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Bridging Mobilities: ICTs appropriation by Cameroonians in South Africa and The Netherlands

Nyamnjoh, H.M.

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Author: Henrietta Mambo Nyamnjoh

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Methodological considerations and data collection

Introduction

This study is part of a larger research project entitled ‘Mobile Africa Revisited’ that has been examining the appropriation of ICTs and how mobile migrant communities are shaping and being shaped by them. The study is an ensemble of conversations, my observations and interaction in the various field sites as well as extensive interviews and the employment of other ethnographic tools that together have resulted in this book. This chapter is divided into two main sections. The first section focuses on my methodological reflections, paying particular attention to my role as a researcher, ethics and how I selected my informants, while the second section offers detailed insights into the methods used for collecting data and my analysis.

The study adopted a multi-sited approach and concentrated on migrants from the Pinyin and Mankon areas of the Bamenda Grassfields in Cameroon, following them to their new localities in Cape Town, South Africa and The Hague and Amsterdam in the Netherlands. The study adheres to the call by MacClancy & Fuentes (2011), Marcus (1995) and Gupta & Ferguson (1997) for a spatial and temporal fieldwork method that takes into account both the point of departure and of arrival and, in this case, a multiplicity of points of departure and arrival. Its particular ethnographic contribution is supported by a holistic approach and the intensive fieldwork and data collection are grounded in techniques of qualitative research. In the process, trust and confidence were gradually built up.

By drawing on Bourdieu’s experiences of ethnographic fieldwork in Kabylia, Algeria, it was considered important to ‘combine intimate knowledge (in this case of being Mankon and from the Grassfields) of practical activity with more abstract knowledge of objective patterns, and using the dialectical relation be-

tween the two to break with the familiar ways in which people understand their own everyday actions' (Calhoun 2003: 8; Bourdieu 2012). It is in this light that theoretical concepts, in conjunction with empirical findings, have led to a study of everyday life. These daily practices, as suggested by de Certeau (1984), are tactical in character and thus require observations from a bird's eye view that include perceptions not only the sense of sight but one that also involves all the senses of specific action be they physical or vocal. What gives this action validity is the meaning attached to the action (Holy & Stuchlik 1983: 17) and, in this vein, the observer needs to be conversant with the meaning. It is in this respect that Förster (2001) proposes that observation is and should be intentional, encompassing the whole world and any ensuing actions that emerge. Attending various tribal functions and meetings required careful observation and involved unpacking or paying attention to the various forms of speech, such as debates and comments made on the side. Conversations and interviews were constantly interrupted by phone calls and text messages and these meant that the observer needed to be alert to the different layers of interaction and connectivity to make sense of them. In the course of my interview with Clovis, for instance, he informed me that he had been issued with a 'you must go' message (30 days to leave South Africa as his asylum permit had not been renewed) and so he was actively looking for different options to legalize his stay and had contacted a number of persons who had promised to get back to him. The beeps, texts and calls were all geared towards providing him with solutions and so whenever there was a beep, we would stop for him to call someone back. By the end of the interview, he had settled on going for a life partner that was proposed by a friend's friend in the township (a weak tie). This scene speaks of the level of connectivity to the host society and the speed with which weak ties can function in bridging social relations, as well as any opportunistic tendencies.

As will be seen in Chapter 6, the Pinyin and Mankon migrants live in clusters and have thriving communities in which I participated, attending both public and private events. Being part of various communities in Cape Town and Cameroon as well as in the Netherlands obviously called for a multi-sited research approach. And I spared no energy in highlighting this multi-sited dimension of the research, as will be shown below.

An important way of trying to understand the migrants and communities I was researching was by adopting a reflexive/inter-subjective approach. In this respect, a reflexive approach that charts my own personal navigation of mobility, connectivity and networking, both as a migrant and as an interviewer, provides a practical and empirical understanding to add to the stories told here.

Methodological reflections

Reflexivity/inter-subjectivity

Being a migrant myself, having lived apart from my spouse and then finally leaving my country of origin thirteen years ago, my encounters with these mobile communities is a reminder of how our lives are inter-related. They are anchored in mobility and seamed by ICTs and networking on the one hand. And on the other, doing research amongst such communities was not matter-of-fact, even though, like them, I too come from the Grassfields of Bamenda, from Mankon, from a similar culture and, as a woman, understand the meaning of transnational parenting. Our narratives are interlinked in multifaceted ways, also as a mother who was constantly away from home during the duration of this study. My choice to do research among migrants and immigrants (Nyamnjoh 2010) was embedded not only in the self as a migrant, but culturally too. If a reflexive approach, according to Mason (1996, in Guillemin & Gillam 2004: 274), entails that the ‘researcher should constantly take stock of their actions and their role in the research process and subject these to the same critical scrutiny as the rest of their “data”’, then the desired effect has been achieved as there were moments in the field when I had to refrain from allowing my experiences from obfuscating those of the researched. Instead, I tried to maintain a reflexive distance. Arguing along similar lines but bringing in emotions as well, Holmes (2010: 140) perceives reflexivity as an ‘emotional, embodied and cognitive process, in which the social actors have feelings about and try to understand and alter their lives in relation to their social and natural environment and to others’. In this respect, the choice of a reflexive approach concurrently helps to understand my own personal migrational trajectories and to better understand those of my informants, and is equally reminiscent of my emotional interaction and contours during my periods of separation both as an interviewer as well as a migrant and mother. It informs too the ethical decisions and the stance I took in and from the field. In this respect, this study is less focused on ‘othering’ than on incorporating a reflexive focus on ‘own’ and breaking down a specialized metaphor of the field (Gupta & Ferguson 1997: 2-5). Enhanced by memory – ‘autonoetic memory’ – that ‘involves the ability to recall personal experiences and its different forms’ (Tulving 1972, in Collins & Gallinat 2010: 11), our life paths interlink in multifaceted ways. It is in this light that Schank (1990) argues that ‘memory organises experiences into retrievable units on which the self draws during social interaction’, the baseline on which people (the researched or researchers) form social units. Taking the cue from Tulving (1972), Collins & Gallinats (2010: 11), opine that ‘the self in consort with memory is implicated in ethnography; not only in the field but also in the study ... the self thus becomes the incubator of ethnography’. However, the question that arises is about the sort of memory that is triggered.

What does the position of researcher create in terms of data? How does it influence the production of data?

