

12 Co-Production, Friendship, and Transparency in Anthropological Fieldnotes

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Introduction

Despite the widely acknowledged need for reflexivity in ethnography, anthropologists often obscure the impact of their research assistants. Dependence on an interpreter tends to be considered a weakness, for it presumably distances an anthropologist from their informants and obstructs access to authentic, ‘unfiltered’ data. A growing number of anthropologists argue that it is precisely through intense collaboration between (outsider) researchers and (insider) assistants that their two ‘worlds’ can be brought closer and insights can be jointly constructed (e.g. Pool, 1994; Mommersteeg, 1999; Turner, 2010; Gold, Gujar, Gujar, & Gujar, 2014; Kaiser-Grolimund, Ammann, & Staudacher, 2016). Writing, discussing, and rewriting fieldnotes play a central role in that collaboration. As Berreman (1962) found, different assistants may lead to significantly different research outcomes. Acknowledging the advantages (or at least inevitability) of an assistant’s impact on research processes calls for methodological reflection.

To explore these ideas, we—a retired professor (Sjaak) and early career scholar (Janneke)—each reflect on how our fieldnotes, as emblems of our fieldwork relationships with research assistants, shaped our respective ethnographic endeavours. We present and unravel three stages in the life of fieldnotes: how they are produced in a sphere of mutuality between researchers and assistants, how they affect relationships in the field, and how they can be used to make ethnographic work more transparent. We first introduce our respective studies and then delve into the themes of co-production, friendship, and transparency. In the discussion, we bring our individual stories together to interrogate how acknowledging the contributions of research assistants through fieldnotes has evolved over the years and the extent to which we ought to reconsider or even revise our anthropological textbooks (cf. Sanjek, 1993, p. 13). In their ‘handbook’ on fieldnotes, Emerson, Fretz, and Shaw (1995) observed that “ethnographers are often uneasy or embarrassed about fieldnotes” (p. ix), considering them backstage scribbles not to be shown in public. In this chapter, we show instead the instructiveness of these raw, and often collaboratively built foundations of ethnography.

Fieldwork and Fieldnotes

Sjaak

In 1971 I started my fieldwork in Kwahu-Tafo, a rural town (about 4000 inhabitants) in Southern Ghana, as part of my Master's in Sociology and African Studies at the University of Ghana. My research focused on conflicts in one extended family at the closure of a period in which anthropological functionalists had been heralding the harmony of African family life. At that time, I considered myself a poor person with a budget the equivalent of about 300 euros for six months of fieldwork. When a Ghanaian student, who had become a good friend on campus, offered to join me during the first months of the fieldwork, I felt uneasy because I feared that I could not maintain a 'household' of two persons, let alone pay him a decent salary. But Kwasi Asante-Darko (his real name) reassured me that a salary was not necessary and that we could manage with the little we had.

When we arrived in the town, we found a small room—2 × 4 metres—in a compound house. Four rooms were occupied by non-family renters (including us) and the other six rooms accommodated about 15 family members. We borrowed four cement blocks to construct a bed with a few planks of wood and a grass mattress. We also borrowed the rest of our 'furniture,' which included an old table, two chairs, two wooden crates that served as small tables or chairs, and a bookshelf.

I emphasize the simplicity of our accommodation and our shortage of money because these factors turned out to be a crucial beginning of the solidarity that grew between us. The room, table, and hurricane lamp (there was no electricity in the town) were the setting where we discussed the fieldwork and worked side by side to write our fieldnotes and transcriptions at the end of the day. If time allowed, Kwasi also transcribed and translated Highlife lyrics that expressed the various problems of life we were studying in a more poetic and emotional form (Asante-Darko & Van der Geest, 1982, 1983).

The fieldnotes were mainly two diaries that we kept together. One diary recorded our daily experiences in the house where we were living and in some of the surrounding houses belonging to the same family. The other diary recorded events that took place in the town. Kwasi wrote down what he heard or saw, and I wrote my observations. Both diaries also covered observations that were not directly related to the research topic. I also kept a third diary of short notes about activities of the day. Neither Kwasi nor I kept a personal diary of our frustrations, anger, and other emotions. Today, I can only guess why we failed to write about our emotions. Perhaps our days were already fully absorbed by other, apparently more urgent activities.¹

About two months after we arrived, a bitter conflict arose within the family. This conflict offered us the three leading themes of the research: conflicts around marriage and kinship, around death and funerals, and

around *bayie* (witchcraft).² We could not record the conflict with our tape recorder. For two days, Kwasi followed the heated discussions and went into our room at more quiet moments to write about what he had heard and observed. His notes about this conflict cover almost 80 pages in my thesis (Bleek, 1975, pp. 48–127).

Janneke

During my first anthropological fieldwork in a village in northern Guatemala, I participated in women's daily life activities and wrote extensive notes about what I heard and saw and thought (Verheijen, 2005, 2006). Having previously worked in the community, I had built a rapport with many of the villagers, spoke Spanish with the local accent and had little trouble engaging in informal conversations. It was precisely this 'deep hanging out' (Geertz, 1973) or naturalistic mode of interaction (Beuv-ing & De Vries, 2014) that added to the reliability and relevance of my data. When I was later preparing for my PhD fieldwork in a village in southern Malawi, I intended to use a similar approach: learn the language, hang out, chat, participate, observe, and write, write, write.

Through a three-year office job in Malawi's capital, I had time to study ChiChewa. However, I learned mostly from books, as the Malawians that I interacted with spoke English fluently. During a six-week pilot study in a rural area, I realized that my book-knowledge of formal ChiChewa was not sufficient to interact at a level of in-depth mutual understanding that is vital for ethnographic research.³ I had to accept that this study was going to be very different from my earlier research in Guatemala. Reluctantly, because it felt like a failure, I decided to hire a translator to join me for a year in a village community, where I wanted to study the interlinkages between women's livelihood insecurity and HIV risk-taking to assess the widespread assumption that economic empowerment can help poor African women avert HIV infection.

