deportation were often characterized by physical immobilization and imagined mobility, or rather by some kind of a continuum between different forms of internal and external mobility and immobility, not, as had often been suggested, by the predominance of a desire for re-emigration (cf. e.g., Schuster & Madji, 2013). Even if all the respondents, Malian former deportees as well as the Cameroonians stuck in "the Ghetto," had previously announced that they were leaving again, all of them were still there one year later.[15] They engaged where they were, bit by bit, continuing their lives and doing their best to support their families or get along by themselves.[16] This impression of immobility and permanence (cf. Gaibazzi, 2015a) became even more remarkable when I extended my stays in the rural areas of Kita and Yanfolila, where many young men seemed to stay on and live after their last deportation, sometimes after several deportations.

During ten months of absence from the field, a lot seemed to have changed. Migratory routes to the north had been opened up again and so the associations became more active in their daily assistance work. The period from 2015 on, furthermore, was the time of the large-scale shipwrecks. Among the 800 people ship-

14 As described in the last chapter, Mali remains very much centralized in and around Bamako, notwithstanding the fact that it contains a number of medium-sized cities (of about 100,000 to 225,000 (Sikasso) inhabitants), these being, in most cases, the capitals of the different administrative areas. For that reason, I speak about rural Mali and beyond Bamako, even though some of my research was also conducted in the cities of Kita, Kayes and Yanfolila, for instance. The great majority of my field research outside Bamako took place in the rural hinterland.

15 A young man I had met earlier shortly after his deportation from France was an exception, as were three young Cameroonians encountered in the "*Halle de Bamako*." All of them, however, had already moved on while I was still in Bamako in 2014 – either to gold mining sites to the southwest or northward, with a first stop in Algeria. The young man from France had suddenly gone off the radar (see further Chapter 4). In fact, being "highly mobile" they had re-emigrated.

16 Since 2016 two more of my main respondents in Bamako have left – one went to the gold mining sites in southwest Mali; the other one returned to France.

wrecked in April 2015, a catastrophe that sparked off the European "refugee crisis," as described above, were almost 200 Malians (Sylla & Schultz, 2020). This brought a new dimension to the migratory phenomenon in the country. In some villages vast groups of youngsters were leaving: "there are so many young people who are leaving currently" one former deportee told me, though he himself remained in Bamako, facilitating the migratory activities of the young of his community. Curiously, the German chancellor Angela Merkel became a repeated theme in my daily conversations as everybody wanted to know more about how she had contributed to a "summer of migration" (Schwiertz & Ratfisch, 2016).

I quickly put the plan to extend the site and become multi-local into practice. It was all about exploring *refoulé* hotspots in the rural hinterland, in other words, places that many deportees had returned to. Moreover, I wanted to accompany those I had met in Bamako back to their families' village sites. Meticulously, we negotiated with the representatives or associated partners of the self-organized deportee groups in the different localities. The AME had only recently appointed an associated representative in Kita, some 190 km west of Bamako, as the organization had become more aware of the large presence of deportees and repatriates in this area, many having come from Libya, and of their significance for the political dimension of post-deportation experiences in Mali as well. Boubakar had been an active migrant and retail dealer in the sub-region and also been deported from Libya about 15 years ago.

A first exploration with a coworker of the AME from Bamako to Kita built the base where I met (disappeared) migrants' parents, returnees, and local government and association representatives: an intense debate around a large table in the city hall revealed the obviously burning issues relating to migration and its current corollary effects. Several villages had been devastatingly affected by the shipwrecks, with sometimes 10–15 or more young men dying at the same time. This had led to bewilderment and despair in the face of an unprecedented situation; moreover, new voices repeating "the youth need to stay" were making themselves heard (as touched on in the introductory chapter and to be further explored in Chapter 6).

In addition, there was already a large presence, many hundreds in total, of deportees and repatriates in the surrounding villages, though a new "wave" of deportees and repatriates from Libya and Algeria would increase the numbers again in 2017, reshaping rural landscapes through the presence of returnees and their organizations and the setting-up of reintegration projects in the context of the EU-IOM-Joint-Initiative (see Chapter 2). Boubakar, the new AME representative in Kita, had not only announced our arrival on the radio,[17] but had also paid personal visits to a number of villages informing them of my envisaged fieldwork. He was

17 It is a common practice to announce different kinds of activities, news and occurrences on the radio as radio is the most widespread and established medium in Mali.

well-known and respected in the city and many surrounding communities. He was engaged in a variety of development and migration projects, but notoriously lacked money: donors had often promised to pay but their promises had rarely been kept.

