Balancing men, morals and money: Women's agency between HIV and security in a Malawi village
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Knitting narratives:
Research methodology

Introduction

To assess the validity and value of any research finding, knowledge of how exactly it was attained is a first requirement. In the case of this study, the data collection process has been far from straightforward. Because of these two reasons I feel it is justified to devote a full chapter to elaborating upon my research methodology.

The importance of methodological accounting became particularly clear to me when I began to notice that the various research methods I used led to divergent and even contradictory outcomes. My research results were apparently conditioned by the methods through which they were collected. This was obviously frustrating and problematic, yet at the same time informative, as will be explained further on. To enhance the accuracy of my findings I switched between and combined different methods, ‘triangulating’ my data.1 Some of the methods I applied intentionally while others developed spontaneously. In this chapter I discuss each of the methods used, as well as other factors that have been of relevance to the process of data collection and analysis.

The lack of a predetermined structure that characterizes this study’s data collection process was intentional, at least to some extent. I did not want to restrict my investigation to the topics that I presumed were potentially related to my research question, rightfully fearing that this would lead me to miss out on important but unexpected explanatory factors. Instead I wanted to grasp as fully as possible all of women’s daily life considerations, in order to be able to frame their sexual choices within the appropriate wider context. Although the direction of the research was instigated by a clear hypothesis to be tested, I had no particular set of

1 An example of such triangulation already appeared in the previous chapter, where I described how I combined qualitative and quantitative data to categorize Mudzi households based on their relative position on the poverty spectrum.
theories in mind to guide the data collection process. In the social sciences, such an approach is called ‘grounded research’ – meaning that theory is formed during and through the analysis of field data and relevant literature (Glaser & Strauss 1967).

To allow for such a holistic point of departure, I opted for a classical ethnographic case-study approach. According to Robert Yin (2003) the case study approach is particularly suited for studying complex, contemporary social phenomena that cannot be easily distinguished from their real-life context. The interaction between livelihood insecurity and sexual relationship choices may well be considered such a phenomenon. By restricting the study of complex, interwoven phenomena to a bounded case, it becomes manageable. In this study, the ‘case’ is one particular village community that I have called Mudzi. The classical ethnographic aspect of my case study approach entailed living among the research population for an extended period of time, building rapport through informal interaction and participation in everyday activities, and so gaining insights into the overall daily lives of those under study. By making these choices, I traded off generalizability to in-depth thoroughness, efficiency to inclusiveness, orderliness to flexibility, systemacity to serendipity. I did not anticipate the hardship that came with this endeavour, which required surrendering to methodological uncertainty and full submersion into a tough field, but on hindsight I believe it has been worthwhile.

The harsh living circumstances in Mudzi, described in the previous chapter, led to a precarious research situation. Studying a population in crisis, or on the edge of survival, triggers ethical doubts, feelings of guilt and shame, and internal conflicts about longing to help while also wishing to understand how life is usually lived without that help. The fact that I embodied potential help and hope further complicated things. Villagers shrewdly attempted to gain support through me by (over)emphasizing their deprivation and enviously competing with each other over my attention, keeping a close watch on my every step and especially anything that might reveal the privileging of another villager [e.g. P2 0155, 1206, 1227, 1475, 1637;}

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2 In its most radical form, grounded research is not preceded by any hypothesis or literature review. As described in Chapter 1, this does not apply to my research. Nonetheless, my choice to let the fieldwork (to a certain extent) ‘take its own course’ instead of departing from well-defined theoretical and methodological frameworks, makes it appropriate to use the classification ‘grounded’.

3 I had done similar fieldwork before in Guatemala, where I spent six months living in a small village community to conduct ethnographic research. However, for reasons discussed in this chapter, I experienced the fieldwork in Malawi to be much more taxing.

4 An example of this is the Mudzi woman who during our fieldwork gave birth to twins. Soon after this, she was left by the babies’ father. The woman’s breasts were clearly too empty to feed both babies properly. I found it very difficult to refrain from helping this woman by buying her some cans of milk powder. Other women must have noted my temptation and were very explicit about its repercussions, threatening that they would all come to demand support if I would help this one mother, as they all had babies and they all suffered [P2 1206, 1227]. And they were quite right at that. It would have been unethical, not to mention jeopardized my position, to help only some while not others. Eventually I did find ways to assist some women that I pitied in particular, including these twins’ mother among others, by paying them to help us fetch or cut firewood, or recruiting them as research assistants (see Chapter 7).
These factors called for inventive research methods and constant caution about safeguarding my position as an independent researcher equally interested in all but without intention or means to actually help all. The harsh living circumstances furthermore challenged my study results simply because part of my energy too was directed at mere survival.

A significant role in the data collection process was played by my research assistant Gertrude Finyiza, a young Malawian woman who responded to the job advertisement I placed in one of the national newspapers. She turned out to be much more than the Yao translator and housekeeper that I was looking for, assisting me also as a cultural broker, data collector, and friend. More than I, a white foreigner unaccustomed to most of what our informants considered normal, she became an entrusted and valued ‘fellow villager’, who always managed to uphold a balance between assimilating with the community and maintaining a detached position as researcher. Her ‘insider’ stories about what had been discussed at the water pump or by villagers coming by for a chat with her, the conversations of the many women that eventually joined me in being taught how to knit by Gertrude, and her reflection upon what occurred around us have all been of invaluable help to my understanding of Mudzi life. In this chapter I therefore also elaborate upon Gertrude and her indispensable role in this study. Whether or not to include the casual remarks that villagers made to Gertrude, some of which may have been intended as confidential, into my data set obviously led to ethical deliberations, which too are discussed in this chapter.

Research approach

Public health research, including that related to HIV and AIDS, continues to rely heavily on survey-based methods for data collection. These methods have clear advantages over qualitative research, such as the already mentioned efficiency and potential for large scale, but also the relatively easy analysis of (numerical and comparable) data (see also Carvalho & White 1997, Scrimshaw 1990). However, as argued by many other social scientists, in Mudzi I found that quantitative data on its own, without sufficient contextualization and triangulation, was inadequate for obtaining a reasonably representative impression of (an aspect of) village life. In this section I describe the study’s data collection process, including the problems we encountered with structured interviews, our attempts to overcome these, and the leaps of faith that together shaped my research methodology.

I have often clarified my research purposes in Mudzi, repeatedly verified villagers’ willingness to participate, stressed that they were free not to answer our questions,

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6 By ‘structured’ I mean that the same set of questions was asked of each interviewee (as Bennett & George 1997: 3).
and asked permission to use their photographs in the final research report. Yet I am quite sure that most Mudzi villagers have no means to fully understand what they consented to (see also Zaman & Nahar 2011: 154). Many might still agree if they did know what exactly their permission entailed – they might care less that their names and faces would be circulated among a small group of academics far away, while others might even feel proud about it (see also Van der Geest 2011: 148). Nevertheless I consider it right to safeguard their privacy by changing all names, including that of the village itself, and to not link names to the pictures that I use. Further personal information of informants, such as age, marital status, and number of children, has mostly been left unaltered. Only in a few cases, when recognition would be possible on the basis of some unique details, have I changed such details to protect the privacy of the person(s) in question.

Moving to the village
Before Gertrude and I settled in Mudzi, I undertook two village-level studies commissioned by ICRISAT concerning farmers’ adoption of one of its improved groundnut varieties. My supervisors at the institute felt uncomfortable with me staying overnight in localities without facilities that were close to middle-class, urban standards, and, after many attempts to persuade them, agreed only if my expeditions did not take longer than one week at a time. In total I spent six weeks in the two villages, one in central Malawi, the other in the deep south. These studies allowed me to acquaint myself with village life and so functioned as a pilot study for the longer-term research in Mudzi.

In the two pilot study villages I lived with a family and could observe the daily life routines within a household – something I was not able to witness as up-close and personal during my stay in Mudzi. Despite the privilege of being part of a family, I found it taxing to constantly be at the whim and mercy of others. In Mudzi, I therefore requested and was allocated a private house for Gertrude and me by the Group Village Headwoman, on the border between Mudzi A and B, next to the path connecting the two villages. The house, which belonged to the Group Village Head’s son now working in South Africa, was one of the better-looking houses in the village: relatively big, divided into four rooms with half-high walls, and with an iron-sheet roof. One of the rooms had been used to lock up the goats at night, and it took quite some time before we got rid of the smell. In this room we stored our pots and hoes. Gertrude and I both had our own separate bedroom.8 We

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7 As I describe further on, during the three years prior to undertaking this PhD study I worked in Malawi as a social scientist at the International Crops Research Institute for the Semi-Arid Tropics (ICRISAT).
8 I had intended to be sleeping on a bamboo mat, just like most villagers. To my relief, however, I found a bed standing in the otherwise empty house. Only when guests came to visit (several curious, in-country expat friends, and advisor Sjaak van der Geest), did I take out my bamboo mat to sleep on.
were supplied with a table and two chairs, and I had brought a cupboard that could be locked to safeguard our belongings – these filled the fourth space.

