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Selbstreflexion im Kontext
medizinethnologischer
Langzeitfeldforschung

Self-reflection in the Context
of Long-term Field Research
in Medical Anthropology



Zum Titelbild/Front picture *Curare* 38(2015)1+2**Autor/innen dieses Heftes in ihren Feldern /Authors of this special issue in their fields**

Oben li: Das Foto zeigt Susanne Rodemeier im Gespräch mit dem Leiter des Pesantren (muslimisches Internat) Nurul Huda, Zentraljava, Indonesien. Beide tauschen sich darüber aus, wie schön es ist, dass in seinem Pesantren javanische Sprache und Tradition gelehrt wird. Eine Seltenheit auf Java, die von Muslimen wie auch Christen argwöhnisch beobachtet wird (zu S. 134–146). [Foto © Susanne Rodemeier 2012] • *Oben re:* Das Foto zeigt Godula Kosack links neben dem traditionellen Dorfhelfer Hada von Guzda (Nordkamerun). Er ließ die Autorin nach jahrzehntelanger Zusammenarbeit am 16.12.2012 zu sich kommen, um sie feierlich zu adoptieren. Danach wünschte er ein Foto von seiner Großfamilie, zu der sie nun auch gehörte. Hinter Hada stehend rechts sein Sohn, links von ihm seine Tochter, die Frau mit Kopftuch und die Frau in weißer Bluse (vorne links) sind seine Schwiegertöchter, alles andere sind seine Enkelkinder (zu S. 117–133). [Foto © Jürgen Kunze, 2012] • *Unten:* Benteng Vredebung, Yogyakarta, Welt-AIDS-Tag 2007. Das Foto zeigt die drei bereits verstorbenen *anak Congklak* (Monchi, Hari, Gendry mit Sohn und Ehefrau). Sie warnen auf einer Demo in Verkleidung gemeinsam mit Thomas Stodulka mit einem Schild: Erinnerung an den Welt-AIDS-Tag: „Pass auf Dich auf“ (zu S. 103–116). [Foto © Agung Prihartono 2007].

The photos of the front picture show three scenes of committed fieldwork: The authors Susanne Rodemeier in Central Java (pp. 134–146, *left above*), Godula Kosack in Guzda (North Cameroon) (pp. 117–133, *right above*), and Thomas Stodulka, Yogyakarta (pp. 103–116, *below*).

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Friendship and Fieldwork: A Retrospect as “Foreword”

SJAAK VAN DER GEEST

*To Kwasi Nimrod Asante-Darko
Friend and co-researcher*

In a call for papers plus introduction for a symposium on ethnography and subjectivity, ATHENA McCLEAN and ANNETTE LEIBING (2011: 190) suggested three pathways for bringing autobiography and ethnography together: (1) exploring the influence of personal life on research; (2) exploring—vice-versa—the impact of research on personal life; and (3) using ethnography as self-exploration. All three perspectives are covered in this double special issue of *Curare*. What these three pathways have in common is that they all look at anthropological research as a personal and subject-bound event that is much more than “collecting data;” it is an experience that affects and follows the researcher throughout his/her life and helps the process of self-exploration through understanding the other. In contrast to the concept of “othering” put forward by critics of orientalism and by postmodernists, the emphasis here lies on what we could call “selfing.” The encounter with the other leads the researcher to reflect on his/her own being. EDWARD E. EVANS-PRITCHARD’S answer to the question of why he travelled to such a remote and harsh environment (of the Azande) and took an interest in such a strange phenomenon as witchcraft provides a—perhaps apocryphal—anecdote in support of this. He had not so much been interested in the Azande, he replied, but mainly in himself. The Azande provided the answer to a question that had vexed him for a long time: what is rationality?

In the introduction to the special issue that resulted from the abovementioned symposium on subjectivities in fieldwork, we (VAN DER GEEST *et al.* 2012: 12) quoted Evans-Pritchard about what fieldwork had done to him:

“I wonder whether anthropologists always realize that in the course of their fieldwork they can be, and sometimes are, transformed by the people they are making a study of, that in a subtle kind of way and possibly unknown to themselves they have what used to be called ‘gone native.’ If an anthropologist is a sensitive person it could hardly be otherwise. This is a highly personal matter and I will only say that I learnt from

African ‘primitives’ much more than they learnt from me, much that I was never taught at school, something more of courage, endurance, patience, resignation, and forbearance that I had no great understanding of before. Just to give one example: I would say that I learnt more about the nature of God and our human predicament from the Nuer than I ever learnt at home” (EVANS-PRITCHARD 1976: 245).