It took some explanation of what I was after in my research (ethnographic basics such as staying in relevant places, speaking with relevant people, living with a deportee's family, etc.). The major criteria we agreed upon eventually were: the presence and number of deportees in a certain village; deportees who for the most part had been on their way to Europe or North Africa; and places that were accessible. The selection of extended field sites necessarily depended on Boubakar's personal contacts and relations. Eventually, I made repeated prolonged stays in two villages in particular. Intense negotiations continued about being part of this field.

In these "selected" villages, we each stayed with a family of former deportees. Each beginning was meaningful in itself. It was clear that we had to follow the "official" and hierarchical procedure for paying visits in this region: through the mayor, if he was based in this specific locality, the chief of the village, and the local notables. Extended introductory meetings, presentations of one's role and research interest as well as exchanges of gifts were essential steps in any first encounter in order to build a dignified foundation and the trust required to enable collaboration. It was indispensable, therefore, that I came with some more or less local counterparts, Boubakar and Birama Bagayogo in this case. Often we would sit together with a group of former deportees and drink tea in the village center or on the patio of a former deportee's house. It was mostly deportees themselves or other key people in the villages who brought together former migrants, deportees, and repatriates by spreading the word that a German researcher was there and wanted to speak with them. This seemed to be a great attraction and maybe a welcome diversion particularly in moments of boredom during the dry season. At the same time, my presence seemed to spark a multiplicity of experiences, expectations, and hopes, or maybe mere curiosity. Certainly, a certain degree of self-selection among respondents needs to be taken into account as well. Overall, however, the high number of people attending underlined the omnipresence of deportees and repatriates.

In one small village near Kita, which eventually became one of my major research sites (as contextualized in the previous chapter), it was young men, former deportees themselves, who contacted Boubakar following the first radio announcement. They paid him a visit aiming to get organized to raise funds for little projects for the *refoulés* in their village and to speak up for them. One of them, who had been sent back on a vessel from the Mediterranean close to the Libyan coast several years before, knew Boubakar from a joint project. When I returned to the area for a longer stay some weeks later, I met these committed young men and became installed in their village. It was during longer stays in their village as well as in the larger village with a community center that I was able to go into greater depth in my research into the rural, social embedding of post-deportation experience.

Collaborating with a co-researcher – considerations of give and take

Through my collaboration with Birama Bagayogo as a co-researcher, my project received a particularly substantive character. Not all former deportees spoke French. Particularly beyond Bamako conversations were mostly held in Bambara, the most widespread local language. My own attempts to learn it by taking classes had only taken me as far as being able to speak in a rudimentary fashion, though my proficiency was enhanced through engaging with people on the spot. Still, having somebody to provide assistance, translation and collaboration, became indispensable for solid research. Birama was a 33-year-old Malian anthropologist, consultant on health issues, and an acquaintance of a Malian doctoral student I knew, who recommended him. From October 2015 to March 2016, he accompanied my fieldwork on numerous occasions as a linguistic and cultural translator, and increasingly as an appreciated interlocutor, advisor, and friend.[18]

While we obviously had to find a modus operandi, our collaboration became smoother and smoother over time. We shared intense days, thoughts, and work during the weeks in a village, but also in Bamako, where I partially worked from his office. Birama supported me as much as he could. I tried to do the same for him, introducing him to the self-organized deportee groups and activist circles as well as other researchers. It turned out to be a very fruitful and productive research collaboration. Mostly received positively by people who were curious and welcoming, we were both strangers in the villages, one more than the other, of course, and in different ways. In these positions, we also represented "money," "foreign knowledge," and a supposed "power." I was often confronted with stories of experts who had visited and, for example, promised a large project, but had never come back. This created mistrust, which needed to be overcome. Otherwise, our supposed expertise was requested for consultations on alternative earnings, discussing small projects like installing a garden, or organizing animal breeding. As foreign researchers, this seemed to be an unavoidable role, and came despite constant clarifications of our reason for being here.

Generally, collaborations such as the one between Birama and myself have raised multiple concerns regarding power imbalances or loss of accuracy through translations (Englert & Dannecker, 2014). However, this research constellation brought essential benefits. Since deportees in the village sites were almost exclusively men of little education and from a rural background, having a male counterpart created more commonality. Birama, for instance, could hang out with the young men, when I was somewhere else in the village or talking to somebody. They would tell him about "male" intimacies, such as hooking up with women in

18 On two occasions, I was accompanied by Birama's cousin Madiba Malé, also a young anthropologist, as Birama had other commitments.

the nearby city while their wives and children stayed at home. These things would hardly have reached my ears otherwise. On the other hand, Birama received a good salary; we had negotiated according to his standards (which I considered high and demanding at first). Even if he had had little connection with the topic of migration so far, he was intrigued to explore this new angle, and applied it in subsequent projects, for instance on informal gold mining.