Like most houses ours had a veranda in front, and two separate little structures behind it. One contained firewood and three stones to cook on, the other many insects and a mere hole in the ground – our pit-latrine. In front of this latrine a grass fence was put up, behind which we washed ourselves.

None of the houses in Mudzi had running water or electricity. Gertrude went to fetch water at the water pump almost daily; I joined her only occasionally – for reasons to be explained later. To charge my mobile phone and the small laptop I brought for data storage, I installed a solar panel on our roof. Despite the many hours in full sunlight, it took about two days of charging to have the laptop running for two hours. Most of our data are therefore handwritten. Towards the end of the fieldwork period I hired an assistant to enter Gertrude’s and my journal entries into the computer.

I paid the Group Village Headwoman a monthly rent of 2000 Kwacha, almost 10 Euros at the time. For this price we also had access to some of the land around the house, on which we planted maize, pumpkins, peas, groundnuts, and sweet potatoes. After some time in the village, I ordered the local bike mechanic to make
us two bicycles from the parts he had laying around, which took him some time because he did not have enough parts for two full bicycles and needed to travel to Balaka several times for extra parts. But when he had finally managed to assemble the bikes, we spent our time more efficiently as it took less time to visit far-away households, the markets and clinics in the area, and even Balaka (a good two-hour, bumpy ride).

My arrival in Mudzi stirred up quite some commotion, of which I only came aware later. It made the Group Village Headwoman and the Village Heads of Mudzi A and B nervous because they felt responsible for our safety while fearing that the presence of a mzungu in a simple, unprotected house would surely attract malevolent attention [P3 0256]. Also lay villagers worried that my presence would attract thieves to their community [P2 0754]. Only upon our departure did the Group Village Headwoman tell us that she had been called twice to the sub-Traditional Authority (one rank above her on the hierarchical ladder) to justify her acceptance of my presence, because she should know that azungu are notorious for blood stealing [P3 2848]. Other villagers too were scared that I had come to steal their blood [P3 0572, 2360]. Not surprisingly then, several asked Gertrude if she was not afraid to share a house with me, and whether it caused her nightmares to sleep so near to me [P2 2071]. A number of young children burst into tears each time they saw me, making me suspect that their parents used me as a bogeyman in their child-raising endeavours.

As time went by, the initial anxiety calmed down, until eventually, towards the end of our fieldwork, women began to worry about our upcoming departure, wondering out loud what they were to do with their afternoons if they could no longer come to chat, knit, or play a bawo game at our house [P3 0890, 1425, 1740, 1923].

**Trial and error data collection**

The first mistake I made when starting my life in Mudzi village was to come in an impressive four-wheel-drive car with the ICRISAT logo on its doors. After this it did not matter how often I emphasized that I was sent NOT to start up a project but merely to understand what daily life in a Malawian village entails. The simple fact that I was connected to an organization made many villagers believe that one day I would confess that I had tricked them about my neutrality so as to spy on who really deserved support and who not, and help those identified as needy [e.g. P2 0071; P3 0537, 0557, 1019, 1532, 1936]. They hoped and expected that I would bring them maize during the hunger season, fertilizer at planting time, and blankets during the cold season. But I disappointed them time and again. Some argued among each other,

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9 A Swahili word originally meaning ‘someone who wanders around aimlessly’, now also in Malawi widely used to denote white foreigners. Plural: azungu.
Gertrude overheard, that I had indeed emphasized from the beginning that I would not come with handouts or start a project [P2 1206, 1227; P3 2487]. Yet they all found it difficult to understand why on earth I would give up my surely comfortable life to suffer with them, asking them so many questions about their impoverished lives if it were not for some form of aid [P2 0071; P3 0537, 0557, 1532]. This suspicion logically influenced the image that my respondents wanted to create of themselves.

It took some time before I noticed the discrepancy between the answers I got during my interviews and what I observed. After I was granted permission by the District Commissioner, the Group Village Head, and Village Heads to undertake my study in Mudzi, I requested all inhabitants to be gathered. I had written and learned by heart a full speech in ChiChewa, in which I explained my intentions and thanked them beforehand for their hospitality. I was not specific about my research focus on women’s motivations for (risky) sexual relationships, as I anticipated that this delicate issue would scare off potential informants, and might stir up gossip about those willing to talk to me. Instead I broadly explained that I wanted to understand daily life in a Malawian village – which was indeed my intention, as noted earlier, so as to contextualize women’s sexual choices. I warned the villagers that I would be asking many questions, because life here would be very different from what I was used to, and I stressed that they were at all times free not to answer. No one ever objected to answering though. But their answers, as I found later, were often tactical rather than truthful.

I had intended to develop a quantitative database, with a number of demographic details on all the adult women in Mudzi village, which I would then supplement with information gathered throughout the research period. Hence, Gertrude and I started out with a basic set of questions for each woman on issues like her age, marital history, size of household, size of land, amount of maize stored (see Appendix 2 for full list of questions). At several occasions throughout the year we visited all households again to ask questions related to that specific period. For example at planting time we passed through the village asking all women what they were planting, how they had accessed the seeds, et cetera. I then used the opportunity to also ask them what they had been eating the previous day, since this was the hunger season. If they answered they had eaten porridge or nsima, we would ask about the source of the maize, and if bought, the source of the money to buy it with.

The first interesting point revealed by the baseline interviews was that the total number of households in Mudzi A and B was substantially smaller than officially registered. Based on the official household list that I received through the District Commissioner I had expected to find 74 households in Mudzi A, and an approximately equal number of adult women. However, when passing by all the houses I encountered no more than 37. As discussed in Chapter 3, I solved this reduction in number of respondents by adding Mudzi B to my sample, of approximately the same size. Both villages had until recently been parts of the
same village, are adjacent, and to this day share one water pump and one graveyard. The inhabitants of Mudzi B were relieved that they were included in whatever project or handouts might come from my presence, while those of Mudzi A seemed disappointed that they now had to share whatever was coming.

Another early mistake I made was not to bother the Group Village Headwoman with my list of questions – which I did out of respect. On hindsight the correct sign of respect would have been to interview her before anyone else. My negligence led to anxiety among the other villagers [e.g. P3 1434], who feared the anger of the Group Village Head. Several therefore came to beg me to interview her too, or ‘register’ as they called it. I then duly did so, elaborately apologizing for my delay in approaching her, and she seemed pleased. According to the answers that she gave, she should be classified as one of the poorest members of the village, with barely any land or food stores. In the days after this interview the Group Village Head sent relatives from other villages to ‘register’ with me too. I was left little choice but to ask them the same set of questions that I had posed to all Mudzi women, as always preceded by an explanation about the purpose of the interview. In this introduction I explicitly emphasized that there would be no future benefit in it for them, that I merely hoped for their willingness to assist me in helping me understand village life.

One likely explanation for the initial eagerness to be interviewed was that everybody soon heard about the small gifts I had brought to our first interviews. As a token of my appreciation of their time and patience I gave each interviewee a 500-gram bag of salt and a bar of soap after the baseline interview. Gifting to respondents is frowned upon by some social scientists, who feel that this ‘buying of answers’ could compromise the authenticity and reliability of data by encouraging informants to tell the researcher what they think he or she wants to hear (Weinreb et al. 1998: 10–11). I found it ethically immoral, however, to extract personal information from respondents for a cause that is unlikely to ever benefit them directly. I surely hope that my data and analyses will somehow contribute to improve the relevance and effectiveness of development interventions. Realistically, however, the impacts of this one individual study will be minimal at best, and most likely not be directed specifically at my informants. Rather, the one to benefit most from this study is me, as it offered me much desired insights on poverty dynamics at the grassroots level, and will further my academic career (and, as a matter of fact, that of Gertrude). To balance the scale somewhat in favour of my informants, and reciprocate for the insights I received from them, I decided to give gifts – which seems to have been the right choice as it was deeply appreciated [e.g. P2 0259–60; P3 2848–9], lowered suspicion about my intentions, and eased my discomfort about bothering villagers with my questions.10

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10 See Bleek (1979) for a reflection on the dubiousness and difficulties of conducting fieldwork, and particularly so-called participant observation, when great social and economic inequality exists between the anthropologist and his or her research population.
The salt and soap gifts were probably one incentive for the Group Village Head and her relatives from outside of Mudzi village to insist on being interviewed. The other likely reason is revealed by their use of the word ‘registration’. To participate in projects or receive handouts, such as the subsidized fertilizer coupons distributed by the government, beneficiaries are usually first registered. To be sure not to miss out on a possible project or handouts that might follow from my enquiries, everybody wanted to be ‘registered’ by me.11

I entered the quantitative data collected through the interviews in an Excel spreadsheet for easy reference and comparison. The first time that I noticed we had been given incorrect information was when our neighbour told Gertrude that she was off to feed her pigs. During the interview, just a few days earlier, she had however claimed not to own any livestock [P3 0539]. I then corrected her answer in my Excel sheet. But when similar cases kept occurring I realized that I should not erase the incorrect information we were given at first, because it could actually be of interest to note on which topics the women were hiding or twisting the truth, and how. As Bleek (1987b) has argued in an article on lying informants: when people lie, it means that something important is at stake.