Gertrude Finyiza (her real name) was one of the 200+ applicants who responded to my job advertisement. My Kenyan supervisor advised me to select a translator with secondary education and a bit of relevant work experience. He expected that better qualified candidates would not likely be willing to stick it out with me under the circumstances in which I planned to live and work. He suggested that someone less qualified might be comfortable without urban luxuries and more grateful for the job opportunity. Of the five young women who I interviewed, Gertrude simply struck a chord with me.

Gertrude turned out to be much more than the translator that I was looking for. She also assisted me as a cultural broker, data collector, and friend. More than I—a white foreigner unaccustomed to most of what our informants considered normal—ever managed to achieve, she became an entrusted 'fellow villager' in Mudzi.⁴ While most villagers were suspicious

of my agenda in Mudzi, that of Gertrude was considered clear and unsuspecting. Many villagers regarded her as lucky to have found employment, and her occasional probing about ‘annoying’ topics as part of her job. Within the ever-changing web of intra-village amities and animosities, Gertrude managed to remain a neutral bystander, accepted by all. Her capacity to operate a mobile phone and extensive handicraft skills were appreciated and often called upon. Gertrude’s access to insider stories, and her reflections about what occurred around us have been of invaluable help to my understanding of Mudzi life. Without her collaboration, I could never have achieved the level of fine-grained thoroughness that my dissertation was later lauded for.

Co-Production

Sjaak

I ended my report on our fieldwork with the remark that Kwasi’s description of the family conflict covered almost 80 pages in my thesis. My thesis? The use of the possessive pronoun ‘my,’ as Janneke also does in her last sentence shown earlier, captivates the injustice done to research assistants throughout long traditions of anthropological fieldwork. The academic system does not allow assistants as co-authors of dissertations although they collected a substantial part—if not most—of the data. Their names can only be found in prefaces and acknowledgements. Why do assistants not appear prominently on book covers and on published articles?

Did I do any better? In the field, Kwasi and I seemed equal colleagues working together to co-produce the research, conducting interviews and conversations with the people of the family and the town. And we sat together at the table as I described earlier, writing our notes and diaries and discussing the day’s events, with the hurricane lamp between us. For example: One evening after most people had retired to their rooms, a quarrel erupted between the two wives of our landlord and household head. Through a big split in our door, we could observe what was happening and held our microphone in the split to record the argument. That evening, Kwasi wrote in our diary:

The following happened: A [second wife] abused Y [first wife]: *Woto a akyea, wo tuntum fi, eyɛ a guare awia na ngware anadwo, mene akrakyefo na ekasa, enye wo a dabiaa wokɔ afuom no*. You have got a crooked buttocks, you dirty Black,⁵ you must bathe in the afternoons, not in the evenings. I walk with literate men, not like you who goes to farm every day. Mame Y [replied]: *Wo nanti a apaepae, enye wo aniwu se wode wo ho sɛɛ kɔda KA mpa so?* You have a splitted heel. Is it not a disgrace to you to sleep with KA [husband] in this manner? [referring to her clothing].

The next day Kwasi made some inquiries and added to our fieldnotes:

The quarrel [had] started with a question asked by A to Mame Y on the farm. The answer was given in the house—that was the quarrel.

A [had] asked Mame Y whether she was jealous when she (A) slept with KA. Y did not answer. She pretended she did not hear the question. When Y came home after evening meals, she asked A why she [had] asked that question on the farm. A abused Y in English. Mame Y demanded an explanation in Twi as she does not understand the English language. A said: “*Wo to a akyea*”—a crooked buttocks, etc.

(refer to page before + Y’s response)

For a few days, we entered our observations in the diary. I could never have understood what the two women shouted to one another and what the old man was mumbling, trying to stop the fight. The vocabulary and the speed were far beyond my competence of Twi. Later, this event became a favourite case in my teaching for several reasons. It showed underlying tensions in this polygamous household (which often looked to me a peaceful and ‘sisterly’ marriage arrangement). The case also showed how participant observation does not end at 5 p.m. Finally, this example became a hot issue in discussions about fieldwork ethics (is it appropriate to record such a private affair without the consent of the persons involved?).

Kwasi was the main author of these notes about the quarrel and countless other observations, but I was the final author when the notes were transformed into a public text. Although we co-published a few papers (Asante-Darko & Van der Geest, 1982, 1983; Bleek & Asante-Darko, 1986), Kwasi never asked for authorship. Perhaps this indicated that he did not want co-ownership of the texts we produced. I dedicated ‘my’ thesis to him (without revealing his full name), although he never remarked about it. His involvement in my research seemed based on camaraderie and the adventurousness of our undertaking. He did not appear to have academic ambitions. After his bachelor’s degree, he developed a career as district chief executive in several towns. Although he never suggested that I include him as a co-author, I assume that he appreciated how I frequently mentioned his support and our friendship. Here, I ‘assume’ because we never talked about it, as far as I can remember. I wish I could ask him about it after all those years, but Kwasi died last year.⁶

Janneke

When Gertrude and I were in Mudzi together, I wrote most fieldnote entries. But these entries relied on both of our memories and memos

that we each jotted down on paper or in our mobile phones while visiting villagers, saleswomen, and nurses. When one of us talked, the other often wrote key words around issues that we considered remarkable and important. When walking home, we avidly discussed what we had heard, seen, noticed, and were intrigued or bewildered about. Many of my diary entries were thus mediated through my post-facto conversations with Gertrude. As time passed and my ChiChewa improved, I relied less on Gertrude's translations. However, her cultural brokerage provided me with additional layers of information throughout the fieldwork. While writing fieldnotes, I turned to Gertrude to help me remember details, clarify formulations, and discuss how events fit within the wider constellation of social life in Malawi.

Women also came by our house to greet us and chat, although more often with Gertrude than myself. At times, I deliberately joined in the conversation to make clear that I understood what they were saying to Gertrude. On several occasions, women noted that I did not laugh when they said something they considered funny, and instructed Gertrude to make sure that I understood. Over time, the shade behind our house became a late afternoon hang-out place for village women and girls, who joined in as Gertrude taught me how to knit and crochet. The bulk of our fieldnotes contain descriptions of the conversations, gossiping, and joking from these daily gatherings.