Birama and the others who provided access to the field, notably the deportee organizations, were of tremendous assistance in handling the complex requirements of giving and taking in the field. Importantly, I had to learn about the social meaning of money and the complexities of a widespread generalized reciprocity (see Chapter 5). I had to literally pay some of the respondents, although many textbooks would suggest the opposite. "It's your business!" one deportee in Bamako said quite frankly, when discussing the issue of how I could compensate them for their time and sharing their (life) stories, suggesting that I give a little money when we met next time. Not only did the run-down economic situation in Mali create existential needs for many,[19] most importantly, those in the city without a social net were suffering: sometimes, it was hard for me to overcome a sensation of wanting "to give support" and being hounded by the awareness that I was unable to do much. Obviously, these historically grown and reinforced inherent inequalities were impossible to overcome simply.[20]

My conversations, collaborations, and relationships with the Malian field have continued until now, in different forms and at different intensities, with a number of respondents, with my gatekeepers, the organizations, with Malian researchers and with Birama – on Facebook, via WhatsApp or email, and sometimes over the phone. They consist in exchanges regarding information and money, or in simple courtesies and keeping up friendships. Despite evident "irreconcilable incompatibilities," particularly with some, being indebted (to one's field) is a condition and characteristic of social relationships. It is not only that the field "never stops," it is rather that the field and my experiences have become part of me and I have become part of this field.

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19 Before, he had been doing little trips with and services for tourists, who were absent now.

20 Others have developed approaches to balance such imbalances (e.g., Giebeler & Meneses, 2012), toward more "equal" and reciprocal giving and taking. However, this requires extensive preconditioning and cannot always be put into practice. I had had an excellent start, beginning my research embedded in a Malian academic institute. I presented my first field results in a small colloquium there and organized an event with the German embassy about educational opportunities in Europe. One scholar was following a similar topic to mine. We started a fruitful and frequent exchange, sharing contacts, thoughts, and arguments, and even conducted joint field research activities; in fact, we have been engaged in academic projects together up to the present. This latter combined effort might meet most expectations of collaborative research and a thorough give and take.

Methods, sampling and data gathering in polyphony

The corpus of data material for this study is very extensive and diverse in form and quality, as is typical for most ethnographies.[21] Even though I followed a snowball process in the main, the idea behind the sampling (Corbin & Strauss, 1990) was to collect the narratives and observe practices of former deportees and their social surroundings. To be considered deportees, subjects had to have been forcefully returned or obliged to return, which included having been repatriated or undergone a transit return. Most such people were defining themselves as *refoulés* (see Chapter 1; cf. also Table 2). In the course of the research the aim became more specific: to collect contrasting samples of urban and rural cases, with central respondents and people who had presumably stayed on after (their last) return and including younger as well as older males. In the following, I will briefly summarize the methods I used, the data I obtained, and the results I analyzed.

I applied a reflexive ethnographic approach (cf. Davies, 1999) including a variety of methods, types of persons, documents, and institutions. To approximate deportees' experiences in accordance with their personal stories, their practices, and everyday observed behavior, I used different forms of qualitative interview (in-depth and semi-structured[22]), informal conversations, and (group) discussions, (participant) observations as well as field notes (cf. Bryman, 2012, p. 213; cf. also Rubin & Rubin, 1995). I used ethnography as an overall approach since it allows going beyond "methodological constraints" ("*Methodenzwang*"; Feyerabend, 1976), thus allowing the researcher to draw on a complex of methods applied according to the needs of the field. Breidenstein et al. (2013, pp. 31ff) describe the methodological "trademarks of ethnography" as fourfold: its subject-matter is social practices; it conducts field work through "continuing immediate experience"; it allows for methodological opportunism through an "integrated research approach"; and, finally, it ends in "writing and verbalization of the social."

21 Bryman (2012) calls the sampling of respondents in an ethnographic research project "sometimes a combination of opportunistic sampling and snowball sampling" (p. 424). Accordingly, much of the time ethnographers are forced to gather information from whatever sources are available to them. Very often they face opposition or at least indifference to their research and are relieved to glean information or views from anyone who is prepared to divulge such details (ibid.).

22 The term "qualitative interview" (in-depth interview) sometimes refers to an unstructured interview but more often refers to both semi-structured and unstructured interviewing. The use of this term seems to be increasing. And the term "semi-structured" typically refers to "a context in which the interviewer has a series of [somewhat more general] questions that are in the general form of an interview schedule but is able to vary the sequence of questions" (Bryman, 2012, p. 212).