While this approach has been foregrounded by Collins & Gallinat (2010), Holland (1999) and Mauthner & Doucet (2003), it is by no means without its critics. As noted by Dyck (2010), reflexivity has been ‘confined to the introduction and/or preface; the text proceeds as before and little is gained, and the information provided becomes too eclectic, limited and little commented upon’. With this critique in mind, I hope by the end of this volume to have challenged these critiques and not to succumb to such pitfalls by concentrating on the stories of those whose life histories form the basis of this thesis, taking into consideration the ‘effects of my presence on the investigation’, as is called for by Holland (1999: 476) before challenging Dyck’s assertion by postulating that ‘reflexivity enhances the most essential quality of our human existence: it is therefore the best criterion of a truly human science or practice derived therefrom’.

Ethics

Social anthropologists such as MacClancy & Fuentes (2011) and Powles (2004) have recommended that ethical observers take the utmost care during fieldwork research, but at the same time there is a call for researchers not to turn into philanthropists or aid workers. While admitting this may, in a way, taint the data that one collects, by the same token it points to anthropologists who, despite avoiding being seen as parachutists or even coming close to being seen as one, when the focus is less on the pursued information than on forging a relationship with the researched. It is, however, difficult to detach oneself from informants outside of the fieldwork and to pretend to focus only on the interviews held and be blinded to the physical and emotional stress that one’s informants are going through. One has to be prepared for the unexpected. How then do we as researchers deal with ‘ethically important moments’ (Guillemin & Gillam 2004: 265) in the field or the unexpected that would not jeopardize or compromise our research and data? As a newcomer to the field, I have come to rely on my own savvy and ethics that are morally accepted within the community in negotiating such issues. What (Guillemin & Gillam 2004: 260) refer to as the ‘home-grown’ notion of reflexivity that actually encapsulates and extends the concerns of procedural ethics. Reflexivity, as such, not only produces knowledge or enhances but, in terms of ethics, it does sensitize us to ‘ethically important moments’ (*Ibid.*: 276). In the same vein and as several authors, for example Macdonald (2011), have indicated in an effort to strike a balance, de Bruijn (1998: 81) suggests that ‘more room must be created for moments in fieldwork that do not convey information: moments of silence, of emotions’ but also moments when the researcher goes out of his/her way to assist the community. She contends that these gestures are the

right thing to do and not making them would be tantamount to unethical behaviour and there would be nothing left to bind the researcher and the researched. However, to have been indifferent to informants when they broke down during interview sessions would not have made me any different from a ‘parachutist’ whose major concern is to harvest information. Gifts and emotional empathy, while they brought us closer, concretized our relationship, allowed me to delve into the individual’s psychic and experience their emotional problems but hidden challenges, which are rare moments in fieldwork.

Such moments in fieldwork I would not consider as strategies but as serendipity. However, on the spur of the moment, I would chose to switch off my recorder and, by turning it off, I was being sensitive to their challenges. Often I offered advice and/or the option of returning later, but the latter was turned down by informants as they said it was a way for them to accept a situation or make closure. It was a healing process for them. Visiting informants when they had new babies in the family offered me a chance to share their joy and such visits were less about interviewing than socializing. I was always alert in the process but we must guard against overindulging as this may lead to a total immersion that could create a situation where it would be difficult to distinguish between researcher and philanthropist.

Selection of participants

The Pinyin and the Mankon people from the North West Region of the Bamenda Grassfields are the target groups in this research for multiple reasons. First, Pinyin has a larger immigrant group than Mankon and it was intriguing as to why they would have such a sizeable group compared with others from the Grassfields. As such, bringing in Mankon would provide a more comparative analysis and allow for us to understand why the Pinyin continue to make up a bulk of the migrant community from Bamenda. The aim was to find out, being both from the Bamenda Grassfields and sharing a similar culture, what their *modus operandi* is in South Africa and whether they see themselves as separate entities. While distinctive identities are maintained, conviviality is highlighted as seen from the extensive community, cultural and economically based network they have created that is self-sustaining and equally maintains strong economic and social links back home. Secondly, compared to other immigrants groups, such as Zimbabweans, Mozambicans and Nigerians, these immigrant groups are less well known and the form of business they practice in Cape Town needs to be given some visibility. Thirdly, their dynamism in Cape Town is varied and while the Pinyins are more adventurous in terms of their business ventures, the Mankon are very conservative. And finally, Pinyin was chosen because people from here started migrating to Cape Town much earlier than the other groups.

From the list of names I was given (cf. Chapter 1) and the contacts I made during my first meetings with Pinyin Family Movement and Mankon Cultural and Development Association, a series of conversations and interviews ensued. In the process, a survey was carried out to examine the composition of the Pinyin and Mankon societies in Cape Town. It soon became clear that their stories differed remarkably depending on their time of arrival in the country. From the survey and without random sampling, respondents were selected based on their date of arrival and the accessibility of their families back home, given that some of their parents had not returned from rural-rural migration localities that they had gone to long ago. This selection strategy was intentional. It was necessary to be able to select respondents that captured the evolutionary and variegated stages of mobility, the availability of ICTs and their experiences with ICTs, as well as fitting one of the following years of arrival in South Africa: 1996-1999, 2000-2002, 2003-present. The migrants' different years of arrival were seen as the proxies for independent variables that, I believed, would give varying outcomes in the mobility, availability, accessibility and appropriation of ICTS by the migrants. There are major differences in the migrants' experiences before the availability of ICTs for earlier groups, and the experiences of later groups are confronted with a multiplicity of choice and rapid advances in these technologies. What would such advances mean regarding mobility and the appropriation of ICTs for later migrants? And how does it depict the mobile community? One important criterion for selection was that respondents' families could be traced back to the home village or its vicinity to complete the circle.

During the selection process, I realized that women only featured in the later period (2003 onwards). What accounted for this late arrival? The first women came to join their husbands and it was only after this that they began to bring other female siblings. This also informs their transcultural family engagements, which were higher compared to those of males due to the conventional gender roles of caring that are expected of them. They are more family oriented and less individualistic.

Snowball sampling was an important aspect during and after the selection process. The few persons I spoke to drafted in their friends, especially those who travelled together to South Africa from Cameroon or those they believed had an interesting story they would want me to hear. I was also directed to meet those who were considered to be well versed in certain areas to corroborate earlier information or to get more information. This was not necessarily because they were part of the intended group of informants. I refer to these persons as 'casual informants' and to those who were regularly followed as 'regular informants'. Their role in the data collection process is 'non-negligible' as noted by Nyamnjoh (2009: 474) who maintains that 'some of the most valuable information in

research comes from the most unlikely sources, times or place'. In this regard, chance or casual meetings with members from both communities were often informative and brought out instances of everyday practices. For instance, whenever I visit informants for born-house (see Chapter 8) or to offer my condolences, I always stayed alert and it was during one such visit that I learnt about changes in PIFAM's constitution and the ban on Pidgin being spoken as the language of operationalization during meeting sessions.