Obviously, the mutual “lessons” that are exchanged between researcher and “others” are the consequence of the anthropological intersubjective research approach, but it also follows from the length of time that the two players interact and communicate. In my case, I have continued visiting the same Ghanaian community for research from 1969 until today. Fieldwork is not a fleeting passing-by but an extended and continuous meeting, which generates affection and mutual expectations. The editors of this special *Curare* issue therefore rightly link subjectivity and emotional involvement to the long-term nature of ethnographic research.

But before I delve more generally into the nature, perils and opportunities of the often close relationships formed during ethnographic fieldwork, let me first recount some memories from my own first anthropological fieldwork and a few—somewhat contradictory—reflections that I wrote shortly after that research period. The research was a case study of an extended family (*abusua*) in the rural town of Kwahu Tafo, Southern Ghana. The focus—on conflicts in the family—was a reaction to the over-homogeneous picture of African family life that the structural functionalist generation of anthropologists had painted.

Kwasi Asante-Darko¹

Many years ago, the Polish philosopher and anthropologist ALICJA IWANSKA (1957) wrote that North American farmers divided the world around them into three categories: landscape, machinery and people (quoted by SMALLEY 1958)². Remarkably, in her vision not all human beings belonged in the category of people. She noted that Native Americans, for example, belonged to the “landscape” and the

Mexican immigrant workers to the category of “machinery.” It seems to me that Iwanska’s perspective can also be applied to the fieldwork situation of anthropologists.

A human being is placed in the category “people” based on the assumption that this person has intrinsic value in him-/herself, and that a relationship with this person remains valuable even if he/she produces nothing (machinery) or is not interesting, beautiful or awe-inspiring (landscape). But in a fieldwork context, are informants not primarily seen as useful, and the wider social context as beautiful or interesting? Indeed, the fact that many anthropologists romanticise the lives of their respondents could be considered a sign of “landscapisation.” Furthermore, the emphasis that anthropologists often place on the good personal relationships they had with their informants does not prove that they considered and treated them as “people.” In many cases, the researcher’s assertion of good rapport with informants is more of an indirect claim of “ethnographic authority and authenticity” (DRIESSEN 1998: 132).

RUSTY NEAL and JANE GORDON (2001: 100) write that “Friendship as a practice is [...] one of the best local examples of what might be called the ‘insider’ position in the dilemmas arising from questions about the differences between insider/outsider perspectives and the research claims of ‘seeing-double’ [...]” Such “friendship,” however, seems to be grounded in utility. The more I have thought about it, the more I am of the conviction that for a researcher, it is difficult to consider and treat informants as “people” in the sense that IWANSKA and SMALLEY propose. The role of researcher leads him/her almost by definition into a utilitarian relationship with people. Informants who are “valuable” are cherished and those who fail to provide information are left aside. The inability of the fieldworker to become a ‘total participant’ shows itself in his/her inability to fully experience people as “people.”²³

Friendship is a human relationship in which the other is treated as a complete human being. The philosopher Kant has characterised the core of moral acting in a phrase that comes down to this: one must always treat others as an end in themselves; one should never use others as means to an end. Friendship could be described as a relationship in which this ideal is achieved. Such relationships are not many in a lifetime; certainly not in a research

situation, where social inequality is likely to obfuscate the emergence of friendship. Indeed, MICHAEL CRICK (1992: 176) remarks that given the disparities of power, culture and class that commonly separate researchers and informants, “speaking of ‘friendship’ [...] is somewhat odd” (cited in TAYLOR 2011: 8).

Above I suggest that the use of the term “friend” in anthropological writings is often an indirect claim of successful research. This is not to deny that anthropologists may develop affection for an informant, an assistant or someone else during their research. In a book I read after my first fieldwork experience (CASAGRANDE 1960), twenty anthropologists described such a person; but in none of these portraits did I discover a relationship that deserved the qualification “friendship.” ETHEL ALBERT (1960: 358), for example, wrote that she had an intimate relationship with her “boy,” but one wonders what she meant when she adds that in that feudal environment, he was her privileged servant and she his ruler and protector. The other essays also show that differences in education and background, especially economic disparity, obstruct relationships of friendship.