Conducting ethnographic field work is based on the experience of the ethnographer as well as that of the respondents. Memorized experiences in my research were retrospectively recounted and gifted with meaning and sense in narrations and stories. As a matter of course, it needs to be acknowledged that there is a large gap between experience and expression (Bruner, 1986, pp. 6f) and we have to take into account respondents' narrative reconstructions as well. Rosenthal, building on the narrative interview after Schütze (1976), highlights the need to differentiate between experience, memory, and narration as researchers. She refers to the "narrated personal life as related in conversation or written in the present time" and the "lived-through-life" (2006), and the need to differentiate between narrated and experienced life history. Narratives of experienced events, consequently, refer both to current life and to past experience.[23] It is essential to take this into account when interviewing and analyzing. One has to familiarize oneself with the given context, not least in order to define the field and one's research topic. In my case, the local language constituted a particular issue. On the other hand, coming too close or, as it were, "going too native" might lead to a loss of oversight and analytical clarity. I had to beware of what I termed, with Susann Huschke, "activist's blinders," so as not to obscure my research capacities. Coming too close might lead to losing this position of neutrality (cf. Jónsson, 2007). The ethnographic challenge lies in a constant shifting of roles, in negotiating, exchanging, balancing, and reflecting over and over again in all possible constellations.

I first of all followed a relatively open interview structure to generate topics and set the scene for understanding deportees' general interpretative patterns of situations post involuntary and forced returns. To do so, I often started with an open question such as "I would like to hear your story of leaving and return: Why and how did you leave your place? Where did you go to? How and why did you return? I will just listen and ask questions afterwards…" Such openings frequently resulted in an interaction made up of questions and shorter or longer answers, which had the character of a conversation rather than a consecutive narration. Some people started talking and talked for a long time. In other cases, it felt as if I was getting bits and pieces, which certainly had to be accepted as respondents' boundaries as well.[24] So, I adapted to each specific situation (cf. also Hasselberg, 2018) over the time of my research.

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23 In this way, "even in the case of a spontaneous memory-based narrative of experienced events we have to assume that there is a difference in general between the experienced event in the past, the recollection thereof and the narrative" (Rosenthal, 2006, p. 3).

24 It is important to note that my interview partners had been thoroughly informed and had agreed to participate in the specific setting and purpose of the research. Moreover, they knew, they could withdraw at any point. I tried to be as careful as possible not to dig deeper when issues appeared to become sensitive.

For the second phase of my field work, I built substantially on the analysis results of that first round of interviews, conversations, and observations. My interest was predominantly in the aspects of failure and shame, suffering, the migratory adventure and *la chance* – let's call it a broader spiritual and cosmological embedding; in addition, I was interested in the complex of family, social, and kin relations and in the complex of hard work, everyday life, going on and "real chances" as well. Questions in conversations with former deportees circulated mainly around their everyday and economic activities, experiences before, during, and after deportation, and their social relations and reactions to the migration and return. Accompanying questions (without spelling them out) were: what did this supposed failure of expectations imply exactly? What role did the families and social surroundings play? What did the unwanted return mean in socio-economic terms as well as in the long run? I developed a semi-structured interview guideline that I discussed and shared with Birama. Eventually, our conversations left a lot of unstructured space, where we ended up talking openly in each conversation.

I conducted repeat interviews during both field phases (cf. Fauser, 2009). This enabled selective points to be researched more concretely, and contradictions and conflictive statements to be carved out (cf. Schultz, 2014). Several of the people I was able to accompany, meet and speak to over the course of the two field phases, became key respondents allowing a specific depth of research. More than that, the twofold sampling over the two research phases, even if primarily implemented as snowballing, also served as a triangulation strategy.

Table 2 (see page 95) gives a rough overview of the methods used and the data obtained.[25]

Besides the framing through the organizations that I had started my field research with, I aimed at getting a grasp of the views of the social environment – the former deportees' brothers, sisters and cousins, their fathers and mothers, their friends, neighbors, colleagues and other acquaintances – to better capture their social embedding. Again this was done through qualitative interviews and informal conversations, as well as group discussions – in Bamako, but mostly in the village