Without any prior knowledge of the composition of the communities, I had no planned number of persons to interview. This only emerged after I had decided who to follow on a regular basis. Even after selection, some were dropped and new names were added because of informants' lack of time or them not being as readily available as I would have liked them to be. I spoke with 26 Pinyin migrants in Cape Town, 2 in the Netherlands and 18 families back in Pinyin village. In most cases, I had at least 3 sessions with them, and I spoke to others on numerous occasions. However, in the Netherlands, I managed to have just one lengthy meeting with each of them due to their work schedules. As for the Mankon community, I spoke with 14 people in Cape Town, 5 in the Netherlands and 7 families in Mankon. There is a disparity between the numbers spoken to in Cape Town and in the respective villages. In Cape Town I spoke to siblings and in some cases to both spouses. However, when I arrived in the village some mothers (widows) had gone to stay with their children in other cities or were ill. Nevertheless, sitting at various Pinyin junctions (*shware* in Kwidegli, Metayen, Ngali and Ndapang) provided useful information not only from parents but also from the people with whom I interacted.

Some informants are highlighted more than others. This was for the purpose of in-depth studies and analysis, and was guided by three factors. First, the information given by them was corroborated by most other informants, secondly they turned out to be very articulate and capture what others expressed with difficulty and, lastly, some of them were the core informants who readily made themselves available for conversations and interviews without appointments or when I wanted clarification on previous interviews.

In the course of data collection, some themes recurred and it is this thematic reflection that informed the various methods. In the sections that follow, I reflect further on them and, in the process, a combination of informants' voices as well as the researcher's engagement with them form the core of the section (Collins & Gallinat 2010: 7). In the remainder of the sections, I guide you through my research sites and show how I gingerly made my way into the field, introducing the various methods I used in data collection.

Methods

Negotiating multi-sited fields

According to Marcus (1995:105),

Multi-sited research is designed around chains, paths, threads, conjunctions, Juxtapositions of locations in which the ethnographer establishes some form of literal, physical presence, with an explicitly posited logic of association or connections among sites that in fact defines the argument of the ethnography.

In more practical terms, this approach involves following processes in motion rather than units *in situ*, away from an investigation of subalterns but towards one of the networks and the mobile community (MacGaffey & Bazenguissa-Ganga 2000). In the process, we arrive at an understanding of the functionality of the system and the in-betweens that would have been missed if we had focused solely on the subalterns from the perspective of a single-sited study. Multi-sited fieldwork is as much about field sites as it is about the informants that are followed. For this reason, I will offer snippets of accounts that transpired.

I arrived in Cape Town for the first time in January 2010 and it was at this time that I was conceptualizing a research proposal and thinking about applying for a WOTRO-funded grant. Having to rely on the contacts my spouse had, given that he had arrived here six months earlier, I was introduced to a few Cameroonians, one of whom was from Pinyin. Meeting him (Joe) was helpful as he told me about the Pinyin and Mankon communities and their respective associations, and gave me a handful of contacts from both associations and some phone numbers so that I could speak to them as well as to parents in the village whose children were in Cape Town. Importantly, he promised to talk to the President of PIFAM on my behalf and offered to take me along to the next meeting. I made contact with the Mankon people and had a follow-up meeting with some persons who briefed me about the Mankon association. This application stage gave me a brief insight into the community through my meetings with the people I contacted and by attending the meetings of both associations. Work only began in earnest when I was offered this research position in April 2010.

Between May and June 2010, I made two short visits to Pinyin and Mankon. Both were very informative and brought me into contact with the parents of children in South Africa. The first visit to Pinyin made me aware of the vast terrain I was working in and I had to devise a strategy in order to meet informants in the various neighbourhoods (with plans to attend two nearby neighbourhoods a day and, time permitting, three).

Joe offered me accommodation in his compound in the village. This receptive host family helped to introduce me to other parents and negotiated a guide with a motor bike (to whom I was eternally grateful) who took me around. We communicated extensively by phone to organize our pick-up and drop-off times and

sometimes he would stay with me for the entire session and introduce me if necessary. His presence paved the way for me and led to my being accepted in the community. Back in Cape Town between July and August, I increased my outreach to include members of the community and attended meetings at which the associations' political views on co- development projects, social cohesion, hierarchy and power, and *njangis* became clearer.

I was in the Netherlands between September and December 2010 and combined proposal writing and research with meeting both the communities there. In contrast to the over 100 Pinyin migrants I met in Cape Town, I met only three and seven Pinyin and Mankon migrants respectively in the Netherlands. Despite the limited numbers, interaction led to useful contrasts that are important to the study. Research was limited to Amsterdam and The Hague. One of the Mankon informants who had been in Holland the longest (John) led me to all his Mankon contacts, while I received the phone numbers of Pinyins from friends and relatives in Cape Town. While recorded interviews were at the centre of data collection here, some informants were not as forthcoming with information as they would have been if it had not been recorded. In this respect, I decided on both interviews and conversations that allowed me to glean a lot in terms of relations with family back home and with the rest of the migrant community. Watching the funeral on DVD of parents or relations was an important aspect of this study as it always led to intimate conversations about migrants' relationships with their families.

My following visits to Pinyin and Mankon were in January 2011 and between September 2011 and January 2012, while in Cape Town between February 2011 and August 2011. I started to be accepted by informants during the second visit and by the third, I was well established. My relationships with people blossomed, especially as I had met their children and had photos of them on my computer that I showed their families or even hard copies that had been sent. I would also come back with gifts and money from the children for their families. I soon became a confidante and parents showed me land they had acquired for their children who were in Cape Town and the land demarcations in the hope that I would convey this information to their children if they should die without seeing them first. As a result of these cordial relations, the apprehension towards me that I had noticed in the beginning disappeared and a lot of information was divulged even though, at times, I asked just one question. For instance, when I asked Mike's mother about her daughter's health problems, her response was 'na witch for compound' and went on to tell me about the rivalries amongst the co-wives in the family and how her daughter had been bewitched. Others talked about simmering tensions that existed in the family and at times we cried together in the course of the interviews or we took walks or read poetry to diffuse the tension.