These observations notwithstanding, I believe that remarks by anthropologists about friendship in the field are generally intended as “friendly” and complimentary. It is therefore unhelpful to quibble about how to define friendship during fieldwork if it is clear that the term “friend” is being used loosely in reference to a person who has been kind and helpful during the research. In view of the fact that anthropologists often cannot find a suitable term for “informants” in the field (cf. CRICK 1992), the term “friend” is a comfortable alternative. In fact, I realise that I myself have used the term frequently for about fifteen people who have been helpful during the many years that I have been visiting Kwahu Tafo.

It may sound presumptuous to say following these critical comments, but during my first fieldwork period in Ghana I did work with someone who became a friend. His name is Kwasi Asante-Darko. If I ever had a friend in my life, it was he. We knew each other from the University of Ghana, where we studied and lived in the same hall. Kwasi decided to accompany me on my fieldwork, even though the research was not relevant to his own studies. Through the research, we grew closer to one an-

other. Our friendship was one of the most important “fortunes” of my first stay (of 4.5 years) in Ghana.

Never in my life had I been in a situation where I was so closely linked to another person. We lived together in a small room (2 by 4 meters), we slept in the same bed, ate the same food, shared the same adventures and wrote together in the two diaries that we kept. We were dependent on one another. He told me intimate things and secrets from his own life and I did the same about mine. When he noticed that I was trying to keep something hidden, he was irritated and offended. I remember one such incident. A young man had told me about all the girlfriends he had had in recent years during a long and frank conversation. Before the conversation, I had reassured him that I would never disclose his communication to anyone. I repeated that assurance at the end of our meeting. Back home, I transcribed his story and Kwasi, who had not been present during the conversation, took the text to read it. At that moment, I realised 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

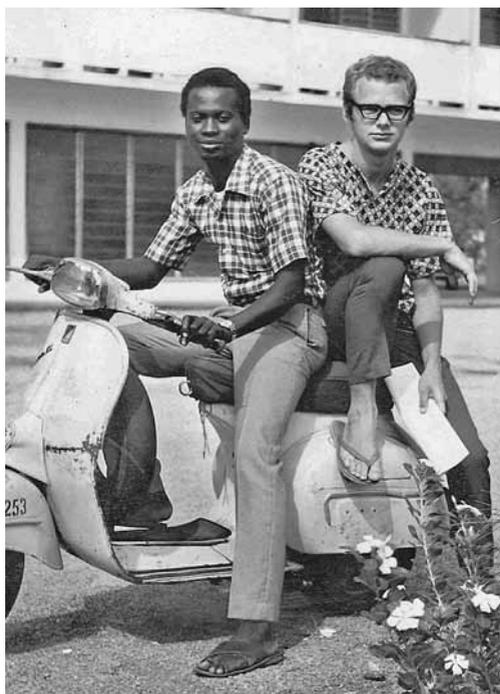
interview. However, one or two days later I realised that he had been hurt. He made it clear to me then that there should be no secrets between us and that anyone who told him something should know that I would hear it as well, and vice-versa.

The great value of our relationship for my study was that I learnt to see life in the community through Kwasi’s eyes. It brought me closer to the experiences of the people in the family. Moreover, he explained to me what specific information actually meant and what was behind the stories we recorded. At night, we had long conversations about what we had heard and seen that day or about our own lives. Those conversations were sometimes so intense that we could not sleep and decided to just get some more work done (in the dim light of one kerosene lamp). He devoted himself one hundred percent to my project. He considered it *our* research, and that identification was not based on a salary (I did not pay him) but on our relationship. When he had to return to university after three months, I suffered a severe slump. A young teacher took his place part-time. I got on with him very well, but he could not replace Kwasi.

A few years after I finished my dissertation, I wrote a short book about the fieldwork (BLEEK 1978) and asked Kwasi to write an appendix, in which he would give his view of the research and our relationship. He sent me a long letter in which he somewhat jokingly “accused” me of a lack of openness about our financial situation. He said that I had never told him how much money I had and where it came from. He had not had the courage apparently to ask me at the time. Had our friendship not been as equal as I had wished to believe?