25 Instead of differentiating according to the destinations of deportations, repatriations, and transit returns, the table divides respondents by the research sites in Bamako and rural areas. The large majority of deportees consider themselves as falling into the category of *refoulés* (see Chapter 1). Self-induced return without external help refers to those who describe themselves as having returned involuntarily, constrained by the external conditions, mostly unplanned, particularly in the context of the Libyan crisis 2011–2014; "voluntary" self-induced return refers to those people who describe themselves as having decided and planned to return, for instance, from Spain, but also from Morocco or Libya. In the latter cases, it may still have been a general worsening of conditions that provoked a return decision, such as increasing unemployment or racism.

sites – oftentimes with the entire extended family. Moreover, I observed the institutional context at different levels: administrative representatives in the village, municipality, and at the regional and national level. Additionally, interviews with Malian government officials, and representatives of NGOs, international organizations, embassies (those of France, Switzerland, Spain, and Germany), and civil society, as well as with other academics, allowed an understanding of the political and economic embedding of post-deportation experiences in Mali.[26]

Furthermore, we had a variety of group discussions with former deportees (see e.g., Bryman, 2012, pp. 501ff), more formally organized or spontaneously emerging, sitting together, drinking tea and passing time chatting or in rather more "serious" conversation. Such group gatherings contributed to grasping the individuals' embedding as well as their situations after deportation more broadly; furthermore, they served as a means to cross-check dates and background information. Sometimes there were illuminating discussions on terms introduced by me or Birama, such as the different meanings of "suffering," and the meaning and practice of "the migratory adventure," but they also covered people's everyday lives and general perceptions. In these "gatherings," the social situation and specific interactions were often the most interesting aspects as they arose out of dealing with the issue of (post) deportations in a semi-public space, which allowed me to grasp the views of the others. Laughter or sudden silence, for instance, could be indicators of issues considered shameful. Still, it was not all about finding out what had "actually" happened; it also involved taking into account the way that the participants presented their stories and how this was related to the intervention created by the interview situation, here taking place in a group (Rosenthal, 1993). Even if this this kind of intervention may come across as rather informal in an ethnographic approach (Breidenstein et al. 2013, p. 80), it allowed us to get an idea of the specific ways in which deportees related their doings and activities post deportation in a group setting. At the same time, accounts given in such groups could replicate what I had heard from other respondents during more intimate sessions.

26 Here, I was mostly interested in the roles and responsibilities of the institutions, their mandates, their evaluation of the current situation with regard to migration / migration and development / involuntary return migration, their perception of the developments in current migration policy and their potential involvement, their take on voluntary return vs. involuntary return and reinsertion, in general and in policy development, their collaborations (with government, NGOs, embassies, the EU), and how they saw the future of migration (policy), in Mali in particular, with regard to (involuntary) return.

Respondent / Data Group	**Method**	**Quantity** Persons / Groups
Representatives of Malian governmental organizations, embassies of EU member states, the EU, Malian and international NGOs in Bamako, Kita, Kayes, and Yanfolila	Qualitative interviews (partially repeated in both field phases)	45
Deportee self-organized groups in Bamako, Kayes, and Yanfolila	Qualitative interviews (repeated)	16
	Informal conversations and (participant) observation (repeated)	
	Contacts via Facebook, WhatsApp, email, or telephone (continues)	6
Transit migrants (mostly from Cameroon, also from Liberia and Sierra Leone)	Qualitative interviews	5
	Informal conversations and (participant) observation (repeated, mainly in 1st field phase)	
	Group discussions	2
	Contacts via Facebook, email, WhatsApp or telephone	2
Malian deportees in rural sites	Qualitative interviews	48
	Informal conversations and (participant) observation (repeated during several longer stays in the village)	
	Group discussions	17
Social surroundings of deportees in rural sites (family members, friends, neighbors)	Qualitative interviews	24
	Informal conversations and (participant) observation	
	Group discussions	19
Malian deportees in Bamako	Qualitative interviews	28
	Informal conversations and (participant) observation	
	Group discussions	2
	Contacts over Facebook, email, WhatsApp or telephone	5
Social surrounding of deportees in Bamako (family members, friends, neighbors)	Qualitative interviews	9
	Informal conversations and (participant) observation	
	Group discussions	3
"Assisted voluntary returnees" with the support of the IOM in Bamako	Qualitative interviews	2
Self-induced voluntary returnees	Qualitative interviews	2
	Informal conversations	10
Self-induced involuntary returnees	Qualitative interviews	5
	Informal conversations	18
Random encounters and informal conversations with people in the streets, taxi drivers, etc.		
Conversations with Malian academics at the transnational research center, the University of Bamako and other research institutes in Bamako		
Field diaries Electronic notes	Fieldnotes Fieldnotes, reflections	13 numerous
Policy and project documents, reports, and statistical data from the deportee and other organizations or political institutions	Collection, lecture and partial content-analysis	numerous
Newspaper articles	Collection, lecture and partial content-analysis	numerous
Materials from academic and national archives	Collection, lecture and partial content-analysis	numerous