Bit by bit I came to understand that asking questions is generally considered impertinent, experienced as intrusive, and unlikely to yield useful results [e.g. P2 1262, 1602, 1887; P3 0254, 1224, 1227, 1509, 2497, 2525, 2544]. At the rare occasions that I would overhear someone asking a fellow villager a direct, personal question, the response was often a reprimand or evading answer [see also P3 1837]. Throughout each interview I could see Gertrude get more and more uncomfortable, and increasingly apologetic for the many questions we were asking [e.g. P3 0966, 2109, 1509]. This aversion against inquisitiveness – further analysed in Chapter 7 – combined with the assumption that there was something to gain from overstating their destitution, led to a largely untrustworthy database [P2 1227, 2078]. I suspect that women’s tactical answering was not only individually intentional, but often also the result of social pressure [e.g. P3 1936]. The community as a whole, and the Group Village Head in particular, would benefit from an impoverished and urgently-in-need image, to attract aid agents, development projects, and handouts.

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11 For a Group Village Head in particular it is beneficial to have large numbers of people registered as village inhabitants. External aid providers generally assume that chiefs know best whom among their subjects are in most dire need of support, often leading them to have handouts distributed by the chiefs. Hence, the more registrants per chief, the more handouts this chief receives. As in the tributary system of the pre-colonial days, chiefs are locally entitled to redistribute the items among themselves and their followers as they see fit. Notably, a distribution considered strategic by a chief does not necessarily match the intended distribution of the aid provider.
The untruthful elements in the answers could in some cases be revealed through observation [e.g. P3 0539, 0975]. An example of this was the claim of one woman that she and her family were only eating mangoes at that time because they lacked maize. Gertrude and I saw her young daughters walk to the water pump each day, however, with piles of dirty pots and plates on their heads to wash [P3 1012]. At least eight women during the interviews claimed not to grow cotton for sale, yet were spotted planting, spraying, weeding, or harvesting cotton on their field, and selling their produce. Separately interviewing two women from the same household also brought out dissimilarities in their answers [P3 0511, 0552, 0975, 4107]. Especially regarding the assets that the household owned (e.g. number of blankets and livestock), the size of the current maize stores, and external sources of support, women apparently felt reluctant to tell the truth during the formal interview. Many other falsities were exposed by the women themselves when talking to Gertrude. They seemed to consider her more akin to them than to me, and so at times as one of them rather than an extension of me as researcher. On one of our first days in Mudzi a group of women joined Gertrude and me on our veranda, and after some time started chatting amongst themselves. One teasingly asked a pregnant woman why it had now happened to her twice that a man impregnated and then abandoned her before she even gave birth. The one who asked this was then reprimanded by an older woman, saying these issues should not be discussed in front of outsiders. It did not take long however, before Gertrude came to be considered more of an insider. Especially at the water pump – the place for gossiping – Gertrude was told or overheard many eye-opening stories that contradicted or supplemented the answers given during the formal interviews. Women seemed no longer uncomfortable discussing their ‘private’ village matters with Gertrude present (and, later, not in my presence either). Gertrude, in turn, apparently did not feel uncomfortable to pass on to me what she was told or overheard. When I once enquired about this, she mentioned that she felt my wish to understand village life and particularly women’s decision-making processes was sincere, and appreciated my commitment to this end. My ethical doubts about taking as data the confidentialities that she passed on to me are considered further on.

Increasingly, women visited our house to greet us and chat – with Gertrude rather than me because of the language barrier (and probably the larger socio-cultural distance in general). I was often present though, and at times deliberately joined in the conversation to make clear that I was informed about what they told Gertrude. At times, the women would explicitly instruct Gertrude to translate to me what they had just said, to make sure I understood, especially when they thought it was very funny. Exactly to what extent they knew that and cared about whether Gertrude communicated everything they said to me cannot be ascertained. What has become clear though, is that in an informal setting women did not seem to make a great effort to keep up the lies they told us during the formal interviews.
Our growing realization that the formal interviews did not result in reliable\textsuperscript{12} data was strengthened by the comments of a small group of women from another village, who one day walked past our house. They discussed among each other that if I had picked their village and interviewed them, they would surely lie about everything to enhance their chances of obtaining support [P\textsuperscript{3} 0483, see also P\textsuperscript{2} 1749, 2007–19; P\textsuperscript{3} 2216, 2529]. I had to acknowledge that the interviews, while straightforward in their planning, execution, and analysis, were in this case not an effective way to gather information,\textsuperscript{13} at least not on topics concerning wealth and poverty status.

The answers to my (more qualitative) questions on women’s marital history did, however, seem relevant and useful. I suspect that many women did not give a complete list of past relationships, but their accounts of how their relationships had started and ended were insightful – even if some of them may have been beside the truth. The courting, marriage, and divorce stories of all Mudzi women gathered through the interviews gave me a basic understanding of relationships from which to interpret the gossip, events, and choices that we heard of throughout the research period.

The demographic database did have some advantages, however. It helped me to identify women in a specific situation whenever I looked for these for further enquiry. For example, when I wondered about the reasons some women might have to settle in their husband’s home village of instead of their own – as traditionally prescribed and still usually practiced, I could easily track down the women who had answered during the interview that they were born outside Mudzi. It has provided me at least with a fairly complete set of reasonably reliable data on basic demographic givens from each Mudzi woman, such as (approximate) age, years of education, ethnic descent, religion, and house description. While the interviewing was thus not fully worthless, it did feel disruptive, unnatural, and uncomfortable. Instead of becoming more familiar, I sensed that the interviews enlarged the distance between me and the villagers, who looked apprehensive each time I appeared at their compounds with a notebook.

Johannes Fabian (1990) has argued against the assumption that informants (or anyone, for that matter) have readily discursive ‘information’ in their heads that

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\item[12] In the quantitative social sciences, the reliability of scientific observations includes the extent to which they can be replicated (Scrimshaw 1990: 89). In anthropology, this aspect is considered unfeasible, if only because most of the variables under study cannot be controlled. Rather, reliability of anthropological data can only refer to the extent to which respondents’ answers are truthful (Van der Geest, pers. comm.). Reliability, then, can only be ascertained and ‘measured’ through thorough knowledge of the particular respondent and his or her context (Bleek 1987b).
\item[13] The same was found by Bleek (1987b) during his study of (among other things) birth control and abortions in rural Ghana, Rahman (1999: 34) during his study of women and microcredit in rural Bangladesh, and Wamoyi et al. (2010: 5) during their study of transactional sex in rural Tanzania.
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can be uttered whenever asked for it. Collecting data merely through questioning people about issues outside of their context is thus not only problematic because it may be culturally inappropriate and so cause unease,\textsuperscript{14} and allows for twisting and hiding the truth. It is also problematic because people often have not thought out beforehand how they think or feel about certain issues, and why exactly they do so (see also Pool 1989: 29–31). Producing on the spot a clear statement that precisely, or even roughly, covers and conveys what one vaguely feels (or would feel if one were to take all the time needed to deliberate over a certain issue) is an art that few human beings master. It can therefore not be expected that informants’ accounts are always (or even ever) reliable representations of a truth – even when the informant had no intention of purposefully twisting his or her truth. Instead of an informative approach to data collection, in which it is assumed that reliable data can be straightforwardly provided upon request, Fabian proposes a performative approach. In his idea of performative ethnography, a researcher should create opportunities to interact with informants on the basis of ‘coevalness’ (i.e. on equal terms\textsuperscript{15}), and let knowledge be shaped during such processes of interaction. ‘Performance’ here does not only refer to empirically observable movements, in the sense of a practice carried out in a particular place. A naturally flowing conversation can also be considered a performative act, in which a certain discourse is co-produced through an exchange of thoughts, reflections, and revisions. I return to the issue of data production through performative conversations further on.

