Ethnocentric ethics in anthropological research

van der Geest, S.

[Link to publication](#)

Ethnocentric ethics in anthropological research

Sjaak van der Geest

The paper discusses the application of ethnocentric ethical rules in anthropological research. The ethnocentrism lies in the fact that North American and European definitions of right and wrong are imposed on anthropological research everywhere in the world. Apparently – and ironically – some anthropological committees seem to assume that western values are universally valid. The paper draws mainly on experiences of the author and of PhD researchers supervised by him.

In 1971 I was collecting life histories and some other data from as many as possible members of an extended family in Kwahu Tafo, Ghana. The main purpose of the research – apart from writing a master thesis – was to question the harmonious picture of *the* African family that anthropologists tended to produce at the time (Bleek 1975). A practical problem I faced was that half of the about 75 adult members of the family were living elsewhere. Most of these were 'trading' in Accra or another commercial town or farming in a remote village that could not – or hardly – be reached by wheeled transport. I was able to follow up some of these absent members by visiting them or during visits *they* paid to the family in Kwahu Tafo. There was however one person, 'Yaw Berko',[1] that I was unable to 'catch', because his farming village was far and difficult to find. Stories from others about him had fed my curiosity. I wanted to meet him. I was lucky: one day, someone came to tell me that Yaw Berko was in town. He had come for the funeral of his sister (cousin to be precise) who had died a few days before.

The next day I went to see him. He was mourning. I offered my condolences and introduced myself. After a short conversation I asked him if I could interview him about his life and some other matters. He apologised and said he could not talk to me because he was too upset and sad about his sister's death. I realised that he was 'escaping' me and insisted on having the interview. After much pressure he gave in. He was too polite

to stick to his refusal. I managed to have my interview and he – reluctantly – told me the story of his life that was as depressing as the death of his sister.

I vividly remember that event and still feel ashamed about my action. Whenever a discussion arises about the ethics of fieldwork, this memory returns. I consider it one of my most unethical acts during my fieldwork.

There are other events in my research that have been criticised by some colleagues as unethical but which I do not regret or regard 'wrong'. This one is from my diary:

> My old landlord has two wives who seem to get on well together. It looks a harmonious polygamous marriage. One night I wake up from a loud noise. Two women are shouting at one another. The door of my room leaves a big split. Through it I can see one of the two wives. The old man tries in vain to calm them down. It is difficult for me to understand what they are saying. It goes too fast and there are many unfamiliar words in it. I hold the microphone of the cassette recorder in the split of the door and record their 'conversation'. The next morning my assistant translates it word by word: "You with your crooked ass," "You are black and dirty," "You better take your bath in the afternoon than in the evening," "You with your cracked heel," and so on.

The quarrel had causes and consequences, which kept me busy the following days. I had to readjust my understanding of the old man's marriage and to change my somewhat romantic ideas about polygyny.

Two years after my first research I returned to Kwahu Tafo to study sexual relationships and birth control which led to my doctoral dissertation (Bleek 1976). I employed several 'dubious' methods during that research. One was to find out more about secret love affairs in the family:

> I asked a young boy (about fourteen years old) who lived in the same house, to check who slept with who in four adjacent houses of the family. In the evenings he hung around the four houses and observed who entered. The next morning he came to have breakfast with me and delivered his report about the sleeping partners of the past night. He became my spy and enjoyed the work. Moreover, he needed the money that I paid him. As far as I know, noone ever found out about his spying activities.

I was not aware, at that time, of an official ethical code for anthropological research. But I don't think I needed such a code to realize that my treatment of Yaw Berko was wrong and unethical and to explain why I thought that my secret recording of the fighting wives and of the nightly visits of lovers was acceptable. I will return to these doubtful ethics in fieldwork later on.

In their plea for 'empirical ethics research' in health care, Willems and Pols (2010: 167-168) write that

Empirical ethics leads to other forms of insight and understanding than predictions and even if it does lead to predictions, testing these predictions is not always the most important way to evaluate the theory. There are more suitable criteria, such as the extent to which new and surprising concepts are introduced, unexpected connections are revealed, new ways of seeing and understanding are opened up and the extent to which it helps people to live with problems and dilemmas in health care.

In this paper I will reflect on a number of ethical decisions in anthropological research in my own career and that of a PhD researcher and some colleagues. The reflection will reveal some of the 'unexpected connections' that Willems and Pols refer to.