Gertrude assisted me in remembering what we experienced, and catalysed the production of information through her approachability with many of the women. Moreover, what Gertrude found noteworthy and how she interpreted occurrences was informative in itself. Surely her interpretations cannot be considered representative of those of Mudzi women; she had a different ethnic and religious background and higher education level than most Mudzi villagers and had spent some years in the city. Yet Gertrude's perceptions were certainly *emic*⁷ rather than *etic*, offering me an impression of local reasonings and pointing out where my 'normalities' differed radically from hers. For example, I would not have realized that a woman washing laundry, a man smoking, or a radio blasting music were not casual everyday occurrences. In a resource-poor context like Mudzi, such trivialities are highly indicative of rare access to money (in these cases for soap, cigarettes, and batteries). Being 'tuned in' to village life, Gertrude was quick to identify such indicators, through which I learned to see the significance of such events too. When we became closer with the women, I noticed that they also made similar interpretations.

Living together, sharing our meals, walking together, and spending many evenings sitting on our veranda looking into the dark night, Gertrude and I had ample time to talk through how we interpreted life in Mudzi. Over time, Gertrude became acquainted with what I wanted to know, and I grew increasingly familiar with her way of seeing, observing,

and interpreting. An example from my fieldnotes illustrates how entangled our modes of thinking had become:

Gertrude remarks at breakfast that Chikondi often writes [in the income and expenditure ledger we asked her to keep] that she pounds maize for other women [in exchange for money].

Trying to find out why Gertrude comments on this [with such seemingly casual comments, she often tried to point something out to me], I ask: “Hmmm, and you don’t think it is true?”

No, with two small babies, it makes no sense that women would ask her to do this job, Gertrude argues, as they will presume that she doesn’t have the power for it. Hmm, indeed, even at her own house we saw two girls doing the pounding rather than herself.

“So you think she hides something [namely, the real source of income] . . .” I conclude, and probe “but which man would propose [to be in a sexual relationship and as part of that gift some money, food items, or soap] to a woman with two young babies??” [knowing the common logic by now and expecting that this is what Gertrude is in fact insinuating, hence checking whether this is what she is referring to].

She answers: “Ah, they [men] can/will [propose to be in a sexual relationship to women with young babies], especially knowing the behaviour of her husband [who does not help Chikondi, only shows up at night for food and shelter], as they will think she needs support.”

(Janneke, May 30, 2009)

Whether or not Chikondi received gifts from men other than her husband is irrelevant. What I considered telling in this conversation are Gertrude’s (implicit) suggestions that Chikondi’s (presumed) hiding of a source of income signals a secret involvement with men, and that women who are known to be not well cared for are likely to receive proposals from men. Both assumptions surfaced regularly in Mudzi women’s commentaries too. These conversations with Gertrude helped me to better recognize, contextualize, and interpret similar comments from other women.

On several occasions during the first year when I had to leave Mudzi, I left Gertrude to describe what occurred and what women came to talk about. She did a great job. Upon my return, we elaborately discussed her notes, and followed up on the questions that emerged. This worked so well that during my subsequent field research in Malawi, Gertrude spent most time in Mudzi alone. By then I had two young children at home and could not travel to Mudzi as frequently. Yet my role could also be played from a distance, facilitated by my lived familiarity with Mudzi and its inhabitants. While I only spent several weeks in Mudzi intermittently, Gertrude stayed for several months. Every two weeks, she cycled to the

nearby town and had her handwritten notes typed out and emailed to me. We then discussed the fieldnotes and next steps elaborately over the phone, after which I annotated her notes while she followed the leads that we identified.

During her weeks alone, Gertrude mostly followed the flow of village life: she attended funerals, accompanied pregnant women to antenatal services, taught some women how to sew clothes and others how to bake bread buns, helped youngsters with their homework, assisted in the fields, and paid social visits throughout the village. She knew my research interests and how much I cared about understanding these issues within the broad context of women's diverse daily life concerns. Critically, she knew what kind of questions could be asked and when, and what could be joked about. An example from her fieldnotes:

PATI [WHO HAD COME TO KNIT WITH GERTRUDE] SAID: "Gertrude, how many kids do you want to have when you get married?"

I LAUGHED AND SAID: "1 or 2 will be enough for me."

THEN PATI SAID: "Me too I just want 2 kids, but my husband says he wants 4 kids."

I ASKED: Why?

PATI: "He said he wants 2 girls and 2 boys, and he also said that I am no longer a girl now but I am a big woman. But I know that he is jealous with me because I am beautiful and he knows that a lot of men propose me, but I will not listen to him that I should have 4 kids. I will wait till 4 or 5 years, that's when I will give birth to another child."

I: "Is he going to accept it?"

PATI: "Yes he will, if not I will end the marriage and go back to school."

I: "How are you going to prevent from getting pregnant?"

PATI: "I will continue using [contraceptive] injection."

I: "What will happen if your husband says he wants another child?"

PATI: "Then I will not reveal to him that I am still using injection, because when other people don't want a child while their husband want a child they use injection and keep the book [health passport] to the friend that you trust."

THEN I SAID: "Does it happen here?"

AND THEN PATI SAID: "Yes! Others are doing it."

(Gertrude, November 3, 2014—edited by
Janneke for readability)

The informality of the conversations and interactions, and many women's voluntary, forthcoming comments make Gertrude's notes so powerful. Much of the available knowledge on daily decision-making in Malawi is derived through surveys and questions that have been formulated by outsiders related to topics that interest researchers. Time and again, we saw teams of unknown interviewers enter Mudzi to ask endless lists of

questions, many of which Mudzi villagers considered odd if not impertinent. After participating in survey interviews, women who joined us in knitting expressed how uncomfortable the questioning had been, and how they had lied as a means to deal with it (see also Biruk, 2018). Although my presence in Mudzi caused anxiety too, as white people are ‘known’ to steal blood and attract thieves, we tried our best not to harass anyone with uncomfortable questions (let alone take body fluid samples). We prioritized building rapport over ‘forcing our way’ to information. Over 16 months of collaborative fieldwork, our co-produced fieldnotes total 1003 typed pages (464 written by Gertrude, 539 by me).