Assistants/co-researchers

Of course, Kwasi was not one of my informants or interviewees; he was my assistant, or rather my co-researcher. Could that explain our friendship? I doubt it. Research assistants seem to me the most undervalued and exploited participants in the anthropological field, and they rarely receive the recognition they deserve. Ethnographers tend to consider the notes and reflections of assistants simply 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



Kwasi Asante-Darko and the author, 1971

of assistants' work by chief investigators.⁴ ROGER SANJEK (1993: 13), writing about ethnographers and their assistants, speaks of "intellectual colonialism," and continues:

"For more than a hundred years, members of the communities and cultures studied by anthropologists have been major providers of information, translation, fieldnotes, and fieldwork. While professional ethnographers—usually white, mostly male—have normally assumed full authorship for their ethnographic products, the remarkable contribution of these assistants—mainly persons of colour—is not widely enough appreciated or understood."

He concludes his "complaint" with a suggestion: "Ethnographers and assistants together made anthropology. We need to revise our textbooks [...]" (p. 16). One classic example of an assistant who contributed hugely to ethnography is a man called GEORGE HUNT. JUDITH BERMAN (1994) and HARRY WHITEHEAD (2000) calculated that Hunt, who was FRANZ BOAS' assistant and key informant, wrote up to 3000 of the about 5000 pages of BOAS' volumes on the Kwakiutl. In only two of them Hunt was listed as co-author, but one could just as well argue that BOAS should rather have been the co-author of most of the volumes. Berman describes Hunt's contribution as follows (cf. p. 483):

"Hunt made large, documented museum collections for Boas; he performed ethnobotanical, ethnozoological, and ethnogeographical research; and he served as a linguistic consultant and researcher. What he will perhaps be best remembered for, however, are the tens of thousands of pages he composed in the Kwak'wala language on ethnographic and folkloric subjects. These materials make up all but a small portion of the eleven volumes of Kwak'wala text and translation published by Boas."

Another striking example of the master-servant relationship is provided by MARGARET MEAD in a letter that her Balinese assistant wrote to her:

"Do you think I can write a short article about the cockfight? But I tell you if you think this action will be a bit bad for your book, I won't do it. I don't want to make profit of any of the stuff we have collected. It belongs all to you." (MEAD, quoted in SANJEK 1993: 14)

Interestingly, while SANJEK sets out to draw attention to the obscuration of the research assistant in published ethnography, he does in fact refer to several examples of anthropologists who did pay tribute to their assistants and even wrote biographies about them. The same happened to me: when

I started looking for exceptions to the "rule" of obscuration, I found many, most of them quite recent. ROBERT POOL (1994), ANNELIES KUSTERS (2011), JANNEKE VERHEIJEN (2013) and ELLEN BLOMMAERT (2014), for instance, introduce their assistant to the reader and describe his/her important contribution to the work, as I did in my research (BLEEK 1978). GEERT MOMMERSTEEG (1999) focuses on his friend and co-researcher and calls his own limited fluency in speaking Bambara a blessing in disguise; it forced him to work together with his friend, who proved indispensable in much more than translating words. Finally, ISAK NIEHAUS (2013) wrote an entire biography of his assistant Jimmy Mohale, who became entangled in a net of witchcraft accusations and died tragically of AIDS. Jimmy was keen to have his life story recorded by his friend the anthropologist, but the reader is left ambivalent. NIEHAUS produces an excellent but also painful and not always flattering anthropological monograph. We cannot deny it: other people's misery is the food of our profession. Friendship cannot stop us. My final opinion is that the biography is both a professional masterpiece and an act of love, the ultimate reconciliation between utility and friendship.

Friendship as method

Anthropologists are divided over the question of whether intimacy is an advantage or an obstacle when doing ethnography. Those who support the latter argue that knowing (too) much renders the typical 'innocent' anthropological questions impossible and the emotional bond restrains the ethnographer from writing "everything" (cf. PACK 2006). Friendship "in the field," moreover, causes role confusion, which may be detrimental to the friendship as well as to the research (HENDRY 1992). I had similar views when I began my anthropological career but—oddly and pedantically—I regarded my own experiences as an exception.