Ethnocentrism

Over the past three decades or so, anthropology as a profession has become increasingly more conscious of the ethical implications and complications of doing research and publishing its results. National associations of anthropology in several European and in the Americas have attempted to design guidelines for correct ethical conduct during and after research. The most influential is the Code of Ethics from the American Anthropological Association (AAA 2000). I will, therefore, use this code as reference for my exploration into potential ethnocentrism in the conceptualisation and application of ethical codes. The AAA code explains in its preamble:

Anthropological researchers, teachers and practitioners are members of many different communities, each with its own moral rules or codes of ethics. Anthropologists have moral obligations as members of other groups, such as the family, religion, and community, as well as the profession. They also have obligations to the scholarly discipline, to the wider society and culture, and to the human species, other species, and the environment. Furthermore, fieldworkers may develop close relationships with persons or animals with whom they work, generating an additional level of ethical considerations … The purpose of this Code is to foster discussion and education. The American Anthropological Association (AAA) does not adjudicate claims for unethical behavior … The principles and guidelines in this Code provide the anthropologist with tools to engage in developing and maintaining an ethical framework for all anthropological work.

Speaking about research, the AAA code refers to three parties to whom the researcher bears responsibility: to the people (and animals) they study; to their discipline; and to the general public. Focusing on the first (the research participants) the code brings up six issues that boil down to these three: (1) respect for people (which may imply that the research is cancelled because it fails to honour the principle of respect); (2) not harming the safety, dignity or privacy of people (which may necessitate anonymisation of the respondents); (3) obtaining informed consent. Missing in this list, in my view, is the obligation to inform the population about the findings and conclusions of the research.

I will now discuss four ethical issues which largely coincide with the points I just mentioned: exploiting or 'using' people for the sake of the research; informed consent; not harming; and sharing research results. The discussion will take the form of a dialogue with my own research experiences and those of some close colleagues. By placing the ethical decisions and dilemmas in the real life context of anthropological research, I hope to capture the empirical and situational character of research ethics.

Duplexity

In an article on ethics and fieldwork, Peter Pels (1999) calls anthropology a profession of duplexity. Anthropologists, he argues, serve two masters; they have to satisfy opposing demands. Those different masters or demands change over time and vary over places but anthropologists find themselves always in a situation where they have to apply considerable 'impression management' to achieve what they came to do: (1) building friendly relationships with the people they study in order (2) to collect the best data that will enable them to write a text that will be appreciated by academic colleagues, funding organizations or policymakers. Thus duplexity easily leads to duplicity, deceit, double-dealing. I agree, anthropologists walk a tight rope; they must balance between sincere friendship and instrumental 'friendship' (which is not friendship). Going by the title of her article "Friendship as methodology," one would think that Tillmann-Healy (2003) has fallen from that tight rope, but I don't think she has. She rather 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 was in yet another way linked 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 the research among older people became friends through our common interest in the pains and pleasures of growing old. That kind of friendship, which dissolved most of my duplexity, developed also in my 'contract' with the young boy who reported over breakfast on the nightly lovers in the family. My growing involvement in family rumours and gossips, thanks to my anthropological 'observations', meant both methodological and personal progress.

I am not denying that conflicting demands during fieldwork can lead to ambivalence, discomfort and double standards[2] but is that a uniquely anthropological fieldwork experience? Of course not. Playing two, three, four roles is a necessity of life in general. Without 'impression management' no one can socially survive, as Goffman (1969) has shown so eloquently, whether he/she is an anthropologist, a medical doctor, a teacher, a politician or a shopkeeper.

How respectful the researcher has been towards the participants in the field and how he handled the problem of 'duplexity' is difficult to verify by others. It is mainly fieldwork confessions and reflection that show how the researcher associated with the people in his study. Occasionally a third person reveals certain unethical activities,

as – for example – happened in the case of Chagnon. Chagnon, who carried out extensive anthropological research among the Yanomamö people in Venezuela and Brazil, was accused of infecting these people with measles for the sake of scientific research (Thierney 2000).[3]

Until today people who are visited by an anthropological researcher seldom talk back and in most cases the only 'vocal' eyewitness of the researcher's conduct is the researcher himself. It is not surprising, therefore, that journals rarely reject an article because the author is known to have collected his data in a disrespectful manner.