Friendship

Sjaak

The co-production of fieldnotes gave me a feeling of equality between Kwasi and I but I may have been misled by my romantic illusions. About five years after the fieldwork, I published a (Dutch) reflection on the study and invited him to add his views on our relationship in the field. His remarks about our financial position made it clear that our collective work on fieldnotes and diaries had not yet made our relationship an equal one. He wrote:

We were poor boys. I was annoyed with Wolf [my pseudonym] but did not show it. He never showed me how he got his money. At that time, I did not have any money to help him, but I was concerned about who his financier(s) was (were). However, I was not the person who carried out the research, so I did not ask about it either. Wolf was not at all easy with money, but at the same time he was very nice. On one occasion I pointed to a car with a sign on which C. D. (Corps Diplomatique) stood. He thought I made an allusion to money [the Cedi—abbreviated as CD—is the Ghanaian currency] and blushed.

(Bleek, 1978, p. 96)

On the one hand, Kwasi’s remark might indicate the distance between us, but the fact that he wrote this also showed the ‘ease’ between us. His next comment is also telling:

We ate three meals a day, strictly according to time and quantity. Nevertheless, we were prepared to share the little that we had with a visitor who came by, as long as that was not an unwanted visitor. I did not understand Wolf then but later I realized how useful this was and now I do the same.

(Bleek, 1978, p. 96)

Doing fieldwork and writing fieldnotes together changed both of us. When I asked an older man in Ghana what friendship is, he replied: “A friend is someone you can share your secrets with.” If that is a good definition, Kwasi and I had become friends. We lived together, ate the same food, shared the same adventures, and wrote together in the diaries that we kept. We depended on one another. We told each other intimate things from our lives. When he noticed that I was keeping something hidden, he was offended. In one such incident, a young man had told me about the girlfriends he had had in recent years and I had reassured him that I would never disclose his communication to anyone. After I transcribed his story, Kwasi (who had not been present during the conversation) took the text to read it. I realized that the young man might not appreciate it if Kwasi came to know the contents of the conversation, as he and Kwasi often moved in the same circles. I told Kwasi and he put down the notebook. Later, I realized that he had been hurt. Kwasi made it clear to me then that there should be no secrets between us and that anyone who told him something should know that I would hear it, and vice-versa.

The value of our relationship for my study was that I learned to see life in the community through Kwasi’s eyes. It brought me closer to the experiences of the family. He explained what was behind the stories we recorded. Those conversations were sometimes so intense that we could not sleep and decided to just get some more work done.⁸

Janneke

Unlike Sjaak’s relationship with Kwasi, my relationship with Gertrude was formally one of employer and employee. I paid Gertrude to provide a service, decided our research agenda, and set the terms of our collaboration. Nonetheless, sharing an experience that was out-of-the-ordinary for both of us, sharing a house, a life, and a mission for a full year produced a lasting bond. Our collaboration continued after the fieldwork ended: I helped her with her studies during her Bachelor’s degree as well as with writing CVs, job applications, and reference letters. Gertrude is always prepared to discuss any queries that I have about our fieldnotes or Malawi in general. We also maintain regular contact about our social lives through social media platforms like WhatsApp and Facebook.

As with all social relations, what constitutes friendship is culturally fluid.⁹ Although our affectionate attachment is strong, Gertrude does not call me a friend. Over the years, she has come to call me ‘Mum,’ and my daughters, her sisters (although the age difference between her and my daughters is substantially larger than between her and I).

Whatever we call our relationship, its deep levels of trust and familiarity allow us to discuss, in a relatively open way, topics that are not easily talked about intra-culturally, let alone cross-culturally. Our bond developed through fieldwork and was shaped by the process of finding

a format for productive intercultural research collaboration. To this day, Gertrude continues to be not only someone I dearly care about, but also an informant, sounding board, and cultural broker. She provides me with first- and second-hand accounts of events and, through our discussions, helps me interpret them. Gertrude also continues to benefit from our collaboration. It has triggered a constant sequencing of research assistant work for her, and eventually supervisory positions within internationally financed studies. She has built an impressive CV and network, and a beautiful house in Malawi's capital city.

Transparency

Sjaak

My publications about family conflicts and sexual relationships in Kwahu-Tafo were hardly transparent. I had assured those who revealed intimate information that I would keep that information strictly confidential. That promise, as I later realized, implied that I had to conceal not only their identity (by using fictitious names) but also my own name and the name of Kwahu-Tafo. Any clue that could identify the fieldwork location and family had to be removed. I also had to break another promise, that I would bring my two books to the family and wider community, in order to safeguard the first (complete anonymity). Sharing the books would have enabled them to de-code the pseudonyms and trace the identities of those persons who had revealed to me incriminating or embarrassing information (Van der Geest, 2003).¹⁰ This strategy worked. For 20 years, the family did not hear from me or see the publications about them. In other words, I purposely reduced transparency. I could have lied and exaggerated with impunity.¹¹ In the 1970s, prior to the internet, checking on the reliability of data from far away was difficult. After 20 years, however, I revisited Kwahu-Tafo and handed the PhD book to the (new) family head and some other participants. Ironically, they were disappointed that the book did not have any pictures, or the name of the town or their own names. After this time, I had hoped that the participants would be interested in talking more about the study, but no one approached me to discuss the contents or to point out mistakes.