LISA M. TILLMANN-HEALY (2003), who wrote in defence of friendship as a methodological tool, made me reconsider some of my pessimistic views concerning friends in a fieldwork context. She argues that the intersubjectivity that is found in friendship provides a firm base for qualitative research and empathic understanding. Simply said, her message is not: make friends to get better data, but: carry out research among your friends.

In my own experience, friendship is linked in yet another way to research: friendship developed in the course of doing research, *because of the research*. Conversing, exchanging personal views and experiences, sharing data and discussing them, forged mutual appreciation, interest in one another, trust. “Assistants” who helped me in later research among older people became friends through our common interest in the pains and pleasures of growing old (cf. VAN DER GEEST 2011). The conversations we had with older people continued when we were together, eating or relaxing. They made us think about ourselves and about the differences in growing old in our respective societies. What struck me most was that they thanked me for involving them in the research, because they had not only felt like key informants but also like learners about their own culture. It was through my persistent questions, they said, that they had seen new things in their taken-for-granted, day-to-day lives.

TILLMAN-HEALY further writes:

“Friendship as method [...] comes with a new set of obligations that do not pave a smooth, comfortable road. When we engage others’ humanity, struggles, and oppression, we cannot simply shut off the recorder, turn our backs, and exit the field. Anyone who takes on this sort of project must be emotionally strong and willing to face pressure, resistance, backlash, and perhaps even violence” (TILLMANN-HEALY 2003: 743).

Not turning your back and exiting the field is a logical consequence of friendship built up during fieldwork. I have tried to live up to this instruction, but it is up to my friends in Ghana to judge me on this. Furthermore, disappearing altogether is no longer so easy with the arrival of email, GSM and other social media.

It is obvious that there is no simple right or wrong answer to questions about friendship or intimacy in fieldwork. My favourite example to illustrate the complexity—or should I say simplicity—of these questions is taken from JAMES P. SPRADLEY’s treatise on the ethnographic interview. Joan wants to interview her friend Bruce about football: “Could you describe a typical football game to me?” Bruce is bored and irritated. She knows what football is; why ask him such a silly question? He would have reacted very differently if an elderly Japanese man from the island of Hokkaido had asked him the question (SPRADLEY 1979: 27–28). Too much knowledge of football stands in the way of this ethnographic inter-

view between Joan and Bruce. But Joan could also have used her knowledge to delve more deeply into Bruce’s fascination with the game and would have learned far more interesting things about football than the Japanese man. Clearly, whether familiarity or intimacy is good or bad for research depends entirely on the context and the content of the conversation. But I suppose that no one will disagree, that profound knowledge and friendly relations will produce more interesting ethnography.

I nevertheless recognise the other dilemma: how much may an ethnographer reveal about people who are or have become friends? The ethical instructions of our profession tell us not to cause harm to our informants and other participants in the research. This may compel us to use fictitious names and other devices to protect their identity, as I did in my first research in Ghana (VAN DER GEEST 2003, 2011) even if the people themselves regret that their names and photographs do not appear in our publications.

In conclusion, researchers and informants/assistants may become friends, and this improves the quality of the research and enriches the personal lives of both. Or they may just make statements about friendship to further their own interests: better research data for the researcher and social or material gains for informants/assistants. Many years ago, CRAIG C. LUNDBERG (1968) spoke of a “transactional conception of fieldwork” and COLBY R. HATFIELD (1973) of “mutual exploitation.” This may sound harsh and cynical, but I myself wonder, is friendship any different from (approved) mutual exploitation?

Notes

1. This section is partly derived from my (Dutch) fieldwork account (BLEEK 1978). WOLF BLEEK was the pseudonym I used to protect the identity of the family members among whom I did my research (see VAN DER GEEST 2003).
2. I have never been able to trace and read IWANSKA’s paper. I must therefore rely on SMALLEY (1958), who summarises the content quite extensively.
3. The tension or contradiction between friendship and the practice of fieldwork has been extensively debated in anthropology; see for example: BLEEK 1979, VAN BINSBERGEN 1979, HENDRY 1992, DRIESSEN 1998, NEAL & GORDON 2001, TILLMANN-HEALY 2003, PACK 2006 and TAYLOR 2011.
4. In an attempt to make assistants more visible, I have placed a picture gallery of friends and co-researchers on my personal website—www.sjaakvandergeest.nl—to give credit to their contributions to “my” work.

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