Informed consent

'Informed consent' is the most concrete ethical rule, which – for that reason – is most frequently applied by editors and publishers in judging the ethical correctness of research before publishing. The American Anthropological Association says the following about informed consent:

> Anthropological researchers should obtain in advance the informed consent of persons being studied, providing information, owning or controlling access to material being studied, or otherwise identified as having interests which might be impacted by the research. It is understood that the degree and breadth of informed consent required will depend on the nature of the project and may be affected by requirements of other codes, laws, and ethics of the country or community in which the research is pursued. Further, it is understood that the informed consent process is dynamic and continuous; the process should be initiated in the project design and continue through implementation by way of dialogue and negotiation with those studied. Researchers are responsible for identifying and complying with the various informed consent codes, laws and regulations affecting their projects. Informed consent, for the purposes of this code, does not necessarily imply or require a particular written or signed form. It is the quality of the consent, not the format, that is relevant (AAA 2000: 3).

The above formulation is liberal enough to allow for adjusting 'informed consent' to local conditions and type of research. The code does not insist on a written and signed form, which would be quite out of place and preposterous in most anthropological research situations. It is "the quality of the consent" that is important. What this 'quality' entails can best be illustrated with examples from my own research.

I began this paper with the worst example of my style of 'interviewing' but most of my interviews resembled 'normal' conversations that often started as conversations do, spontaneously and informally, with the implicit understanding that both parties agree, otherwise they would not engage in it. But I also approached people more formally for a 'long conversation' (or 'discussion') on a specific subject. I would for example ask: "Can we talk today about how it feels to be an old person?" Or: "How you built your house?" Nearly all older people enjoyed my visits and were happy that there was someone who took an interest in what they had to say. I soon discovered why: it rarely happened that someone came to them with questions about their life his-

tory. One of my dearest quotes is what an old man confided to me: "There is no greater pleasure for an older person than a young person asking a question."[4] One older man was so pleased with my visits that he came to search for me if I failed to visit him. It would indeed have been absurd to ask him and the other older people to sign a form of consent, before starting our conversation. It would have killed the occasion and probably frightened the elder. Zaman and Nahar (this issue) provide striking examples of this absurdity from their fieldwork in Bangladesh.

Were the elders 'informed' about the purpose of my conversations? I told them but I am not sure they fully understood and remembered what I had told them. In general, people in the town knew what I was doing: writing a book about the way older people live and are respected and they liked that idea.

But there were also occasions that I did not fully inform the people about the purpose of my conversation. It would be odd to do so in situations where a conversation naturally developed and it would be unwise when telling them would spoil the very purpose of the meeting. Asking a nurse if she takes medicines home without permission, would be senseless if I tell her that I am studying informal practices in the hospital. Not informing and not asking consent is more rule than exception if we include 'observation'. Everything I see, hear, or smell becomes 'data'. It would be ridiculous, for example, to ask two children (or their mothers) who are fighting for permission to write down what I observed.

Ethnography is permanent sensitivity, which is impossible to combine with informed consent. The most incisive reflections on illness and suffering have been written by authors who were themselves thrown into the depth of pain and uncertainty and reported on their experiences from that situation. A telling example is Gerhard Nijhof's (2001) description of his stay in the hospital and slow recovery from cancer. There is no room and no need for informed consent from doctors and nurses in this lucid account, as long as their identity is not revealed to the reader. I assume that only his partner and children, who are easily recognisable, gave some kind of 'consent' to the publication of his reflections.

An extreme form of no informed consent is research that can only be carried out if under cover. Asking consent would destroy the very purpose of the research because it would almost certainly make the research subjects fabricate misleading information. There are countless examples, many of which involve research into ideas or practices that are socially disapproved of, such as racism and other forms of discrimination, dubious selling practices, corruption, theft, violence, etc. Researchers, for example, posed as mothers of a sick child and asked drug sellers for the best treatment (Tomson & Sterky 1986; Wolffers 1987) or asked permission from doctors to study patients without telling them that they would also observed the doctors and nurses (Greenhalgh 1987; Zaman 2005; Zaman & Nahar 2011; Van der Geest & Sarkodie 1998). In all these cases the ethical acceptability depends on the researcher's honest weighing the goal and the means, a condition that producers of codes of conduct may find far too slippery.

Moreover, the impossibility or the absurdity or the strategic refusal to ask informed consent, must be balanced by the way the researcher handles his data. I have always taken utmost care that the identity of the people I studied was kept confidential if that

was what they wanted or if I judged this necessary in all sincerity. I will return to this later on.

Those who think they have to watch the ethical correctness of anthropological researchers, may feel that they are losing sight and control of what takes place during fieldwork if such 'subjective' and 'whimsical' ethical decisions are practised. The result is that they look for more concrete and 'solid' criteria to carry out what the AAA in its preamble says they should *not* do: to "adjudicate claims for unethical behavior." A telling example of sticking to the code as if it was a law, happened to my once PhD student Francine van den Borne.