The problem of interviewing obviously not only exists as ‘wrong’ answers, but often also as ‘wrong’ questions. In order to get valid answers – hence information about the specific issue that the researcher intends to assess – careful selection, formulation, and framing of questions is crucial. And for this, sufficient knowledge about the particular research setting is required (see also Bleek 1987). For example, when I asked farmers which crop they harvest first after the hunger season they named an early-maturing groundnut variety, which I then took to be the crop that marked the end of \textit{njala}, the hunger period. However, when I actually witnessed the end of the hunger season I noticed that instead of groundnuts, fresh maize cobs were the first to be picked from the fields and, either boiled or roasted, consumed with great fervour. Except for those individual cobs, however, the maize was

\textsuperscript{14} After we finished our interview with them, some women sighed with relief that answering our questions had not been as difficult as they had feared it might be.

\textsuperscript{15} To be exact, with the term ‘coevalness’ Fabian (1983) literally means ‘a sharing of the present time’ between ethnographer and the other participants of his or her project to co-produce knowledge. With the term he calls attention to and criticizes anthropologists’ tendency to perceive their research subjects as still living in pre-modern times.
but not harvested, as it must remain in the field until the cobs have fully dried so that they can be stored and ground into maize flour. The first crop to be actually harvested were indeed those groundnuts, but by then people already been able to fill their stomachs with fresh maize cobs. The farmers had not intentionally tried to mislead me. I had simply made a wrong assumption and incorrectly formulated my question when trying to find out when and how the hunger season comes to an end. Even though I had lived in Malawi for three years before I moved to Mudzi, had extensively read the literature on sub-Saharan African poverty dynamics, and had made several short trips to villages targeted by ICRISAT projects, much of what I encountered when living in Mudzi I could not have imagined before. Consequently, I could not have formulated questions on these issues, and would never have found out about them if I had relied only on self-designed questionnaires.

Reluctant to continue the formal interviews as the main method of data collection, I intended to rely more on informal interactions that I felt were both more appropriate and more fruitful. These, however, are more difficult to bring about and direct. Instead of purposive planning so as to get instant answers on specific questions, I found myself at the mercy of the ‘flow’ of things. The village gossip and personal affairs that women confided to Gertrude were incredibly insightful, keeping us updated about the happenings in and around Mudzi, the topics considered interesting to discuss, and the various opinions about these happenings. Where possible, I tried to create opportunities for informal visits, such as buying tomatoes from a village woman, helping on another’s field, or asking to see or learn a certain craft – all reasons that were perceived as far less threatening than being approached with a written list of straightforward personal questions. Eventually I found that an ‘excuse’ for visiting was not even required, as a casual visit just to say hello was highly appreciated and often led to interesting, naturally flowing conversations. Although we thus let things around us take their own course as much as possible, Gertrude and I always tried to keep in mind and be particularly sensitive to the issues relevant for this study’s topic of interest. What we heard and observed around us – besides being data in their own right – were leads to further inquiry, which we followed up on whenever the opportunity occurred or could be created. Hence, our increasing resort to ‘free-wheeling’ should not be interpreted as “simply being there and passively watching what people are about” (Pelto 1970: 92). Even though we tried not to dictate the course of actions around us, our own actions were certainly guided by the ultimate goals of this study – namely unravelling the interrelations between livelihood (in)security and sexual decision making.
One important opportunity for informal interaction presented itself largely unplanned. After I had selected Gertrude to be my research assistant, she told me about her handicraft skills. Because I had long wanted to learn how to knit I was pleased to hear this, and decided to bring some knitting needles with me to Mudzi so that she could teach me. I deliberately brought some extra pairs, in case one of the village women would want to join us. In the late afternoons, when we had finished interviewing or visiting and I had written down my notes, Gertrude and I sat outside in the shade and knitted. As we appeared ‘not busy’, women felt free to come and sit with us and chat. It took some time before the first one dared to take up a pair of needles and try, but eventually more and more women and girls joined me in learning how to knit and crochet. The fact that I often had to ask Gertrude for help and made many mistakes may have helped them get over their initial shyness. During these knitting sessions I was as much of a ‘coeval’ to the visiting Mudzi women as I probably could have become. For the time being we were not hierarchically distanced because of our roles as researcher and researched, but joined in mutual enjoyment of the same leisure activity and pursuit of a shared goal. I had never expected that these informal classes would eventually lead to a group of 10 to 15 women and girls voluntarily gathering at our house practically each afternoon to knit, crochet and, most importantly, chat. Only when their fields required the women’s attention at planting and harvest time, were Gertrude and I left to knit alone. Notably, the exact composition of the group was different almost every day. In total about 35 women and girls spent at least some afternoons with us in this way. The group of most regular visitors consisted of about 15 women. Most of the women who joined us were unmarried at the time, or had a husband who was away for a period of time (working in elsewhere in Malawi or in South Africa). Women who had a partner staying with them at the time tended not to join us or only briefly, soon running home again to see if he needed attending [e.g. P2 1503–4; P3 1609].
As can be expected, these daily gatherings were a great opportunity for the ‘performative’ conversing that I much prefer to ‘informative’ interviewing. As Fabian (1990: 18) puts it, “the ethnographer does not call the tune but plays along.” During the formal interviews I asked respondents to reflect on issues outside of their context, ‘out of the blue’ as far as the respondents were concerned, making it difficult for both them and me to get to the heart of a matter. During the knitting sessions, women brought up issues (opinions, jokes, complaints) that emerged more naturally from normal interaction – hence within a logical context. While the interviews evolved around topics that I had chosen, the conversations during the knitting sessions were directed by the interests of the women and girls themselves. If the group was big, there was usually

\[\text{16 Obviously, after a certain topic is introduced by an interviewer, a more-or-less naturally ‘performative’ conversation, in which there is ample opportunity to jointly ‘create’ insights, can still develop about that topic}\]
a lot of joking and laughing, and gossiping about people not present. When there were only a few women, the conversations became more personal and intimate. I feel that the triangulation of 1) straightforward, factual data collection (although to some extent unreliable), with 2) informal, on-the-spot, and within-context-produced reflections of women on themselves and others, and 3) our own observations of village interactions gave me a good insight into the daily lives of the women of rural Mudzi.

Obviously, women’s informal narratives were also full of twisted truths, as revealed by the fact that various parties (or even the same party on different occasions) regularly told us or each other different versions of past events. Rather than trying to uncover what really happened, tempting as it may be and often was, I tried to focus on finding out what each party might have had to gain by telling their particular version. As remarked earlier, when people choose to lie, something important is apparently at stake.

Activities like fetching water, collecting firewood, and cultivating the land also helped me to better understand the women, by personally experiencing the physical hardship, repetitiveness, and social aspects of such tasks. These activities also helped to strengthen my bonding with them, as they generally appreciated that I was not afraid to get my hands dirty and my muscles tired [e.g. P2 0065–6; P3 1799]. When I managed to accomplish a certain task they seemed sincerely delighted. I did not undertake all household tasks each day though, to ensure sufficient time for writing down my elaborate field notes, but also not to be an interruptive factor at the normal water pump conversations. Letting Gertrude fetch water on her own usually led to more interesting new insights than when we went together.

Some of these pictures were taken by Gertrude, and others by villagers interested in operating my camera.
Besides the interviews, informal conversations, and participatory activities, merely living in the village and being around to observe life brought many unexpected insights too. I would never have imagined that women would join literacy classes when they were already able to read and write, hiding this skill by using their left hand. Or that many villagers, having just made it through months of struggling for (expensive) food, once their meagre maize harvest was in, would begin selling it for ridiculously low prices. Or husbands disappearing without a word when the hunger season set in, leaving their wives to cultivate the fields and take care of their children alone. And to then shamelessly return when the harvest was about to be brought in, with a small gift to appease their abandoned wives. Several of these women had during the knitting sessions foreseen this, and vowed not to take back the deserter if he dared to pull this trick. But when it came to be, each woman did. All the events and developments that I had not expected to occur worked to puzzle me, triggering me to delve deeper, and so helped me to eventually better understand village life dynamics.