For others, seeing me and receiving news of their children was emotional and it was almost like seeing their children, especially for those whose children had never been home since they left. For most parents I became like their adoptive child and, as such, it would have been an insult if I had visited and not shared in family meals. This sketch of how research was conducted offers glimpses into the various field sites and of my main methodological approach, namely a multi-sited approach. By the same token, it provided insight into the relationship between migrants and families at home. Such ‘deep play’ (Geertz 2005) has been considered by some (Evans-Prichard 1951 in Hannerz 2003: 209) as surrendering to the field. It naturally depends on how one looks at it but I think that this is not a tangible enough reason. It is often only by being committed and being seen as truly integrated, as opposed to being a ‘parachutist’, that one gets to know a community and in turn is accepted by them. Then one can claim some degree of authenticity regarding the data. This is not, however, to negate the fact that profound ethnography cannot emerge from a casual encounter.

Kontri Sundays (rest days reserved for non-farming activities in both villages) and market days were good moments for long conversations with parents as we sat at *shware* (the main junction of each quarter in Pinyin) over a drink and talked for long hours. At the start of the research in the villages, my tendency was to record the interviews but I soon realized that the interviews were not as profound as the conversations so I abandoned my recorder and depended exclusively on conversations and note-taking, and when I retired home in the evenings, I tried to reconstruct the conversation and attach meaning to people’s body language and tone of voice. This meant that even though I took notes, I had to devise a kind of shorthand to be able to record as much information as possible. I also had to depend on my own memory in the evenings to reconstruct the interviews and record them in diary form over my 15 months of fieldwork. I was careful not to limit myself to the conversations and interviews but also to remember the things I had seen. Memory and remembering was thus an important and integral aspect of data collection because, as Kohn (2010: 186) posits, ‘memory’s ability to construct emerges from the individual’s contact with other people and places and events’. My diary became an important companion for recording any new and emerging ideas as well as a tool in data analysis.

My regular attendance at both associational meetings in Cape Town added another dimension to my interaction as I was not limited simply to individual visits. These meetings at times provided a platform for zooming in and out, thus taking on the betwixt and between that seemed to present ‘grey matter’. Going virtual, offered the opportunity of having a look at various associational interactions and at the spatial organization that was mediated by the Internet. What I attempted to do throughout my travels was to show the network of relationships both locally

and translocally and how they were being seamed or torn apart by their appropriation of ICT. In the process, I embarked on showing patterns of collaboration between migrant communities and their home villages and amongst migrants and the challenges that occur and how these are dealt with spatio-temporally.

Being a native of Mankon, my situation was slightly different there compared to in Pinyin. Knowing most of the parents as neighbours who had watched me grow up during my teenage years when my father retired to the village made me now more of an intruder in family matters and some people were apprehensive about talking to me and confiding in me. In this respect, 'going native' at the start seemed to be my nemesis. But in due course, they began to relax and, eventually, as in Pinyin, I became a close confidante. This notwithstanding, some informants, regardless of their familiarity, were very forthcoming and I am heavily indebted to them. Although by going native I was seen as the 'other' and this questions the notion of 'going away to the field', it is only by 'establishing a physical distance from home and field that sufficient objective be obtained' (Collins & Gallinat 2010: 8). While the challenge of carrying out fieldwork at home initially seemed to concern research ethics and shades of 'at homeness' (*Ibid.*), it turned out to be contentious because of my reception by my informants. Hence I was never truly 'at home' because the 'role of ethnography is enough to create a sense of difference' (*Ibid.*: 10). How did this estrangement play out in Mankon with regard to data collection? I chose to go along with this dual identity (of being from Mankon and being a researcher). Through this, in some instances, I used previous knowledge to pose intimate questions and at other times I played the researcher and feigned ignorance of certain family issues they expected that, as a neighbour, I would already know. But importantly, I had to learn a new way of belonging to a community that I had taken for granted that I belonged to. Learning to belong, in this case, meant not all questions were explicitly asked and I adopted the habit of 'knowing without asking'. In the process, I gave meaning to it in relation to the meanings attached to the action by the participants. For example, in the course of an interview with Jessica and her husband in Mankon, an argument broke out with Jessica accusing him of never reaching out to any of their five children abroad unless they called him. She maintained she was the only one who called, especially if she had not heard from them for a long time. Connection, here, seemed to be disjointed and perpetuated the assumption that it is only those abroad who should connect with home. Nevertheless, there were instances when being an insider paid off as I could juxtapose facts and rumours (after repeated interviews), and read meaning into them. The case of a neighbour's son sheds light on this. He lived in Cape Town and was not well regarded by most in the village as he operated a 'line' that took boys from the village to South Africa. The last group of boys were repatriated, although some later went

back via other means and some asked to be reimbursed. The rumour in the village was that he was dishonest and fraudulent in his business dealings. The mother claimed the opposite and said that it was the fault of those who were repatriated and who were not smart enough to have run away from the border patrol. In South Africa, his friends thought he was not honest and was a bit shady in his business dealings. After my first interview session with him, he told me he was busy because of the nature of his business. Although his mother communicates with him regularly and he goes home as well, there seemed to be some disconnection. For this reason, I would concur with O'Reilly's (2009: 111) suggestion that it often poses a problem to 'achieve naïve distinction between insider and outsider'.

In Mankon, as in Pinyin, I focused more on conversations and held only two recorded interviews. A lot more was revealed through this form of data collection than if informants had been conscious that the conversation was being recorded and would have thought more carefully about what they were saying. For instance, on my first visit to Pinyin, I had a recorded interview with Mike's father but I could sense that this was not flowing as it did in normal conversations. I chose to keep the interview short and came back another day for a visit with only my pen and notebook. This time, we discussed matters for over three hours and it was then that he disclosed the difficulties he was having in getting financial assistance from his son in Johannesburg and from another in Cape Town (not Mike). He commended Mike for his assistance but expected more from him. After we finished, he left for a meeting and I went to meet Mike's mother in her kitchen. I wanted to believe that she was encouraged by the length of time I had spent with her spouse that she immediately asked me 'what papa told me'. In a nonchalant manner, I said 'something about the children'. Without any further questioning, she started recounting her own story about how her husband was too demanding of her son and this was causing family feuds. These conversations revealed the tensions that come with migration and the notion of entitlement parents feel they have regarding their children's finances. By the same token, this stops the children from calling regularly because of fears of demands that will be put on them. Back in Cape Town, Mike told me he usually calls when he has something to send home and mostly communicates with his mother because she is less demanding.