I wrote up as much as possible in field diaries, as well as electronically "writing and verbalizing the social," often sitting to one side, observing, reflecting, listening, and also asking questions. On other occasions, note taking was hard. Judith Okely (2008) coined the term "knowing without notes," which implies the idea of an embodied field: in this sense, many things that I experienced contributed to understanding and deepening my work and later my analysis, even if they were not meticulously written down.[27] I complemented my note taking with an audio recorder and sometimes a video camera to generate a more "focused ethnography" (Knoblauch, 2001)[28], especially in light of my "extending the field site." I also took many pictures. While respondents, particularly those I knew better, would take a video themselves, on other occasions this observing external eye caused serious uneasiness and suspicion of errant publicity and lack of privacy. In the end, though, the visualizations and recorded interactions provided more atmospheric and enriching impressions to support my analysis.

Throughout, I appreciated random encounters with people in the street, the shop owner next door, and, importantly, taxi drivers, "classic" informal respondents (cf. Lachenmann, 2010). In addition, I collected policy and project documents, reports, and statistical data from deportee and other organizations or from political institutions, as well as newspaper articles and relevant academic literature, paying particular attention to material from from Malian researchers and others in the region. Not least, I went to Malian academic and national archives. I never did a formal analysis of these materials, but they served to supplement my understanding of the broader context and the embedding of my data. Even so, if pertinent lectures and reading were indispensable, some aspects or specific expressions could only be understood with the assistance of people from the field. Eventually, I clarified questions and local terminologies through "going back virtually" to the field by talking over the phone, skype, or exchanging messages by email.[29]

In the end, the polyphony of my research, the different types of people interviewed, the institutions and sites selected, the positions taken as well as documents included, served to triangulate my data and perspectives. Triangulation of interview and ethnographic materials is an important quality criterion for justifying and

27 Breidenstein et al. (2013) describe a central intuition, which has led to criticism of the method as it cannot be "objectively" measured as other qualitative approaches would be assumed to be.

28 The idea of a "focused ethnography," which in sociology is mostly about doing research in one's "own" society (Knoblauch, 2001), seemed to constitute a sociological ethnography beyond the "ethnos" of another society (which still requires an "estranging" of the familiar; cf. ibid).

29 After I left Mali in 2016, Birama also remained in contact with some of the respondents. Moreover, he conducted an additional fieldwork exercise in 2018 in the village where I had spent most time and verified some issues.

assuring results (Englert & Dannecker, 2014). More than that, this polyphony was an indispensable research strategy when it came to answering my research question. Understanding (post-)deportation experiences, their social embedding and life after deportation back in the Malian south, could not have been adequately done by speaking to deportees only.

Analyzing along the general lines of grounded theory

I quickly got an inkling of the essence of what I later termed the supposed "failed migratory adventure." It lay in the recurring motifs of money loss, "*la honte*" (shame), social expectations in light of the observed "issue in the head," and restlessness, that came out of my first round of analysis and was built on my immediate observations in the field. As described in the introductory chapter, talking about failure, in terms of "*j'ai échoué*" or *maloya*, was an emic conception; but one that was also used in the political setting and, importantly, by the deportee organizations and other political actors. Furthermore, it was a media and academic discourse that I felt needed to be explored in more depth and with more differentiation.

A "thinking about data" (Schultz, 2014; transl. S.U.S.), i.e., a going back and forth from the data to theoretical reflection, started early on. I took my first steps in the field guided by the parameters of grounded theory: from an entry into the organizations to a snowballing of my respondents, to developing broader and more specific questions and extending my field site. From the enormous corpus of audio and video data, and the notes I had collected, I selected what was to be processed, transcribed, and analyzed further on the basis of places and cases – those I had known best and for the longest time, in Bamako and in the countryside, as well as those that were most contrasting, in order to best explore and describe the range of the phenomenon. Contrasting, in this instance, meant selecting those with the largest differences in outcome, so long as I had sufficient validation of the type in question (in terms of theoretical saturation, see below). Data from the rural hinterland mainly derives from the two villages in the area of Kita where I stayed longest.[30] I transcribed parts of the data in French myself; while Birama carried out the majority of the transcriptions from the second field phase. Parts of this

30 Even though I also spent several weeks in the area of Yanfolila, where large numbers of deportees returned in the first decade of the 2000s, I had to leave the data out of fine-grained analysis and, above all, when writing this study. It would not have been possible to present it here in a sufficiently contextualized way. Nonetheless, the insights gained from this tranche of research and first analysis contributed substantially to my understanding of my overall topic and to better shaping my argument.

audio material were in Bambara, which he translated simultaneously. In this process we talked intensely about central emic terms and conceptions; still, I am aware that aspects must have been lost in translation. Transcribing serves as a first, more in-depth round of analysis. I revised the transcriptions I made with Birama thoroughly and supplemented them. This was an essential step in becoming familiar with the data and starting the analysis. Some sections needed to be listened to over and over again before I could grasp their subtle, emotional meanings.