‘Participant observation’ is the common term used to denote the most characteristic research method of anthropology, referring more or less to what I just described: taking part in the daily life routines of the population under study. Wolf Bleek\(^\text{18}\) (1987) has noted that this term is often misleading, as anthropologists are seldom able to actually take part in and observe the specific activities that they study. In my case living up to the term would have entailed engaging in sexual relationships, but more importantly – to come anywhere near to experiencing what Mudzi women go through – it would have entailed somehow getting rid of all my safety nets. The first requirement was beyond what I was prepared to undertake for this study, if it had been achievable at all. The second was utterly impossible. Rather, as Bleek (1987b: 315) remarks, “anthropological knowledge is predominantly based on what people say they do, not on what researchers see them doing.” Indeed, although through our (long) presence in Mudzi Gertrude and I witnessed many relevant contextualizing aspects to sexual

18 As noted earlier this is a pseudonym of Sjaak van der Geest. See Van der Geest 2011: 146–7 for his reasons for using a pseudonym.
relationships, the major contribution of our participation in daily life was the access it offered to women’s spontaneous (rather than artificially induced) narratives.

I did not, however, manage to live in Mudzi full time during the one-year research period. In the beginning I still had certain responsibilities towards ICRISAT, e.g. meetings to attend and presentations to give. Furthermore, after three years in Malawi I had come to dearly miss home, family, and friends. Although at times it was possible to phone from and to Mudzi, often the network did not allow this, and even text messages often did not go or come through. The utter disconnectedness from my ‘former’ life, lack of contact with my home base, combined with heavy malaria prophylaxis and malnourishment, caused many sleepless nights, nightmares when I did sleep, and eventually mental distress. Although feeling guilty about it, I knew I could entrust essential parts of the data collection to Gertrude, so I left the village four times. During those times Gertrude took over the daily journal writing and as such kept track of what occurred in Mudzi while I was gone. Once I returned to the village I read her notes and discussed them with her in depth, which then generally led to important new leads to follow up (whenever an opportunity for this occurred or could be created).

Additional data: moving beyond the village
During the interviews several Mudzi women mentioned that they engaged in selling food (raw or processed) in times of financial need to find money. Such women, having their own means of generating income, might have different expectations from men and sexual relationships than women without an independent income, I hypothesized. To gain a greater understanding of the causes and consequences of women’s independent money making, we began looking for and talking to small-scale businesswomen selling their merchandise at one of the three markets in the Mudzi area. Only one of these (at what is called a ‘trading centre’ in Malawi, with permanent market structures and a small clinic, approximately five kilometres from Mudzi) is in operation all year round, while the other two (at approximately one and two kilometres away) are virtually non-existent during the lean season. In total we interviewed 30 market women (see Appendix 3 for the set of questions) on their business, marital, and child-bearing history to see if any pattern could be discerned regarding the relationship between income-generating activities and sexual choices. These data are discussed in Chapter 8.

Dutch readers may know Thea Beckman’s children’s novel Kruistocht in spijkerbroek, which has also been made into an English film under the title Crusade in jeans. During my time in Mudzi I at times felt like the main character of this story, who through an accident with a time machine is stranded in the 13th century. Nobody around him has a clue about the lifeworld he grew up in, moreover, that world no longer exists. Even though I could obviously go back to my own lifeworld, it often felt so out-of-touch that I experienced it as merely a vague memory that could not be shared with anyone around me.

When I stayed in one of the pilot villages during the hunger season, I had brought with me cookies and cereal bars, which I tried to eat at night without letting others hear it. I felt so awful doing this that I thereafter decided to go hungry along with the villagers.
Women with an official job and hence steady income would also have provided useful information to shed light on a potential link between livelihood security and sexual decision making. However, such women were hard to come by in and around Mudzi. I only managed to find and interview one young woman employed in a mini-shop in the largest market in Mudzi’s vicinity, and a nurse employed at the small hospital at this marketplace.

This nurse was also a relevant source of information on the sexual and health behaviour of people in the area. Both she and the chief nurse of the district hospital in Balaka gave me access to their monthly birth statistics of the past years, which allowed me to assess whether a significant difference exists between specific months in the number of births (and hence the occurrence of unprotected sex nine months earlier). The interesting results of this exercise are discussed in Chapter 5. Gertrude and I also had ourselves tested for HIV in the clinic offering Voluntary Counselling and Testing (VCT) services nearest Mudzi, to experience the setting and procedures, and we visited this clinic twice more to collect statistics on the number of positive and negative tests carried out among men and women.

Other interviews outside the villages were held with staff of the various development organizations active in Mudzi, both at their so-called field offices in Balaka town and at their national headquarters in Lilongwe. Although the insights gained through these meetings were interesting, they were not of sufficient relevance to the purpose of this book and are therefore not used directly.

In an attempt to gain insight into women’s management of their meagre resources I asked four Mudzi women to keep track of their daily incomes and expenditures for three and a half months. These financial diaries have been particularly relevant for illuminating how resources flow through the community and how women tactically invest in social relations. The findings from the diaries are more elaborately described and analysed in Chapter 7.
Language

The interviews were held in either ChiChewa, the national language of Malawi spoken by all but the oldest generation in Mudzi, or ChiYao with the elderly women. I had deliberately selected a research assistant who is fluent in both tongues. I had studied ChiChewa myself too, but in ChiYao I could only greet. During my earlier research in Guatemala I had not undertaken any interviews; all my data was gathered through informal conversations, which I believe greatly enhanced the reliability as well as local relevance of the data. This had however only been possible because I had sufficiently mastered Spanish, the mother tongue of all my informants. I aspired to accomplish the same with ChiChewa, and have studied it extensively. However, after the six-week pilot study, I realized that I had spent most of my time and energy merely trying to communicate. Spanish had been relatively easy to learn, being another European language with familiar grammar rules and recognizable words. ChiChewa was of a completely different order, without any correlation to the languages I had known so far. Each word completely alien, each sentence a complicated puzzle with an unfamiliar logic. I had to acknowledge that I had not been able to master this new language sufficiently to get to the level of mutual in-depth understanding necessary for ethnographic research. That is where Gertrude came in.

During the interviews we held in Mudzi I formulated some questions in ChiChewa, while others I posed in English and Gertrude would then translate. After some time she rarely had to translate the responses back to English, as I well understood what had been said. Spontaneous conversations were more difficult though, when I did not know the topic and therefore regularly missed the point. Some interviews were recorded and then transcribed by Gertrude and me together, to allow for in-depth exploration and discussion of what had been said and how. However, the various types of recording equipment I had brought not only often failed on me, but also increased the discomfort of the interviewee, leading me to increasingly leave the equipment at home [P3 0623, 2212, 3562]. Once transcribed, I usually had little difficulty disentangling the grammatical puzzles and understanding the meaning of the statements. So while I have unfortunately missed many literal quotes – lost in translation especially in the case of informal chats, there were many others that I could follow literally and interpret personally. The transcriptions of the interviews showed Gertrude’s translations to be usually very literal, converting the exact words used rather than summarizing whole expressions. This enhanced the accuracy of the translation and joint interpretation process. To diminish distortion I furthermore instructed Gertrude to write down in ChiChewa all the quotes that she overheard or were uttered to her in her field notes as literally as she could remember.
Analysis of data

Gertrude’s and my hand-written field notes were converted into almost 650 typed pages. I have coded these notes with the data analysis software ATLAS.ti (version 6). The encoding has helped me to organize the mass of qualitative data – the many detailed stories about what occurred and was discussed during every day of a full year. Whenever I began the analysis of a new subtopic within this study, I first retrieved all of our notes about that particular topic through ATLAS.ti, read through them, and further organized them into subsections so as to eventually ‘find’ a system in them and allow for generalizations.

Obviously my analyses do not only rely on what Gertrude and I have written down. When rereading the diaries or ATLAS.ti compilations, in my head the anecdotes became contextualized within memories of the particular situation (or discussions about that situation) as well as of relevant situations that occurred prior to and after it. My own notes thus functioned predominantly as reminders of situations and forgotten details of that situation rather than as data in itself. Gertrude’s notes, however, were not only reminders but also useful for discourse analysis, as her specific selection of what occurrences were worth noting down, and her reflection upon and description of these added an extra layer of ‘emic’ information. I further elaborate upon this aspect in the next section.

Researchers

In most sciences the personal background of a researcher should not matter. Trying to uncover universal laws, the effects of anything else but the selected variables under study are eliminated as much as possible. The value of findings is measured by the extent to which they are objectively verifiable and replicable – meaning that if the same test were carried out by another researcher at another moment, similar outcomes should be found. While the research tools and procedures need to be made explicit so as to allow such replication, the particularities of the researcher should be irrelevant and are therefore usually considered unnecessary to reflect upon.