Janneke

When I wrote my dissertation, I moved between our fieldnotes and my analytic abstractions of them. I kept minute track of the paragraph numbers of which fieldnote excerpts underpinned my claims. This allowed me to reference our fieldnotes numerically to make visible the transformation of 'raw' ethnographic data into academic analysis. I fancied this possibility to include all the 'gems,' such as Gertrude's rich emic descriptions

and reflections. I also liked the prospect of readers suggesting alternative interpretations or using our fieldnotes for their own research purposes. Moreover, I wanted to afford justice to women's agency and daily endeavours, which are so often missing in development discourse on the archetypal poor and suppressed African woman, forced to engage in sexual relationships with men in order to survive. Many of Gertrude's fieldnotes shed a different light:

THE NEW HUSBAND OF DORIS LOOKED HAPPY, THEN I SAID: "I think you are enjoying the marriage?" He laughed and said: "Yes I am enjoying it because my first wife when I came back from the business, she was giving me a pot to cook, while she knew that I am tired. She was giving my clothes to wash myself, and most of the time I was not going home until 9 or 10 pm because I didn't like quarrelling with her and I was just sleeping without eating and go to business without my wife cooking."

(Gertrude, December 1, 2014)

CORA CONTINUED BY SAYING: "Have you seen, Gertrude, my husband didn't work in the field today. I asked him to go with me at the field but he refused and he asked me to cook some nsima [staple food, thick maize porridge] for him then I refused. I told him that with the relish that I have—I want to [will only] prepare a meal for my children."

(Gertrude, October 20, 2014)

DORIS SAID: "Gertrude! Yesterday the boyfriend of Malita came and I just showed him where Malita stays because I knew that her husband is not there. Then this morning Malita told me that they chatted a lot and he gave her 500 MK for soap and I also told Malita that I have another boyfriend and maybe I will get married soon. Then Malita says she don't want marriage anymore but just chibwenzi [casual relationships] but I told her that if I will see that this proposer doesn't provide what I want, then I will chase him [away]."

(Gertrude, November 17, 2014)

I felt that making this array of fieldnotes excerpts more accessible could pay tribute to the women who so kindly accepted us into their lives yet are muted and misrepresented in virtually every development report that I had read.

However, creating direct links to our fieldnotes had a range of consequences that required consideration. As we never wrote our fieldnotes for public dissemination, they had to be anonymized, which was difficult because we used personal names inconsistently throughout our notes. We also had to check whether pseudonyms sufficiently protected our

participants' anonymity. Sometimes, I felt that a pseudonym was insufficient to conceal the identity of someone who shared potentially harmful information. In these cases, I changed our fieldnotes to assure protection against possible recognition. Given that I have not disclosed the real name of Mudzi, readers cannot verify the accuracy of our notes (although informants can).

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spot whether she is available for and interested in starting a relationship. The many self-reports of random proposals and proposals from unknown men are backed by first-hand experiences of Gertrude and myself [P2 0176, 0288, 0906, 1020, 1048; P8 0083]. Even when proposals came from men unknown to them, many women accepted without hesitation [P2 1131, 1208, 1710; P3 2064, 2105, 3766, 3819, 3849; 3900, 3927, 3959, 3976, 3978, 4000, 4012]. Considering the relative shortage of men (see Chapter 3), they were relieved to have found someone who was prepared to commit himself, and feared that their proposer might take his offer elsewhere if they dawdled too long [P3 0418, 0714, 0746, 1444]. Some justified that there was no point in trying to find out more about the man, because both he and others could be lying anyhow [P3 0495, 0530, 3965]. Several women acquiescently said they had trusted that God sent them the husband they deserved [P3 0742, 3917, 4000, 4018]. The only way to find out if someone will make a good partner, many women felt, is to just start living together [P3 0530, 2785, 2795, 3900, 3962, 3978, 4012]. For some women, the mere fact that they were proposed *banja* rather than *chibwenzi* was enough reason to accept:

Salika was 19 when a man came to the house where she stayed with her parents. When he asked her to start a family with him, she accepted immediately because "when a man proposes *banja*, it means he is serious about taking care of you". [P3 3978, also P3 3455-6, 3889]

1710 Back	<p>Later on Jane came for the second time with a certain boy. I asked her who is the boy? And she answered that he is my boyfriend we met yesterday at Mudzi B playground, and he proposed me then I accepted and she answered that he is a radio repair and it will not be difficult for him to provide money for soap. (Jane laughed) he stays at M. village and he comes in this village to play football with his friends. Then they continued their journey to the grocery. Then Innocence came and I started asking her if people at the maize mill leave madea, and she said that some of them they take when going home while others, they decide to leave at the maize mill. Then Innocence continued that did you heard aunt G that yesterday Emra was quarrelling with Fissa because of goats and I said no! the goats of Fissa were destroying cotton in the field of Emra (4 goats) then Esnart told Fissa that please! Take your goats out of my field if not that means you will pay 500mk per goat (that's what the chief said) and Fissa said that I can't do that, if you want some money just sell one of your goats and Esnart started crying and went direct to chief Mudzi B to explain what was happening. Then GVH said that don't cry we will talk tomorrow (then she left) in the afternoon I just saw Chikondi going to wash her clothes at the borehole. No-one else came to knit, I hope they went at the funeral.</p>
3849 Back	<p>Elube (45, 2nd marriage) got married very young because of "mavuto": her father had died and when a man came to ask for her her mother allowed. They survived through ganyu. However, 2 days after giving birth to first child, he left to Lilongwe and never even sent soap, so she decided to end the marriage (he has come to see his child, but never brought anything). Moved back with mother and stayed alone 2 yrs. Second husband moved around looking for wife, met each other in street where he proposed about 30 yrs ago. Because of her problems she decided to agree. Asked her whether married, she answered marriage ended, then asked if she had kids, one, he agreed to take care. He was the first to propose after first one left. She had been waiting for someone to propose marriage to her. Was begging soap from her mother, wanted to marry to find soap on her own. But is not good husband, she has to do ganyu to buy clothes for kids, mat etc. He just plays cards, and when he wins uses money for himself (e.g. matemba - dried fish for own snack + maybe 10 MK salt for HH). Has zibwenzi, she heard from others and even himself. Told him to go back to his home vlg, afraid of diseases, but he refuses, saying he gave up trying to have several wives. But she can't be sure, because often he comes home late... She continues to feed him because of chisoni - his mother died so he has no other place to eat.</p>