The issue of theoretical sensitivity and prior knowledge is the central conflict between Strauss and Corbin, on the one hand, and Glaser, on the other, the founders of grounded theory.[31] Clearly, the selection of my general research topic, the social dimensions of post-deportation in Mali, was driven by knowledge of the political setting and academic literature in deportation studies. I set out to do my first field work early during my research project in order to preserve some of my own openness. I conducted a first analysis through open coding of interview data and field notes orientating myself on the descriptions of Corbin and Strauss (1990) after both field phases. Instead of generating full-fledged theoretically saturated categories at first, I formed code groups indicating three thematic blocks and tensions, which served for continuing my field research.[32] From all of this I selected a number of terms to be more systematically researched in their emic meaning and local embedding back in the field.[33]

A first impression of "theoretical saturation" (Strauss & Corbin, [1990] 1998, p. 212, cf. Bryman, 2012, p. 421) was gained through people giving me similar accounts. I thought at first that this meant that they were providing me with superficial stories and I would need to go deeper; however, I soon realized that it was much more to do with their stories, how they perceived and told them and the kind of

31 While Anselm Strauss and Juliet Corbin suggested that prior knowledge would exist in any research and contribute to theoretical sensitivity towards the research topic, Barney Glaser was convinced this prior knowledge would hinder openness towards grasping the research phenomenon in the field. In line with Strauss and Corbin (1990), Schultz (2014) recommends writing up one's knowledge in order to visualize and be conscious of potentially hindering presumptions. This can be done, for instance, by making situational maps (cf. Clarke, 2005).

32 The first tension was inherent in the term "the migratory adventure," the shifting between leaving and staying related to hardships and sufferings and simultaneously chances and opportunities. The second tension emerged from the complex of sociality, responsibility, and obligations towards the family, in close or more distant social relations, and from embedding after deportations. Third, I found a tension between being hard working yet having nothing to do, which could again be a form of suffering. In fact, these already closely resembled my final analysis and argumentation.

33 These circulated around three complexes: 1) stigma, shame, failing ("*la honte*"; *maloya*); 2) migration, mobility and adventure and related terms such as *tounkan*, *clandestin* or *voyager*; 3) the meaning of *la chance*, of suffering (*la souffrance*), expressions such as "*ce n'est pas facile*," "*il n'y a rien*," succeeding (*réussir*), and money (*le moyen*; *wari*).

reconstructed narrative that came out of that. Of course, in some cases there could be even more subtle layers of meaning that I came to, as well as differing theoretical angles. Not least, my polyphonic and at the same time in-depth research approach was driven by the need for theoretical saturation. Even if cases sometimes seemed to be erratically different and dispersed, I was able to analyze comparable and similar patterns and commonalities clearly in accordance with the specific deportation trajectories, origins and destinations of former deportees, their time spent abroad and, after their return, the money they had earned or lost, and their age, gender, and education. These aspects will be developed throughout this study.

From the open coding of the material after the second field trip, I again formed summarizing codes and code groups in order to come to categorizations, comparing them with the codes and code groups from the first field trip. While axially and selectively coding, I started to write up, beginning by constructing and following my cases – again starting with those that were closest and best "known" to me. From there, the topics emerged again through writing. They centered more and more on the issues of the adventure, (im)mobility, suffering, money, kinship, hard work, courage, and *la chance*. I went back to axial coding again, and even more to axial wording – that is to say that I worked with the word cloud function in my MAXQDA analysis tool as I was not fully satisfied with the results of my open coding and grouping, it seemed far too broad still. What came out again were the complexes family/kin, money/*moyens*, migration/adventure/(im)mobility/journey, hardship/suffering, and failure. So I went back to the transcriptions and searched for the embedding of the terms "money/*moyen*/wealth," "suffering," and "adventures/(im)mobilities," and how these were interlinked. I started reading and theorizing further, while continuing to think about my data. Recapitulating what I had analyzed through earlier categorizing, I was able to develop more full-fledged categories and theoretical concepts. Overall, it worked out like a circular process. In the end, the recurring topics and theoretically saturated[34], undergirded main themes were "failures, success," "suffering and courage," "masculinities," "social navigating," "*la chance*," "money, social surroundings," "mobility, immobility," "youth and becoming," "rural–urban," "narrating, talking, not talking."