In this sense anthropology is quite exceptional. Part of ‘solid’ anthropological ethnography is self-reflection, as the researcher is his or her own tool to generate data (and in this case, so was Gertrude). The researcher-as-tool is not neutral, nor omnipresent, nor invisible, which makes it relevant to reflect on how his or her presence impacts the data collected. With most anthropologists I share the belief that we can only gain partial understanding of people’s beliefs and practices. Firstly, we can merely observe what we have access to and choose to look at, or stumble on accidently – which is influenced by who we are and how people in the field respond to us. Furthermore, because insights into the daily lives of the research population are usually gained through taking part in daily life, this active presence of the researcher inevitably influences the field, the informants,
and hence the data collected, as I too experienced in the field and described above. Secondly, as widely acknowledged by anthropologists, making sense of what we observe always involves interpretation. In sum, both what we can observe and how we interpret it is conditioned by our own personal background and the particular circumstances of the fieldwork.

Despite these weaknesses as a scientific research method, I believe participant observation has several crucial strengths that make it worth using. As explained earlier, many of the insights I gained in this study would have been difficult to acquire through other research methods. To overcome the method’s weaknesses, an anthropologist must be conscious of his or her position within the field and within the process of data collection and interpretation. While always aiming to minimize bias and distortion, it is still necessary to point out or at least share some of the personal background information of the ethnographer – to let the reader know through which eyes he or she is given a view and analysis.

As my research assistant Gertrude played a significant role in the construction of our data set, it is relevant to not only describe my own but also her personal background. Despite the widely acknowledged need for reflexivity in ethnography, anthropologists often obscure the role and impact of their research assistants (Pool 1994: 20–21). Dependence on an interpreter tends to be considered and experienced as a weakness, for it presumably distances an anthropologist from his or her informants, and so obstructs access to authentic, ‘unfiltered’ data. A few anthropologists have argued, however, that it is exactly through intense collaboration between the (outsider) researcher and the (insider) assistant that their two ‘worlds’ can be brought closer and insights can be jointly constructed (Bleek 1978, Mommersteeg 1999, Pool 1994). Acknowledging the advantages, or at least the inevitability, of an assistant’s impact on the process of data construction calls for a reflection upon the particularities of this assistant. As experienced and described by Berreman (1962), different assistants may lead to completely different research outcomes. In the following I sketch Gertrude’s personal background and elaborate upon her role in the research. Where relevant throughout this book I note her contribution to the data or analyses concerned. This is not only a matter of respect for her work, but, in line with the above, my own attempt to grasp and represent to the readers how interpretations took shape and conclusions developed.

Principal researcher: Janneke Verheijen
I am a 34-year-old woman, born and raised in a well-to-do, urban environment in the Netherlands. When, at the age of 10, I read about the Masai in one of my

21 As noted, I take this to mean taking part in the daily life routines of the population under study rather than suggesting that anthropologists can actually participate in and observe all activities under study.

22 Gertrude has read all that I have written about her in this study, and approved of it.
grandmother’s weekly magazines, I was hooked, and dreamt for years of living in an African village myself. I had not heard of anthropology yet, and regretted that the dream seemed unachievable. As time went by I came to realize that there are many groups of people around the world living lives profoundly different from mine. My interests broadened, and studying Cultural Anthropology was the logical result. During these studies I spent 18 months in a Guatemalan jungle village – partly working in an eco-tourist resort and partly conducting my Master’s research, which focused on the impacts of the recently introduced televisions in the village, especially on gender relations. I found that the soap operas that most women loved to watch diminished their acceptance of gender inequalities as natural and inevitable (see Verheijen 2005 and 2006).

Looking back, the theme that played the central role throughout my studies was the resilience and agency of marginalized, oppressed groups that are usually depicted as powerless victims. I was intrigued and inspired by two books in particular: *Veiled sentiments: Honor and poetry in a Bedouin society* (1986) by Lila Abu-Lughod, and *Weapons of the weak: Everyday forms of peasant resistance* (1985) by James Scott. Both books acknowledge and eloquently analyze the agency of those living under severely restrictive circumstances to create opportunities for self-determination. What these books exemplified for me was that cultural power dynamics are not to be taken at face value. There may be winners and losers, but the losers are not necessarily passive victims. Often they actively deviate from existing norms – albeit within culturally accepted limits. While this adds to their feeling of self-worth, at the same time it affirms and solidifies these cultural boundaries, as these are not challenged but (grudgingly) accepted. Both books prove that studying such power dynamics – defiance within societal boundaries – can reveal a lot about the wider cultural setting. Not surprisingly, my fascination with this theme resonates throughout this book.

After my studies I obtained a position as social scientist at an agricultural research institute called the International Crops Research Institute for the Semi-Arid Tropics (ICRISAT) through the Associate Professional Officer program of the Dutch Ministry of Foreign Affairs. While ICRISAT’s headquarters are based in India, my station was located in Malawi: I was finally off to Africa. My assignment was to conduct research on the impacts of AIDS on the food security of small-scale, resource-poor farmers. Together with my long-time boyfriend, I lived a more-or-less typical expatriate’s life: my sizeable salary (and the relatively low prices of living) allowed us to buy a car, and employ a housekeeper, gardener, and night-guard. Even when hunger ravaged the rural areas of Malawi, we found the supermarkets in the capital well stocked with imports from South Africa, and the (few) restaurants served us as usual. I spent most of my days in the office, making only occasional day-trips to the field. On such trips our delegation was always welcomed by groups of singing and
dancing women – the villagers had been expecting us and were ready to show us what they thought we wanted.\textsuperscript{23}

During this period I enrolled in the IS-Academy: another initiative of the Dutch Ministry of Foreign Affairs, in alliance with various knowledge institutes in the Netherlands. Launched in 2005, the IS-Academy (IS refers to the Dutch acronym for International Cooperation) resulted from the observation that social scientists are insufficiently involved in developing policy on international cooperation, and civil servants do not habitually draw on available academic knowledge. The aim of the IS-Academy is therefore to improve communication and collaboration between policy makers and social scientists for the benefit of sustainable poverty reduction and development. As one way to accomplish this, the IS-Academy funds research positions for both young civil servants and social scientists on policy-relevant topics. With a research proposal on the potential contribution of agricultural interventions for reducing female susceptibility to HIV I was selected for one of the two PhD positions in the HIV/AIDS trajectory. For reasons mentioned earlier, I could only commence the actual fieldwork for this study after my three-year contract with ICRISAT ended in 2008.

\textit{Research assistant: Gertrude Finyiza}

As my preparations for the Mudzi fieldwork coincided with the final phase of my contract at ICRISAT, I had to follow the institute’s formal employment protocol to find a research assistant. This meant placing an advertisement in one of Malawi’s national newspapers, in which I called for a female Yao speaker who was willing to live in a village with me for one year. I received over 200 application letters in response. The male and non-Yao speaking applicants were easy to dismiss. From the remaining letters I first selected those from respondents with the highest education levels and most work experience, and discussed these with my (Kenyan) supervisor. He gave me the wise advice to instead select someone with only a secondary degree and, at best, a bit of relevant work experience. He expected – probably rightfully so – that those better qualified would never be willing to stick it out with me under the circumstances in which I planned to be living and working, while someone less qualified would probably be less used to urban luxuries and grateful for the job opportunity. From the five young women whom I then selected for interviews, Gertrude simply struck a chord with me. Her Yao (besides ChiChewa) was fluent, her English sufficient, her attitude pleasant.

\textbullet\ Personal background

Gertrude was born in 1985, in her father’s home village in the Southern Malawian district Mangochi. Although her father was of (patrilineal) Ngoni descent, most

\textsuperscript{23} In Chapter 8 I delve a bit deeper into the interaction between external development agents and their intended beneficiaries.
of their fellow villagers were Yao, which allowed Gertrude to pick up the Yao language from her peers. Her mother worked as a midwife, and her father was employed as a jack-of-all-trades at a nearby Roman Catholic missionary post. When he died, the Italian sisters offered support to his family by taking their youngest daughter Gertrude into their orphanage’s boarding school, where she received an exceptionally solid education for rural Malawian standards.

Halfway through her daughter’s secondary schooling at the mission post, Gertrude’s mother decided to move with her children to her own home village in the Central Malawian district Dedza – so that they could inherit land when she eventually died. An employed relative was found who was willing to pay for the continuation of Gertrude’s secondary education near their Dedza village. The man soon died, however, leaving Gertrude with the choice of following her sisters’ (and many other village girls’) example of marrying and farming, or somehow managing to find her own money to finish school. As a child, she had been intrigued by those villagers who every morning dressed up nicely and left for a job, and by her mother when she would dress up to attend a formal meeting. This is what she envisioned for herself too. When a distant aunt living in Malawi’s capital Lilongwe offered to take her in and care for her, Gertrude assumed this entailed continuing school and quickly accepted. Unfortunately, the woman was too pleased with the diligent nanny, cook, and housekeeper that Gertrude turned out to be, and kept postponing her niece’s enrolment in school.