In Pinyin in particular, sitting around the fire in the evenings while my host prepared the evening meal was quite revealing. These were the moments when I asked the unanswered questions that had cropped up during the day, found deeper meanings in some of the figurative language used or simply told them about my activities that day. This usually led to extensive information about the

Photo 2.1 Researcher's host mother in Pinyin preparing the evening meal



people I had met. In this way, I could come to see the community from a broader perspective. I found this quite useful in my research in Pinyin where I lived with the family of a Cape Town migrant.

Despite the fulfilment I felt from interacting with the various communities and listening to their stories, fulfilment and challenge are not mutually exclusive.

The importance of the approach I adopted in this research project is embedded in the researcher's presence at multiple sites and his/her ability to analyse and interpret events that are chronologically or simultaneously linked. However, this approach does not come without its flaws. In certain instances it is difficult to be present at all sites at the same time (as in my case), especially when all the sites become sites of important activities. The researcher is thus compelled to make a choice and to rely on interviews to reconstruct the others. In other words, engagement in one area forecloses one's ability to participate in important happenings in other areas, and hence having to depend on interviews and second-hand information, whereas the physical presence of the researcher would be able to give different perspectives to the situation. For instance, two weeks after I left Cape Town for Cameroon to continue fieldwork in Pinyin and Mankon, Ron had an accident and later passed away. I did not hear about this although I was in Pinyin. Having worked for a few weeks in Pinyin, I then moved to Mankon and his body was brought back to Pinyin for burial in the meanwhile. I came back to Pinyin and learnt about it but the funeral had already taken place and I had

missed out on all the events and had to rely on interviews to construct my analysis. Similarly, with families and relations that span home and the host country, it is not very easy to capture their story in a fluid form and in its entirety as the story changes depending on who is telling it. By the same token, participating in most activities turns out to be monotonous (Hannerz 2003).

At a personal level, it would be foolhardy to claim that the globetrotting involved in the course of this study was easy. It was not. Long periods of absence from my family were difficult to cope with and towards the end of my stay in any of the localities other than Cape Town, my motivation decreased as homesickness and nostalgia crept in and affected my attention span. It was difficult to write even a paragraph at such times and, faced with such emotional challenges, I could understand those of my informants only too well. Going home I felt as if I was a prisoner who had been released on parole because the writing still had to continue once I was home. Having to weave into my studies the role of wife and mother was quite a challenge and I often felt guilty when my children wanted to chat but I was busy writing.

This notwithstanding, when I returned to Pinyin and Mankon, I touched base with all my contacts and was given news about all that had happened in my absence. This was not necessarily to harvest information but was a way of being committed to the welfare of the individuals that I had come to know. I paid condolence visits to some and visited new-born babies and their parents too. The scenario was different in Cape Town where I was in constant communication with some by Skype, on Facebook with others and had email contact with yet others.

Comparative analysis

The comparative approach adopted in this project was an integral part of my research method and juxtaposing both societies helped to understand their commonalities and differences and how these traits could help to map out their mobility trajectories and unpack their appropriation of communication technologies. The aim of this approach was to gather different insights into the mobility patterns of Pinyin and Mankon migrants as well as to discover the social construct of their societies in different cases and capture their complexity. Based on historical and contextual comparisons, I was able to draw nuanced conclusions given the progressive nature of the study. This approach established the absence of a 'common cause or effect, even if all circumstances are identical' (Rihoux & Ragin 2009: 2). The approach does not deviate from the qualitative aspect of this study but instead complements it. And it allowed me to examine constellations, conjunctures and configurations (Ragin 1987). The configurations were the determinants that produced a given outcome (Rihoux & Ragin 2009) and through

these, I was able to address the ‘why’ questions about ‘outcomes resulting from multiple conjunctural causes’ (Ragin 1987) that characterize the different communities. In this regard, it provided a basis for making statements about my empirical regularities and for evaluating and interpreting my findings, as well as room to go virtual and look at the Pinyin and Mankon Internet-mediated news forums. Following an historical approach, I looked at specific experiences beginning with the various mobility patterns of the Pinyin and Mankon and how differently these evolved. How have they informed the present and what identification can be discerned from them? I went on to look at the arrival of migrants in the host society and at how their arrivals coincided with the various phases of communication evolution. What comparisons was I able to make from the absence and presence of ICTs in the lives of the migrants according to the time of their migration?

Confidentiality

The issue of confidentiality was of concern to most participants, especially those who were better educated, and were constantly asked how I planned to use the data I had collected. I thought long and hard about this and about not infringing their privacy. And for this reason I decided on pseudonyms. In the same vein, most requested anonymity and so, prior to any recorded interview, I assured interviewees of their anonymity and promised them I would use pseudonyms. With some, I went back to crosscheck the information I had been given during interview(s) to be sure they were comfortable with the use of certain parts of it. Once someone explicitly specified what should not be used, which I naturally respected. This kind of attitude is crucial if one is to uphold the trust vested in the researcher and to be able to build up further confidence in the relationship. Abiding by strict ethical concerns insofar as confidentiality is concerned was less for academic reasons than for the benefit of our mutual relationship throughout the period (see Nyamnjoh 2009: 373). However, one major concern was how to submit to ‘narrative hospitality’ in certain instances or to be honest with information that would reveal the identities (such as recounting information about unsuccessful migrants) of some informants by virtue of their reputation within the community in spite of using pseudonyms. Choosing not to tell this side of a story would be falling prey to presenting a single story that was not balanced. Nevertheless I chose to go for a complete story and found reassurance in Rapport’s (2010: 90) advice (see also Guillemin & Gillam 2004) where he propounds that:

.... ethical and intellectual compromise are intrinsic characteristics of social research and whichever choice is made is unlikely to bring complete satisfaction ... the fieldworker is he or she who learns to live with an uneasy conscience but continues to be worried about it.

On my first trip to Pinyin, I was introduced to Mbu Joseph, who agreed to talk if I dropped by a day later. I started recording but soon realized he was not comfortable so I took notes instead as we conversed. Formal interviews and recordings in both villages seemed to make my respondents uncomfortable, hence I opted for conversations. This also meant I could spend long hours with families, although I tried to steer the discussion and sometimes digressions were apt. The preference for conversations in the village revealed more information as people tended to be relaxed. This in no way suggests that the formal interviews did not achieve their intended purpose. Returning to Mbu Joseph, he talked about his son who went to Cape Town hoping to assist him with his other children, but the father was woefully disappointed. He became sick and was unable to get proper medical attention. Suddenly, he changed the conversation to himself, telling me how he would have been in a senior position in the military today had his father not dissuaded him from going and urged him to marry. He said ‘I have never really told this to anyone, I regret not having joined the military, I would not be here today suffering; I emptied my account to send this boy to South Africa and all the promises he made about helping with the rest of his siblings have come to nothing.’ That said, he turned his face from me so I did not see his tears. I offered him a hug and proposed that we take a walk to *shware* (the junction in the quarter) where we had a drink. There was a similar situation with Pa Anyam in Bamenda who cried during the course of our interview because his son has gone to the US and had not come home in 25 years: he has seemingly (dis)connected from the family. His son’s lifestyle in the US broke his heart and he is unable to tell his mother about it as it would kill her. I stopped recording and read some of his poems and we discussed the themes. This calmed him down and we continued.