Figure 12.1 P. 123 of Janneke's dissertation, followed by two examples of linked fieldnote fragments

When I travelled back to Mudzi to show the book that I had written about our research, I asked permission to make our fieldnotes public. All Mudzi inhabitants were excited to see, and in some cases keep, the book, although none could read its content. No one objected to me sharing the fieldnotes and so I decided to go ahead and create hyperlinks to our fieldnotes. The digital PDF¹² version of my dissertation (Verheijen, 2013)¹³ includes 300 pages of analysis (the dissertation) and over 600 pages of fieldnotes. In the dissertation text, I created hyperlinks between my analytical claims and the collections of fieldnote excerpts that contributed to my claims, thus allowing readers to easily click back and forth between the two documents. Clicking on a paragraph number links to fragments from our fieldnotes, which appear on screen (see Figure 12.1 for an example).

Discussion and Conclusion

Our experiences working with field assistants and writing fieldnotes are similar, but there are also some significant differences. To address these contrasts and similarities, we relate some of our practical, methodological, and ethical considerations with experiences of other anthropological fieldworkers.

Co-production

Both of our research assistants also wrote fieldnotes. We are not sure if our practices are commonplace as the role of local assistants in anthropological research often remains unacknowledged. Judith Berman (1994) investigated George Hunt's contribution to Franz Boas's studies of the Kwakiutl people in British Columbia, Canada in the 1900s.¹⁴ Hunt, the son of a local Tlingit woman and British father and fluent in the Kwak'wala language, took notes about his observations on customs, stories, rituals, and local explanations. Berman discovered that Hunt's contribution to Boas's publications on the Kwakiutl was enormous. Hunt wrote extensive texts in response to questions that Boas posed to him, either by mail or face-to-face (p. 488; see also Sanjek, 1990, pp. 195–203). To some extent, Boas did acknowledge Hunt's contributions. In one volume (Boas, 1921), Hunt appears in the title and is named as co-author on three books (as far we could confirm). Boas usually introduced Hunt very briefly as his assistant and translator. In the preface of Boas's (1930) book on Kwakiutl religion, he mentioned that he met Hunt in Chicago where he contracted him as assistant and taught him to write the Kwakiutl language (p. ix). He emphasized the imperfections in Hunt's orthography and that he corrected Hunt's transcriptions. At the time, Boas did not reflect on his own background and position in the field (let alone his assistant's). Berman provided such a reflection in her

detailed description of Hunt's life and career as Boas's assistant and de facto ethnographer. She pointed out that Hunt (not Boas) was the participant observer; Boas preferred one-on-one interviews. In the absence of modern technology to record conversations, Hunt's transcriptions were often his own compositions (Berman, 1994, p. 491).¹⁵

Without criticizing Boas unduly, research assistants seem to us the most undervalued and exploited participants in the anthropological field. They rarely receive the recognition that they deserve. Ethnographers tend to consider the notes and reflections of assistants as their own property. 'Ownership' of data is taken in the purely economic sense of the term: the servant working for the master. Plagiarism, a fatal crime in academic life, is not thought to apply to the theft of assistants' fieldnotes.

How might we look for new ways to acknowledge the specific contributions that various individuals have made to final publications?, In an afterword to a special issue about authorship and research assistants, Akhil Gupta (2014) remarked that "we lack a sophisticated vocabulary for dealing with the range of authors that help produce ethnographic work" (p. 397). Should we adopt the practice within the sciences to include between five and 15 authors (some of whom may never have seen the text)? This tradition seems to us as much a lie as sole authorship in anthropology. In both traditions, we witness plagiarism; the numerous co-authors of science publications claim authorship of a text they did not write, and the sole anthropological author steals texts written by others and presents them as his/her own.

Would giving authorship to co-researchers in anthropology be an exaggerated gesture? We may need a solution that lies between acknowledgement and authorship. For example, in his ethnography on the interpretation of illness in a Cameroonian village, Robert Pool (1994) showed in detail the role of his assistants Lawrence and Pius and their influence on the direction of the research, and how his interpretations were co-produced with his assistants. Writing reflexively is not only telling the reader who you are, but also presenting the contributions of the co-researchers, as Janneke did with Gertrude (see also Mommersteeg's (1999) research among older people in Jenné, Mali). Ann Grodzins Gold is explicit in presenting and discussing the important contribution of her assistant and research colleague Bhoju Ram Gujar, a government middle school headmaster in Rajasthan, India, with whom she worked for 30 years (Gold et al., 2014). In an article co-authored with Bhoju and his two daughters, she emphasized that ethnography is a process where "findings and minds come together and things click, or chime" (p. 335). In this article, Bhoju spoke about his perception of the collaboration and was particularly outspoken and confident about his crucial role as 'assistant':

the assistant has a more important role than the researcher, because if the researcher makes a mistake . . . no one minds because after all

she is a foreigner. . . . But as for the assistant . . . every single question that he asks should take into account the local atmosphere.

(Gold et al., 2014, p. 350)

Our attempts to recognize and discuss the positionality and contribution of assistants during research contribute to the gradual opening up of the process of “making ethnography” (Sanjek, 1993, p. 13). This trend reflects the increase of reflexive ethnography and of ethical guidelines for research and publishing. Roger Sanjek’s (1990, 1993) publications on fieldnotes and the 2014 special issue of the journal *Ethnography* on “Reinserting research assistants into ethnography’s past and present” distinctly changed this awareness. Not acknowledging the work of research assistants has become a matter of unease and apparent lack of reflexivity. Several authors now express their regret and shame for failing to rightfully recognize their assistants (Gold et al., 2014).