When Gertrude realized that it had never been her aunt’s plan to actually pay for her education (nor for her work), she decided to leave and find her own means. She enrolled herself in a private secondary school because these accept payment in monthly instalments rather than per term as required at public schools – which would lead to amounts too large for her to save up. To gather the monthly instalments she awoke at 3 a.m. every morning and fetched water for houses under construction, walking up and down pumps and construction sites with large water cans on her head. At 8 a.m. she would leave work and head for school. Within two years she graduated. In the year between graduation and becoming my research assistant, she found money in various ways. She rented a telephone and settled herself beside the road to offer calling services, produced seeds for a multinational agribusiness, and taught handicrafts and English at a privately run school. With the money she earned she rented a small house without electricity or water in one of Lilongwe’s ‘high density’24 areas, and sent whatever she could spare to her mother who had become too old to find money herself. Her mission remained finding funds for further education. When I hired her, she felt that the accomplishment of this mission was finally within reach.

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24 The term ‘high density area’ is used in Malawi to describe city neighbourhoods in which the (generally small, run down) structures are built close to each other, in constrast to the ‘low density areas’ in which (generally large, luxurious, fenced) compounds are built at some distance of each other.
• Role in the research

Gertrude quickly became a valued personality in Mudzi. She was respected for her education level, English proficiency, and employment with a white foreigner, and so taken as role model by Mudzi’s youth and women [e.g. P2 0111, 1322, 1519; P3 0374]. She was valued for her neutrality within the ever-changing web of village amities and animosities, her capacity to operate a cell phone, and her handicraft skills, and was frequently asked for assistance in all of these terrains [e.g. P2 0155 1225, 1305, 1381, 1395, 1439, 1498, 1551, 1600, 1643, 1801, 1906; P3 1065, 1801, 1923, 2678]. Despite their deference to her, many people seemed to feel at ease with her, as if she were one of them. She indeed has an amazing ability to strike the right tone with everybody, from rebellious teenagers to the distinctively old.

While most villagers were long uncertain and suspicious of my agenda in Mudzi, that of Gertrude was considered clear and unsuspicious. She seemed to be seen as simply lucky to have found employment, her probing about certain ‘annoying’ topics was understood as something that came with the job that she had taken up (as any of them would have done if they had the opportunity), rather than blamed on her. The normality of her participation in daily life activities such as fetching water, her fluency in the (social, cultural, and spoken) languages, her non-threatening agenda, and her pleasant personality all contributed to the fact that Gertrude became such an important ‘instrument’ for data collection. Furthermore, as mentioned earlier, her reflection upon these data proved to be relevant information in itself. Surely her interpretations cannot be considered representative of those of Mudzi women, as she had another ethnic and religious background than most Mudzi villagers, had a higher education level than most, and had spent some years in the city. Nonetheless, her perceptions were certainly emic25 rather than etic, offering me an impression of the ‘logico-empirical systems’ (Harris 1968: 571) that prevail in Malawi, pointing out where my ‘normalities’ differed radically from hers and possibly that of Mudzi villagers [e.g. P2 1498; P3 0545, 1249, 1774, 1801, 2342, 2458, 2560, 2623]. For example, I may never (or at least not without considerable delay) have become sensitized enough to realize that a woman washing laundry is not a casual everyday occurrence. As I further discuss in Chapter 7, in a resource-poor context like Mudzi such a seeming triviality is actually highly indicative, namely of rare access to money for soap. Being ‘tuned in’ to village life, Gertrude was always quick to casually remark upon this underlying information [e.g. P3 0930,

25 The concepts ‘emic’ and ‘etic’ were coined by linguistic anthropologist Kenneth Pike (1954), who defined them as follows: the “etic viewpoint studies behaviour as from outside of a particular system,” while the “emic viewpoint results from studying behaviour as from inside the system” (1967 [1954]: 37). The terms have become widely applied in the social sciences, although there are no standard definitions (Lett 1996). In general, ‘emic’ tends to refer to local, participant, or insider explanations of phenomena, and ‘etic’ to scientific explanations of the same phenomena and the emic statements about them.
through which I soon learned to see the significance of such events too. When we became more intimate with the village women I noticed that they jumped to the same conclusions in similar instances. In the next section I delve deeper into the content of the data generated by and through Gertrude.

I have already described how Gertrude’s handicraft skills may have saved the study, as these inadvertently led to the frequent informal meetings that offered me indispensable insights into Mudzi women’s minds. Another unplanned way through which Gertrude generated insights that would have been difficult to retrieve otherwise was her venture to buy enough maize to get herself through the year after our fieldwork, now that she had a salary and access to cheap maize. When Mudzi villagers harvested their maize and looked for chances to sell some of it, Gertrude turned out to be a reliable buyer. This was appreciated by those in need of money, and offered us a great opportunity to see when, how, and why maize was sold, even though it was a secret undertaking in many cases [e.g. P2 1500, 1508, 1550, 1562, 1565, 1572, 1582, 1597, 1611, 1668-73; P3 2185].

Gertrude’s advice in the selection of gifts was also valuable, and indicative of how well she was ‘tuned in’ with Mudzi villagers. While I initially expected that during the hunger season villagers would be most happy to receive edible items, she convinced me to instead buy soap and salt – which were indeed highly appreciated. Her better understanding of Mudzi women’s preferences also showed when at the end of our fieldwork we bought zitenje (the cloth that women wear over their skirt, carry their babies in, use as blanket, etc.) for all village women as a final thank-you-and-goodbye gift. To my surprise and amusement, the designs which I had picked were disliked by the Mudzi women for being ‘last year’s fashion’, while those that Gertrude had chosen were favourite.

• Gertrude’s data
For an impression of the data the Gertrude generated in her field notes, I copy a (quite randomly selected) excerpt from it. In these notes she describes what occurred, and particularly what has been said (revealing to some extent also what is left unsaid), during the afternoon of March 19 2009 by the women that came to visit. The texts between square brackets are my additions for clarification.

At 1: 50 Sofia [25], Kondwani [35], Ellesi [16], Ada [18] came to knit. Kondwani started saying that Esnart [23] is not getting better because of the fighting and she is failing even to go and fetch some water. Then Ada said that: am going back home to make porridge for my dotta [daughter]. And Ellesi started laughing and said that: Ada you are now busy taking care of your child, does it mean that you didn’t know that if you sleep with a boy without condoms you can get pregnant? Then Kondwani answered that: Ellesi you need to respect Ada because you are not of the same age.

At 3: 40 Livia [21] came and she called me separately and she started saying that: I have missed my periods for 2 and half months but I don’t know what is wrong. Then I told her that: if you slept with your boyfriend without any protection that means you are pregnant and Livia
said that: ah-ah-ah, I didn’t [admitting later that she did] but at the hospital when I went for injection they told me to go back after 3 months but I didn’t. Then Livia left with a sad face. Then Sofia [25] said that: aunt Getu [as some called Gertrude] ndapita tiyenzi mundiperekeze [I’m off, escort me], then I escorted her while Ellesi, Kondwani, Lovely [30], Grace [22] were still knitting, then Sofia told me that: Livia seem to be pregnant because she told me that she has missed her periods for 2 months and she told me that she don’t know what to do (we just laughed) and Sofia said to me that ticheza mawa [we chat tomorrow] then she left. Then I asked Grace about what people cook when there is a funeral (where they get food). Then Grace answered that since we harvested from our fields we contributed maize and nandolo [cowpeas], that’s what we are still using right now. Then Tumanene [26] said that: am going back home because my 2 dottas are ill (malaria). Then Kondwani started saying that: am going to beg some ndiwo [from] Sofia [relish, in this case beans which Sofia had received a bag of from her ex-husband as form of support, peace-making offer, and overture for reunion. Kondwani received a cup of cooked beans for free.].

Sofia came back and she started laughing by telling Tumanene that: please you need to tell your husband in South Africa to be buying nice clothes for you because he sent some clothes as if they don’t sell nice clothes in SA! He must sent [sic] some money for you to buy what you want! Because everybody at the pump was laughing at you. And Tumanene was angry and didn’t answer anything. [P2 1437–1441]

Gertrude’s narrative style is simple. She straightforwardly sums up what happened next and next and next, mostly using first-person speech when quoting others. Although there is generally little elaboration, reflection, or interpretation by Gertrude on the events that she describes in the diaries themselves, her notes offered me ample leads for further inquiry, which always generated interesting and often relevant discussions with her (which I then described in my own field notes). These discussions regularly brought up new questions and so led to new directions for our research endeavours.