I selected the supposed failed migratory adventure as core category for this work as it best captures and combines all relevant phenomena in one guiding

34 In the language of grounded theory, a category operates at a somewhat higher level of abstraction than a concept in that it may group together several concepts that have common features denoted by the category. Saturation does not mean, as is sometimes suggested, that the researcher develops a sense of déjà vu when listening to what people say in interviews but that new data no longer suggest new insights into an emergent theory or no longer suggest new dimensions of theoretical categories (Bryman, 2012, p. 421).

theme.[35] It allows me to shed light on how the political and economic dimensions of deportations are socially reflected in deportees' narrations and lifeworlds, and how the explicit political effects, emotional and material losses of the global deportation regime translate into embodied and ambivalent individual and social dimensions. I will develop the chapters that follow along the lines selected.

How to study conditions post deportation? Concluding reflections

This chapter has provided a methodological account of how I researched the multi-locality and omnipresence of deportations over an extended field site in Mali within a relatively short time. To conclude, I will briefly summarize my response to the question of what kind of ethnography I carried out in order to eventually come up with fully developed and satisfactory results.

Deportations need to be analyzed in all their complexities and dynamics. The multi-sited approach is quasi tailor-made for researching people on the move, and allows one the best standpoint from which to grapple with the rupture and other effects of the administrative practice of deportation in its transnational dimension and all aspects of the migration cycle, as research in deportation studies has increasingly shown (e.g., Hasselberg 2018; Plambech, 2018, 2017; Alpes, 2017; Drotbohm, 2016, 2015, 2012, 2011; Khosravi, 2016; Golash-Boza, 2015). I did not, as originally and theoretically intended, follow the "highly mobile" deportees and *refoulés*, but more and more applied myself to and embraced the topic of immobilization after deportation as it emerged from the field. My research was still multi-sited in a sense, though, in that I started to explore and capture the post-deportation field in Mali in its different varieties and forms in different localities. Against this background, my field work was characterized by a tension between being called by the field and staying focused, following "the local cultural grain," but also conducting research in as "polyphonic" a fashion as possible (Meyer, 2013, p. 312), thereby collaboratively developing the field and fulfilling the respondents' and gatekeepers' expectations as well as my own research demands. This had to be balanced and renegotiated over and again.

I developed a combination of a "go along" (Kusenbach, 2003) and, what might be called, a "hang out" ethnography. I followed individual deportees over a longer period and increasingly focused on the everyday life of Malian deportees in rural and urban places as well as their social embedding. I thus eventually used some of what

35 Even if it can be hard, or even impossible, to find one all-explanatory core category (cf. Corbin & Strauss, 1990, p. 14). In the end writing a study of this kind is a matter of making decisions – where to start, what empirical data to select and what not – there may eventually be different stories to tell from the data.

Hahn suggests are the benefits of a place-based approach, to observe the translocal, global, and transnational (social, cultural, political, economic) dimensions in their connections with and disruptions of what I would call the effects and embedding of the (post-)deportation regime. The extended site gave me both a particular view and a broad understanding and allowed me to go beyond potential localized/place-based limits, enabling me to gain an in-depth view of a broader phenomenon in one analytical frame, in one single site – the post-deportation situation in Mali in its different facets, the various actors, places, discourses and deportation practices it involves.

As a white European woman, I need to acknowledge that there are specific results and shades of narrative presentation I collected, which others would have come to differently. Against this background, my research collaboration with Birama Bagayogo turned out to be particularly beneficial as we fruitfully combined a Malian, academic and urban (though rural in origin) with a German research view and positionality. Moreover, my research strategy to set up a polyphonic and in-depth approach at the same time allowed the operationalization of my research question in finding out about deportees' lifeworlds, conceptions and social embedding after deportation. In all this, our encounters, interactions, and observations represented in themselves a current effect of restricted mobilities, externalized borders and the deportation regime in a society characterized by longstanding "cultures of migration" and migratory adventures.

In the chapters that follow, I will analyze conditions post deportation as effects of the global deportation regime, reproduced by deported actors on a local site. I will develop grounded descriptions of these conditions by presenting my arguments on the basis of the data I collected in Bamako and in the two villages near Kita where I did most research during eight months over the course of two years, supplemented by telephone calls, email, Facebook and WhatsApp conversations, from the end of my field work until the time of publication.