In Cape Town, some informants talk of the simmering tensions between them and their in-laws who are constantly making demands on them or feel that their son (spouse) spends all his income on his spouse and her family. Of the over ten meetings I had with Joyce (none of which was recorded) because she had a lot to tell but was not organized in her telling, we engaged in conversations both at her business stand and at her home. I did record one conversation with her husband and three with CJ about how he came to have a South African passport. People like him, having just returned from fieldwork himself, confided in me because they understand the importance of research.

The case of Lola (in Pinyin) whose husband is in Cape Town is worth mentioning. She got married at the age of 16 while still in secondary school with promises that she would continue her education but these turned out to be empty promises as she started having children soon afterwards. Her husband travelled to Cape Town when she was six months pregnant. According to her, she has been

mistreated by her in-laws ever since his departure. All the land she used to farm with him has been usurped by them and she showed me crops that she planted in front of the house and that were destroyed by one of her brothers-in-law. Money was being sent to her through another brother-in-law and he decides how much to give her or gives her money when he thinks the last consignment must have been exhausted. Her husband in Cape Town says he looks after her and the children very well and sends them a monthly allowance of FCFA 10,000 (clearly not enough) and sometimes more at Christmas, in September and at other festive times of the year. She would love to go back to school or to Bamenda to do some business but no one, including her husband, wants to give her a chance. Her parents often have to assist her with food and palm oil, her two daughters looked malnourished and untidy, and I sensed some kind of tension each time I visited them. On my last visit, one of her brothers-in-law (the person who destroyed her crops) interrupted our conversation for over an hour when he sat with us as (I correctly guessed) he wanted to know what we had been discussing. I stopped the conversation and we discussed life in the village. He tried unsuccessfully to send Lola on an errand but she retorted that it would be rude for her to leave me when I had come to visit her. During our conversation, I could feel a certain tension in her as there were long moments of silence and at one point her voice cracked. And when she talked about her destroyed crops, her eyes were full of tears. It was often during these moments of silence that I listened to the unsaid and felt the outpouring of emotions, especially of regret, and feelings of abandonment.

Feelings

In these periods of fieldwork there were difficult moments to draw a fine line between being a researcher and an emotional person who empathized with the stories being told. I was particularly touched by Lola's story because she never looked happy when I met her. On all three meetings with Lola as well as when I met Jessica and her husband, I could feel the emotions, tension and (dis)connections that were being brought to bear on families by migration and mobility. Although I had my eyes wide open, I needed the assistance of other senses to make a holistic analysis of what was unfolding in front of me. Concurring with Simmel's view (1997 in Urry 2007: 24), I too felt that the eye is a 'unique sociological achievement ... it is the most direct and purest interaction that exists ... and it produces the most complete reciprocity of person to person, face to face'. I could go further and add that observations go beyond using the sense of sight as this is just one of our senses and observation calls on us to employ the rest of our senses too to be truly enmeshed in the field. While conversations enabled me to learn more about their daily lives and facts omitted in some interviews, they also helped me to fine-tune my questions and this gave leads in other directions.

Language

One important thing that worked in my favour was my ability to speak Mankon and understand both languages, confirming Marcus' (1995: 101) point that the ability of the researcher to speak/understand the language impacts on the quality of the fieldwork. Being culturally tuned in and speaking and understanding the Mankon and Pinyin dialects respectively positively impacted on the research, as code switching was quite common, especially during intimate conversations when the interlocutor did not want to use Pidgin English. They would then adequately express their thoughts in the local dialect or in expressions that precisely captured the point. This was the case with Jessica's husband who was disgruntled when his son asked for money from South Africa, and he summarized his frustrations in his own dialect. Translated it would read as follows: 'Children go overseas but mine has gone to *ndzimabuh*', (a quarter in the village). This statement adequately captures his frustration at his son not being able to send home remittances and instead asking his father to send him money. Going to the next village means there is not much there, hence his father still has to support him financially. I was comfortable about having interviews in the local dialect but I did not go beyond basic greetings and words of welcome. This could be explained by the fact that the ethnographer they saw in me enhanced their perception of me being 'not native enough', as speaking the dialect seems not to make me as native (which I thought I was) in their eyes (Collins & Gallinat 2010).

Photos

One feature of my approach was collecting photos and listening to the stories behind the photos. My digital camera allowed me to take photos and make short recordings simultaneously. However, I soon realized that making videos shifted my attention away from social interaction (see Cooper *et al.* 2013) and I focused on taking photos, although this also meant that I had to be careful not to miss out on any key moments. The camera provided a significant service by recording a scene that made me contemplate more deeply or re-examine what had been said.

Some of the photos were scanned and the originals returned or those that were sent through me to kin in Cape Town or Cameroon were scanned with the permission of the sender. These photos were full of memories and meaning for both the sender and the recipient. For me too, it was crucial to know the way in which the data had been gathered and some served to document research sites and as mnemonic devices to reignite my memory later. It is in this regard that when I wrote each chapter, I often began by going through the photos on my computer. They were the perfect reminder of specific scenes and triggered memories, added detail to a story as well as being testament to having 'been there' (Collins & Gallinat 2010).

Some photographs also showed contrast between old and new or changes (see Chapter 4). As noted by Bourdieu (2012: 4), photographs were ‘materializing and memorizing observations’. Some were not necessarily new photos, but ones which has been taken before the relation migrated and they were sent alongside recent photos. I suspect the intention was to see how those at home had grown. When the photos were delivered, I studied the attention and behaviour (mannerisms) of the receiver when looking at all the details on the photos, listening to his commentaries and the emotions expressed by the receiver. These were often of nostalgia, mixed feelings at times of joy and sadness about being away from the family but at the same time being able to see them. The photos Lola sent to her husband was the first opportunity he had had to see the child that his wife had been pregnant with when he left home. She was 14 months old on the photo. Lola’s husband looked at the photos of Lola and the two children pensively, and I sensed feelings of regret and separation when, in reference to the younger one, he exclaimed that *‘na ma pekin don big so wey I never take eye seeam!’* (My child has grown so big and I have not been able to see her up till now!).