As mentioned, not only were Gertrude’s field notes an important contribution, but also her verbal reflections upon the daily lives that we saw and heard unfolding around us greatly contributed to my understanding of Mudzi. As we spent much time together – living together, sharing all meals, and walking long distances to go places or meet villagers – we had sufficient time to talk about what we noticed and how we interpreted this. An example of this from my own field notes:

Gertrude remarks at breakfast that Chikondi often writes [in the income and expenditure diary we asked her to keep] that she pounds maize for other women [in exchange for money]. “You don’t think it is true?” I ask. No, with two small babies, doesn’t think that women would ask her, thinking that she doesn’t have the power for it. Indeed, even at her own house two girls were doing the pounding rather than herself. “So you think she hides something [the real income source],” I conclude, and probe [knowing the common logic by now] “but who would propose to a woman with two young babies?” She answers: “Ah, they [men] can/will, especially knowing the behaviour of her husband [who does not help Chikondi, only shows up at night for food and shelter] they will think she needs support.” [P3 2458]
Justification for a focus on women

In this research I have focussed on women – of all ages, but particularly those in the mid-age category: those in the prime of their lives, with a household, field, and children to care for. Men have only been included at times, when the opportunity arose. Not often did I purposefully approach a man for interaction [e.g. P3 0879, 2132–8, 2549–52] – with the frequently visited male village head of Mudzi A as the exception because of his formal position as community spokesman. This focus on women has both practical and topic-related underpinnings.

As has become clear in this chapter, to a large extent I have let the data collection process more or less run its own, itinerant course. As a result, we interacted most intensively with the village women rather than the men. Women form the core of the (largely matrilineal) Mudzi community; they are its stable members. Their husbands and boyfriends are rather temporarily involved individuals, not much connected to the other men and women in Mudzi. Although this by itself was no reason to exclude them from the study, the fact that they were less present in everyday public social interactions, which are dominated by women, made it difficult for us to casually encounter and interact with them without much disrupting the ‘natural flow’ of things. Interaction between non-related men and women is uncommon in Mudzi and generally frowned upon as it could indicate a (upcoming) sexual relationship. As will become clear throughout this book, there is a lot of rivalry between Mudzi women over men. As a result, any ‘suspicious’ interaction between a man and woman is closely watched and scrutinized. Gertrude received relationship proposals at various occasions throughout our fieldwork period [P2 0893, 906, 1020, 1048], which she kept scrupulously secret from other women, out of fear of triggering jealousies and distrust. Building on my experience in Guatemala, I had purposefully asked my husband to visit me in the field so that my marital status would be clear to all. I so hoped to avert male proposals on the one hand and female fears of competition on the other. However, as I was without my husband during most of my time in Mudzi, some expressed surprise about the fact that I did not take on a lover for the time being. “He sure did,” I was guaranteed, considering how long I had already left him unattended.

Our active interaction with Mudzi men may or may not have aggravated the fears and suspicions that my presence stirred in general, and may or may not have caused (covert) tensions and hostilities. In any case, beyond the various practical considerations, men’s opinions were only of secondary relevance as my research question revolved around women’s decision-making processes. Surely, we observed men’s actions where possible, and heard lots of talks about men, so that men are not completely absent from this study. Readers particularly interested in the Malawian male perspective on sexual relationships and AIDS I refer to Kaler (2003, 2004a) and Kalipeni & Ghosh (2007).
By concentrating on the women who lived in Mudzi during our fieldwork, men were not the only ones left ‘out of focus’. Even more so were those who decided to leave Mudzi. This is important to realize, because for all I know these may have been women and girls with particularly different attitudes towards securing a livelihood and sexual relationships from those who stayed behind and were studied, which may have biased the results. Likewise, I have not inquired much about prime-age women who died prior to the study, who may have had deviating attitudes towards sexual relationships. I regret not having gathered more information on numerous issues including these, but there is only so much that a researcher can do during a limited period of time and given the focus of the study.

Ethical considerations

Contrary to some other social scientists, working in other settings or at the service of or with funding from other agencies, I was not obliged to have my research proposal approved by an ethical council. ICRISAT did require that I follow a course and take an exam in ‘research ethics on human participation in research’. I did so at the Office of Research Integrity of the US Department of Health and Human Services.

Neither of these two facts has much to do with my moral concerns during our fieldwork. These concerns first and foremost sprouted from common human sense (‘common’ at least in the cultural environment where I grew up). The first of the two moral dilemmas that most bothered me was the extent to which I could and wanted to disturb Mudzi women – who were obviously too polite to turn me away yet seemed uneasy with my visits. The other moral dilemma concerned the acceptability of including women’s and men’s informal comments that were not expressed to us for obvious research purposes.

There are no universally ratified ethical rules that social scientists must abide, as there are for some other professions such as medical doctors. There are however various, largely overlapping, lists of ethical guidelines for anthropological research, of which I take the 2011 Association of Social Anthropologists’ (ASA) Ethical Guidelines for Good Research Practice26 as reference point here, because this code offers advice for both my concerns.

The problem of including Mudzi women’s and men’s informal comments in our data set is the fact that it cannot be ascertained whether this would be approved of by those who expressed the comments. What is lacking, thus, is their informed consent as far as this specific data is concerned – a directive mentioned in most ethical research codes. All villagers have however been invited to the introductory

26 Available at: http://www.theasa.org/downloads/ASA%20ethics%20guidelines%202011.pdf
meeting in which I elaborately explained my wish to stay in their community to gain an in-depth understanding of their daily lives, and in which I asked for their approval for this endeavour. As also noted earlier, I have made clear, both at this point and at later occasions, that everyone was at any time free to refuse participation without further consequences. All village women, thus including those who regularly visited us at home, have in addition been told again about the research and asked personally whether or not they were willing to help me in this study. All expressed their willingness. Moreover, all women (and men) who visited our house did so completely voluntarily. They were aware of the general research purpose of our stay in their midst, and are likely to have been aware that most of what they said in my or Gertrude’s presence reached me. To this end I often deliberately participated in conversations to emphasize that I could and did follow what was being said, and brought up issues that women had told Gertrude.

Although I frequently emphasized our general research purposes, I never mentioned to Mudzi villagers my particular interest in sexual decision making and HIV risk. This goes against the ethical code that potential informants must know precisely and in as much detail as possible what they are consenting to. I have felt no ethical hesitation, however, in remaining vague about my ultimate goal of unravelling the interaction between livelihood insecurity and sexual practices. Firstly, I did not lie when stating that I wanted to gain a general understanding of women’s daily lives. As mentioned, this was indeed what I hoped to achieve. Secondly, as also noted earlier, I believe that more detail about my exact topic of interest may have harmed the individuals that I would have approached with questions or interacted with in any other way. The main principle of most ethical research codes states that the quest for knowledge may not harm the wellbeing of research participants. The fact that I hid my precise interest focus was the result of weighing pros and cons: I preferred to protect my (future) informants by emphasizing my general interest in women’s lives and decision-making processes rather than their involvement in sexual relationships.

Other measures were also taken to minimize potential harm and protect the wellbeing of research participants. We never passed on information that we received privately to third parties within the village community. We tried our best to avoid discord and jealousies among villagers, for example by not privileging anyone over others. By changing the name of the research locality as well as all names in this thesis, I am reasonably confident that no harm will be done to any of the persons involved by using the comments that they expressed to the research team (that is, Gertrude or me) or in our presence.

The ASA ethical guidelines furthermore suggest that anthropologists should avoid “undue intrusion” by their enquiry and methods (principle I.3), elaborating that:

In many of the social scientific enquiries that have caused controversy, problems have not arisen because participants have suffered directly or indirectly any actual harm. Rather,
concerns have resulted from participants’ feelings of having suffered an intrusion into private and personal domains (ASA 2011: 4).

As explained in this chapter I have often felt that visiting villagers’ homes with a list of set questions and pen and paper to note down their answers, or even voice recorder to register their every word, made many nervous and uncomfortable. I believe that this research approach was too intrusive for the Mudzi case, and was effectively circumvented by increasingly relying on informal conversations. In other words, my discomfort about including villagers’ conversations with or in the presence of Gertrude was eased because it allowed me to lessen the burden that I would have placed on them otherwise. In the ethnographic chapters that follow, the fruits of our struggles to gather relevant, personal, and often intimate data are presented and assessed in detail.