Reflecting on our fieldwork examples, Sjaak did not think of positioning his assistant Kwasi in the methodological paragraphs of his Master’s thesis. Kwasi’s contribution was relegated to the acknowledgements. Sjaak dedicated his thesis to Kwasi by citing an Akan proverb to tell him that their friendship would endure: *Ogya deda ano nye sɔna* (It is easy to make a fire with a piece of wood that has previously been burned). Sjaak later worked to ‘repair’ his shortcoming by creating a portrait gallery on his website to acknowledge the nine research assistants that he had worked with over the years, and what they meant for him and the fieldwork. Janneke, however, paid far more biographical and methodological attention to her assistant and described in detail who Gertrude was, how she became her assistant, how they divided the work, and Gertrude’s work in the transforming their fieldnotes into an ethnography. The difference between the two fieldwork experiences prominently show this increased awareness of reflexivity and the methodological role of assistants over half a century.

Friendship

Several anthropologists have suggested that authors conceal their assistants in attempts to hide their own inadequacies with regard to language skills or cultural competence (Gupta, 2014; Kaiser-Grolimund et al., 2016; Middleton & Cons, 2014; Pool, 1994). Gupta (2014) spoke of a “dirty little secret” (p. 393) that many anthropologists are afraid that disclosing their lack of knowledge of the language will undermine their ethnographic authority.

How credible are Sjaak’s claims of friendship? In their comments on an earlier version of this chapter, some colleagues asked us to reflect further on our positions as “Northern/Western white researchers working with Black research assistants in African country contexts.” They advised us

to be more analytical and critical about the issues of race and socioeconomic status. Sjaak felt disturbed by the word 'race.' He never used the term in relation to his ethnographic work, and never thought of living in a racist situation doing fieldwork. Had he been naïve? It is problematic to deny racism today, when debates on 'everyday racism' and 'white privilege' are widely spread. Rethinking his position of many years ago, Sjaak remembers many examples that show how he was privileged during his fieldwork. Sjaak's position and collaboration with Kwasi may have been different from 'normal' researcher-assistant relations, which could explain his unease about the qualification of racism. Maren Kristin Seehawer (2018, p. 453) pointed out that publications about decolonizing research mainly originate from or refer to societies with a recent (or ongoing) colonial presence: the Americas, Australia, New Zealand (Smith, 1999), and South Africa.

Socio-economic status, combined with what Sjaak has called 'over-admiration,' was a formidable part of the fieldwork context. If by European standards he could perhaps be considered poor, to the inhabitants of the town he was rich. When Kwasi visited Sjaak in The Netherlands, five years after Sjaak had left Ghana, he said:

The local people had a genuine cause to suspect that you were rich. You had a typewriter, a tape-recorder, a watch, a flask, and you drank 'tea' every morning. The few clothes you had were of better quality than the clothes they wore. To them these were signs of affluence, however simple you were living. Sometimes a fieldworker may have a brand of cigarette or drink which the local people never have seen. This makes them believe that the fieldworker has something they do not have. In the compound where we stayed during the fieldwork the inhabitants asked for the water which we had used for washing our clothes. They used that water, which still contained some soap, to wash their own dirty clothes.

(Bleek, 1979, p. 205)

Despite the economic gap between researcher and informants, Sjaak believed that he and Kwasi were sufficiently equal (financially and with regard to university training and age) to become friends. His lack of funds had been a blessing in disguise for the quality of his research. It forced him to live closely with the family who was the topic of his research.

The disparities between Janneke and Gertrude were starker, and the hierarchy in privilege between them (and between each of them and Mudzi villagers) cannot be denied or undone. Obviously, there is no reason to assume that a Northern European urban or Malawian urban lifestyle is inherently better and more desirable than a Malawian village lifestyle. Likewise, the pursuit of an 'MA' or 'PhD' degree should not in itself be considered of higher worth or more desirable than for example training

in appeasing ancestor spirits, protecting against witchcraft and counter-ing spells, or a career as a nurse. However, engaging in academic research opened doors for Sjaak and Janneke that did not in the same way for Kwasi and Gertrude, nor the research participants. Our presence in the fieldsites was by choice; we could leave at any time, and go to any place we desired. We felt assured of our access to formal safety nets and resource-full informal safety nets. What obligations do these privileges incur on us, towards our research companions and participants? Did we do enough to build capacity, and help create opportunities for pursuing the kind of lives that Kwasi and Gertrude aspired to? Did we ever wonder what entitled us to be there, and whether it was okay to enjoy the privilege that allowed us to be there, and further enhance our position in the global hierarchy?

When Lisa Tillmann-Healy (2003) began to write about friendship as a method of inquiry, she received “many quizzical looks” (p. 731). People agreed that friendships can arise in the context of research, but were sceptical about friendship as a methodological tool. It sounded like an unethical practice of manipulating a relationship for the sake of better ethnographic achievement. Tillmann-Healy responded that she was pleading for doing fieldwork “within an ethic of friendship” (p. 731). We too want to emphasize that friendship can improve the quality of fieldwork, in spite of claims that emotional attachment can also be interpreted as a threat to the distance that is needed for critical reflection. We believe that friendship leads to shared ethnographic experiences and mutual introspection. Both Sjaak’s and Janneke’s bonds with their research companions intensified during the course of doing research together, or rather: *because* of the research. Writing notes, exchanging personal views, sharing and discussing data forged mutual appreciation and trust, which in turn added depth and rigor to the research process and outcomes. We are both convinced that our field studies could not have been as profound without this bicultural, co-creative bond.

Two significant developments have changed the character of fieldwork relations. First, the gradual (but incomplete) ‘decolonization’ of anthropological fieldwork has created a less hierarchical work climate. With email and mobile telephones, researchers do not disappear as often used to be the case in the pre-digital era. Relationships and reciprocity continue and researchers may be continuously reminded of the needs and problems of their friends and their families in the field (Kaiser-Grolimund et al., 2016; Middleton & Cons, 2014; Parry, 2015; Seehawer, 2018). The promise of life-long friendship is made concrete by financial help, prayers, and symbolic gifts that continue to be exchanged.¹⁶

Transparency

Following anthropological tradition, we both protected the identity of our informants by changing their names and whereabouts in our

publications. However, we both also made choices related to transparency, although, interestingly, in quite opposite directions. As additional means to protect his informants' identities, Sjaak anonymized his own name. Janneke provided full access to the fieldnotes written by her and Gertrude. Both of our choices were driven by a sense of responsibility and accountability towards our informants. Sjaak felt that he owed it to his host family and community to provide privacy. Janneke felt that she owed her host community a detailed presence in the international arena where persistent prototypes of the vulnerable African woman justify all sorts of paternalism, yet do not align with Mudzi women's day-to-day struggles.