Similarly, the most recent photos Joyce had of her sons in the village with her parents-in-law were over two years old and had been brought by her husband when he visited. Before I left for Cameroon, she pleaded with me to take photos of the boys while I was in Pinyin and bring them back for her. She immediately went with me to the photo shop and printed out all the photos of her family (her father, parents-in-law and children). Her joy at seeing the children was obvious and she proudly showed them to her neighbours in the market, all of who commented positively on their suits and shoes. When I later visited at her home, I found she had enlarged the photos and hung them on her wall. Through such acts much is revealed about the psychological state of mind of the receiver, the nature of their relationship and whether it is intimate or casual.

By the same token, I took photos from Cape Town to Paul in Bamenda. He lives in Cape Town but had travelled to Cameroon on a business trip for five months and his wife sent some recent photos that had been taken at the beach of her and their son. When I handed the photos to Paul, he took a long look at them and exclaimed; *‘wey! my woman don dry finish because I no dey for ee corner’* (‘my wife has gone pale because of my long absence’). One could sense remorse in him for staying away too long. But he was happy to see that she could distract herself, as well as keeping their son happy and that she was able to enjoy an outing to the beach. In the end, he told me that he has to go back to Cape Town by November in order to be with them. And the photos that Emile in Holland sent to his mother (Chapter 9) express her joy at having lived to see more grandchildren, even though only virtually. Paying such close attention resonates with Geertz’s

(1973: 17) suggestion that ‘it is through the flow of behaviour or social action that cultural forms find articulation, as well as in various sorts of artefacts’.

Similarly, I collected photographs, scanned and returned them (Chapter 9), re-snapped enlarged photos on the walls, and took lots of photos of happy occasions and the participants. These photos in themselves told stories and brought joy to families at home and in Cape Town when I showed them. Most of those in Cape Town opted to make prints. These photos form part of what Hammersley & Atkinson (2007: 212) refer to as documentary construction of reality wherein ‘documentary sources construct “facts”, “records”, that are crucially involved in social activities’. Also, they are expressive forms of virtual presence and connectivity. Conversely, in such circumstances, the photos highlight the geographical distance between migrants and family as families express nostalgic feelings about when they will see the each other, ‘maybe s/he will come when I am gone’ was often said by parents. But the one thing that keeps them alive is the hope of seeing each other and the photos act as a substitute for missing kin.

The use of ICTs in fieldwork

The mainstay of multi-sited research is its ability to follow the researched in their daily activities. However, respecting this rule meant I was out of action at some field sites for a while due to my presence at another. I had to develop a Janus-like approach to be present at all sites. In this respect, ICTs helped to fill the void, especially when I was in Holland as I often communicated with informants in Cape Town then too. While communications within the villages were not too regular for parents who were often on their farm where there is no network reception, I nevertheless sought to be virtually present in Cape Town through phone calls, Skype and visiting informants’ Facebook pages to see what they had been up to and to get news of events in Cape Town. I depended on my phone for my daily programme and to make calls and schedule or confirm appointments. I made calls to informants to clarify interviews when I was writing them up, which saved quite a lot of time. I received SMS information about upcoming events and also invitations and this was by far my best source of connection with all my informants. This was also my GPS navigator (Tom Tom) for getting to the homes of informants. And it also meant that if I was unable to reach informants, it made contacting them difficult and I took the chance of going to their homes at weekends to meet them. If I was lucky, I would find them. In summary, the phone was my ‘Swiss Army Knife’ (Ling & Donner 2009).

The Internet was key in data collection. Exchanging emails with informants meant that there was regular communication even when I was physically absent. Skype conversations with Joe would update me about the Cape Town community and developments in the village (Chapter 9), while phone calls with Joyce and

Paul would keep me abreast about happenings in the PIFAM and MACUDA respectively. My knowledge of Internet-mediated associations was gathered from the group emails that were posted on the Pinyin News Forum (PNF) and Mankon Forum (MF). The reason for checking these mails was to access the extent of mobility, virtual connectivity and connection with the home village.

Although I was not very regularly on Facebook, I did meet some informants there and we had brief chats. I took a keen interest in going through some of the photos and the comments posted, especially those brought from home. Facebook also enlightened me about migrants' virtual connectivity and this was by far the most frequently used means of communication by those who were well versed with the Internet and had smart phones. Most of them had parallel groups of friends. Chris, for instance, who belongs to four different Facebook groups, was interested in football and Cameroonian politics, and updated his page almost on a minute-by-minute basis.

Life histories

A detailed ethnographic study requires delving into life stories to piece together the past and the present so as to have a story in the continuum and to understand whether the present is informed by the past or if there is a disjuncture. As such, *quasilife* histories (that do not have all the attributes of a life history) draw extensively on memories and depend on how far back the informant can dig into his/her memory to X-ray the past. While part of the aim was to string together generational history, it goes further in 'illuminating the social and constructive elements of an individual that make him or her potent in the social context' (Marcus & Fischer 1986: 183). This allowed me to juxtapose mobility, trade and networks of the past with those of the present migrants and to come up with an analytical framework of transformation and continuity. Given that mobility and trade are dynamic processes, this approach was used to establish social practices of the past and link them to the present to ascertain whether there was any continuity or disjuncture in these patterns. The importance of the stories is a means of appreciating how various events and their relationship fit together. Collecting life history interviews with migrants in Cape Town, the Netherlands and families back in the respective villages of Pinyin and Mankon was done to understand and explain the mobility patterns of the migrants. Recording or recounting life histories was often integrated in interviews and conversations. In the host countries, I would usually begin a session by asking about the individual's life before coming to South Africa and the family background for those who had one living parent. They then recounted what they remembered about their fathers as well as stories they had been told. The information on Joe's father (Chapter 3) was, for instance, narrated to me by Joe himself. Some of it he had experienced first-hand and the

rest he had been told by his father. In the home villages, I asked about people's parents' lifestyles from when they could remember. Often, they talked about how they tilled the fields and later joined the Fulani as herdsman and from whom they 'earned' a calf each year. In recounting these stories, the parents back in the village were meticulous in reconstructing the journeys of the bush traders, the various stops that were made and their duration. Bianca, like Joe, narrated the story of her father's trade connections. He had been a cattle trader and would travel from Pinyin to the cattle markets in Mesaje, Sabongari and Nkambe to buy cows to resell in Bamenda and as far away as the South West Region, and he had also kept some in his herd. This meant that the family would go for weeks without hearing from him and he would occasionally send a message through a fellow Pinyin trader.

Juxtaposing the life-history accounts of those in the village and in Cape Town shows how most Pinyins in Cape Town have continued in the lines of trade practised by their parents. The same does not hold in the Mankon community where people have followed patterns quite diverse from those of their parents. When we review the stories recounted, the emergent theme that runs through them is the different pattern of mobility. Their stories (see Chapter 3) describe a plethora of encounters in different places and with different people. Despite a tendency to be mobile, the feeling towards home and to those there is still strong. While the availability of new technologies has removed the distinction between the village and the city, such distinctions are being challenged when they are overwhelmed by emotions.

When conducting life histories as interviews, it was difficult to be detached from my informants' emotions. Many families were happy to welcome me into their homes because my presence made them feel a little more connected to their migrant kin. Equally, the interviews were a painful reminder of the distance between them and their kin (especially wives left behind) or the disconnection between them and their sons. Such periods often brought out raw or subdued emotions that had been piling up and that were expressed by the shedding of tears, but they were determined to carry on with their stories. Telling their stories had a healing effect. At the end of the story sessions, some parents volunteered to show me the piece of land they had reserved for their sons and the demarcations, with instructions about what belonged to who. And I became a 'trustee' of their oral testament and had to transmit it to the children in case they did not see them again. I liked to believe that telling me their stories was less for the purpose of the research than because they viewed me as one who was close to their children and that I could adequately express how they felt. Proof of this was when I was able to reconnect a parent and their son, and the latter later completed the house the father was building and provided assistance with opening a small shop. Upon

my return to the village, he continued his story from where we had left off. As a result of the relationship that we had built up, it did not take any prompting from me for him to start talking and he acknowledged my input in reconnecting him and his son, although this lasted for a limited time only.

Archival research

Archival research is important in linking the past with the present and to deciphering what changes, transformations or continuity is taking place. By so doing, this study called for ethnography that is historically oriented and that ‘dealt with complex as well as primitive societies and that recognised culture contact, movement, and change’ (Vincent 1991 cited in Gupta & Ferguson 1997: 20). Parts of this study were based on archival research as an important step to establishing previous mobility patterns that could shed light on the present but equally on how these patterns had been largely shaped and defined by ICTs and the revolution in transport. At the regional archives in Buea, I went through administrative reports on Bamenda and Victoria Provinces to gain insight into mobility and communication trends at the time. Prison and taxation records show that Pinyin traders were heavily engaged in long-distance trade and that one of them was arrested for evading taxes from bush trade and another was sent to jail, while yet others had to pay fines.

The juxtaposition and simultaneous use of these varying methods were all geared towards telling the stories and capturing the voices of those being researched, with the aim of highlighting their life trajectories in ways that have, until now, only been told piecemeal.

Data analysis

As already mentioned, the data collected are qualitative and include the words, silences, events, actions and experiences in the everyday lives of the actors. Interviews were done in English and Pidgin English. For the most part, the Pidgin transcriptions have been used as such and, where necessary, the English meaning of the quotes is given. Following Mauthner & Doucet (2003: 422), part of my approach in data analysis is a ‘voice-centred’ focus, which is equally hinged on reflexivity but is not reduced to a simplistic representation of the respondents’ voices without any attempt to offer interpretation and/or analysis (*Ibid.*). This approach allowed me to measure my level of emotional relatedness towards the informant while at the same time trying to understand and interpret the informant’s story without tainting it with my own perceptions and assumptions. Nevertheless, there were times when charting my own migratory story and challenges helped to reduce the shyness of the informant. This approach was particularly useful given the fact that I am not only culturally in tune with both cultures but

also hail from Mankon, which could result in me overlooking certain narratives or questions. Over and above this, the method, as indicated by Mauthner & Doucet (2003: 422), holds at its core the ‘idea of a relational ontology in which conceptions of the separate, self-sufficient, independent, rational “self” or “individual” are rejected in favour of notions of “selves-in-relation” or “relational beings”’.

Given the duration of the data-collection period, it would have been easy to fall prey to the standard three-phase formula of data analysis: data reduction; data display; and conclusion drawing or the verification process. However this study benefits from Hammersley & Atkinson’s (2007) suggestions that reducing, condensing data, identifying or isolating groups of common categories and meaning making should begin as the study begins and continue through the duration of data collection to the writing of the thesis. They advise that ‘in ethnography the analysis of data is not a distinct stage of the research ... it begins in the pre-fieldwork phase, in the formulation and clarification of research problems, and continues through the process of writing reports, article and books’. It is precisely for these reasons that my research dairies have become my ‘Bible’ and I have been reading and re-reading the notes I took to develop and analyse what I heard and saw. One additional point that was helpful in the analytical framework was memory – visual, aural and emotional feelings – that formed part of my everyday writing.

Conclusion

Methodological scripts often follow the traditional patterns of writing these sections such as observations, interviews etc. And, as I demonstrated above, I chose to adopt the themes that were outstanding during data collection and to expound on them following my interaction and experiences with the communities.

Working with mobile communities in three loci where activities are constantly on the move required more than the Malinowskian intensive model, i.e. flexible methods beyond traditional ethnographic ones. It called for a multi-sited approach to capture these mobile communities on the move. This means that much as the research was focused on ICTs, communication technologies became an integral research tool for negotiating multi-sitedness, understanding the mobile community that by definition is difficult to grasp, tracking down informants and simply ‘linking up’ to improve relations between the researcher and the researched. By juxtaposing Internet-mediated technology, social media and telephony, I was able to begin to grasp migrants’ mobility and appreciate the intricate web of connections that they had built and were continuously forging. As a result, I established a virtual and Janus-like presence at the field sites when I was physically absent through constant communication.

I became a perfect bridge that linked mobile communities at home and in host countries as a result of my movements thanks to my research funds. Families were linked through the courier and the photos that I carried back and forth and these also became units of analysis. The same goes for the photos that I took. These flexible methods illuminated a rounded understanding of mobility in the face of advances in ICT as well as the power relations that ensued and the concept of social capital (cf. de Bruijn & van Dijk 2012).

Comparative analysis in research is not an entirely innovative technique (MacClancy & Fuentes 2011). However, by dint of the fact that this study spans Cameroon, South Africa and the Netherlands and features two ethnic communities, it called for a new and creative way of looking at comparative research. In this respect, this study has been innovative in its methodological considerations. The chapters are interspersed with ethnographic quotes from informants and I use them extensively because they are ethnographic sources that reflect my insights into the sociocultural and economic meanings of both communities.