Digitalization has made its greatest impact on improving the transparency of anthropological research through making fieldnotes accessible. Our two cases show the development from a near impossibility to check the raw data and its transformation into published work to digitalized visible fieldnotes. The call for transparency and the utilization of technology to give access to raw data are closely linked to rising concerns about ethical concerns applying to fieldwork and publication. With these opportunities, new ethical concerns arise, as, for example, many anthropologists worry that funders will increasingly demand public accessibility to all their raw data. Clearly, the story of fieldnotes will continue to be at the centre of anthropological debate.

Conclusion

Our two narratives about doing anthropological fieldwork focus on the production and effects of fieldnotes and the role of research assistants therein. Situating our stories among more general discussions around fieldwork, assistants, and fieldnotes, we suspect that both of us had closer relationships with our assistants than most of our colleagues. We both lived in the same house or room with our assistants for many months, sharing meals and, in Sjaak's case, even the same bed. But the differences, in particular the extent to which we presented the assistant in our written work, are equally prominent. Sjaak made little mention of Kwasi's involvement in the research while Janneke described Gertrude's crucial role and impact on the research. Janneke's reflection represents the fast-growing awareness of the role of assistants in producing fieldnotes and "making ethnography." This awareness is congruent with and expands the trend of reflexivity in ethnography. We conclude by emphasizing present concerns about research ethics and calls for transparency, which should be seen as steps towards the decolonization of anthropology and which deserve more attention in the training of today's generation of anthropologists.

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general. Gertrude has read this essay and agreed with it. She added the point that local research assistants should not only be brought on board to assist in carrying out the research, but ideally at the earlier stage of designing the methodology. For this particular chapter, Sjaak thanks the respondents in Kwahu-Tafo and the other friends and assistants who could not be mentioned in this essay. Janneke wishes to acknowledge the Amsterdam Institute for Social Science Research (AISSR) of the University of Amsterdam and the Netherlands Ministry of Foreign Affairs which funded her PhD research. The subsequent 2014–2015 field study was funded by grant R01-HD077873 from the US National Institute of Child Health and Human Development. We lastly want to thank Casey Burkholder and Jennifer Thompson for their inspiring support throughout our writing process, and the two anonymous reviewers for their positive and constructive comments.

Notes

1. Samantha Punch (2012) promotes keeping a personal diary in the field and giving it a place in final publications.
2. See Bleek (1975). I published my Master's thesis under a pseudonym (see Van der Geest, 2003) to protect the identity of the family. I also changed the names of people and the name of the town for the same reason.
3. After reading an earlier version of this chapter, Gertrude underlined this point about the importance of speaking the language of the people studied to be able to grasp the finesses of what they say—even if this means communicating through a third person.
4. Mudzi means village in ChiChewa. I use it as a fictitious name of our research site to protect the participants' privacy.
5. This term may need some clarification. People in Ghana differentiate between degrees of blackness/pigmentation. Fair women are generally admired and so women may lighten their pigmentation with hydroquinone-based creams. *Tuntum fi* should be understood in that context. The abuse refers to an 'ugly-faced' Black woman. Wife A calls Mame Y 'dirty Black' to express her contempt for her rival (personal communication, Kodjo Senah).
6. I stayed in contact with Kwasi until his death (after a long period of illness) and visited him whenever I came to Ghana. I regret that I was unable to attend his funeral but fortunately my brother, who lives in Ghana, represented me and donated the traditional gift to his widow and the organizers of the funeral. In the booklet about his life that was distributed at the funeral, I thanked him for his help and friendship.
7. Linguistic anthropologist Kenneth Pike (1967) defined the concepts 'emic' and 'etic' as follows: the "etic viewpoint studies behaviour as from outside of a particular system," and the "emic viewpoint results from studying behaviour as from inside the system" (p. 37). The terms have become widely applied in the social sciences, although there are no standard definitions. 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.
8. The previous two paragraphs draw on Van der Geest (2015).
9. Killick and Desai (2010) wrote that the scholarly work on friendship is "haunted by the problem of definition" (p. 1).

10. In a vivid example of this dilemma, American sociologist 'James West' (1945) wrote a study about a small town in the United States which he called 'Plainville.' West was not successful in keeping the identity of the town and its inhabitants hidden. Soon after the book was published, people found out the real name of the author (Carl Withers) and town (Wheatland, Missouri). Gallaher (1961), who conducted a follow-up study of 'Plainville' 15 years later, told me that students were the first to identify 'West' and 'Plainville.' Some of them went to visit the town and irritated inhabitants with their questions. When a copy of the book was placed in the local library, someone added the informants' real names next to their fictitious ones (Van der Geest, 2003).
11. Recent scandals in all sciences, including anthropology, show that this type of fraud is far from uncommon.
12. The most sustainable option at the moment, because, unlike webpages or applications, a PDF does not require maintenance.
13. <http://hdl.handle.net/1887/21741> (Repository of Leiden University); <http://dare.uva.nl/record/1/398593> (Repository of University of Amsterdam).
14. For non-anthropologist readers: Franz Boas (1858–1942) is usually regarded as the 'father of American anthropology.' Boas was critical of the then popular evolutionist perspective and rejected the idea of higher and lower levels of development. He argued that each culture is a unique historical phenomenon with values of its own. He proposed the concept of cultural relativism.
15. But we may safely assume that this also applied to professional anthropologists in the pre-audio recorder era.
16. Marina de Regt (2015, 2019) has described the sometimes problematic financial implications of friendship in the field, and pointed at differing notions of reciprocity and money in the aftermath of her research in Yemen.

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