She carried out research on casual sex and condom use among young girls and women who sexually network in and around entertainment places like bars, bottles stores, and discos in urban Malawi (Van den Borne 2005). Aware that these women might provide socially desirable answers, she used as one of her research methods trained and closely supervised fake male customers to find out how and where women barter sex, whether they possess condoms and would negotiate for condom use. To their surprise, some women who negotiated in the bar for condom use, even bought the condoms and carried them to the room, were willing to accept plain sex. Her research proposal was discussed by the National AIDS Control Programme and by the Ethical Review Committee of the Ministry of Health and Population in Lilongwe prior to the fieldwork. The Committee agreed with the proposal and its ethical implications but was concerned about the fake clients 'wasting' the women's time and causing them financial losses. If the fake customers compensated the women for their 'lost opportunities', the Committee did not have any ethical objection and provided proxy consent. Committee members were not worried about the unconsented and fraudulent research method. They were convinced that the mystery client method was the only method that could provide reliable and contextualised information about the effectiveness of the peer education training method and gain insights into what is really going on in the process of negotiating for sex. Committee members, however, felt that the women should not suffer financially from the 'cheating' because of a research setting. They argued that the country and the Ministry would benefit from the deception: the research would assess the value of the peer education method and produce more reliable information on sexual practices in an HIV-infected society and would thus provide better policy and programme recommendations. Van den Borne followed their advice and compensated the women for the time they 'wasted' on her mystery clients.

However, when Van den Borne submitted an article to an international social science journal about the background, rational and ethical implication of this method and about the training and supervision of the male fieldworkers who acted as fake clients, it was rejected. The anonymous reviewers and the editor considered her method to be a gross breach of the ethical code. They thought she had misled her informants, which is never allowed. Not even, it seemed, if this served an extremely important purpose. The Malawian Ethical Review Committee found her method justified provided the girls and women were compensated, but what they considered right for their own society was overruled by an alien code of western scholars. In her reaction to the editor Van den Borne wrote:

> A straightforward application of those international guidelines [the anthropological code] remains problematic and is unable to address the tensions between universalistic and relativistic perceptions of ethics when dealing with transcultural research. The intention of my article was exactly to present those ethical concerns to my international colleagues and start a discussion. By not publishing this article such a discussion is prevented from taking place. By publishing the article your journal would show its concern about proper ethical conduct in the field.

But she did not convince the editor … I agreed with her defence and was relieved that her article was eventually published elsewhere – after two years and several more failed attempts (Van den Borne 2007).[5] In retrospect, I believe that she could have collected the same information through her excellent relationship with some of the women. Yet, *seeing* the women's indulgence to clients' pressure in actual practice was more convincing and – therefore – was likely to have greater policy impact.

Not harming

I always promised confidentiality to the participants in my research. I told them (perhaps somewhat arrogantly) that they could trust me: what they told me in confidence would not be revealed to others, whether it was about witchcraft accusations, secret love, abortion, HIV/AIDS, or any malicious gossip. At the same time I planned to write about the things they told me, in great detail. The only way to combine these two opposing objectives was to guarantee 'absolute' anonymity.

It is a good anthropological tradition to give one's informants and their community pseudonyms, but I soon realized that in this case such a measure would be an insufficient guarantee of confidentiality.[6] Ghana's academic community is like a village. Through my (the author's) name it would be simple to trace the identity of the town and consequently of the informants. Moreover, two young people from Kwahu-Tafo were students at the same university where I was completing my master thesis on marriage, inheritance and witchcraft accusations in one particular family (Bleek 2005). They could easily read my published accounts in the university library. After giving fictitious names to the town ('Ayere') and the people, I decided I had to hide my own identity as well if I wanted to protect the people who had told me about their confidential – dangerous and 'shameful' – experiences. I chose the name Wolf Bleek as a pseudonym for myself.

Using a *nom de plume* seems to draw ethnography into another genre of writing: semi-fiction. Laura Bohannan's (Bowen 1964) ethnographic 'novel' is a case in point. The pseudonym, some may think, frees the author from the obligation of ethnographic precision and rigor. I cannot speak for Bohannan but in my case the opposite was true. By changing my own name, I was able to maintain many ethnographic details, which I would have been forced to change if I had published the text under my real name. In fact, I chose a pseudonym *because* I did not want to give up the richness of my observations. Paradoxically, perhaps, my *nom de plume* helped me to prevent 'semi-fiction' from sneaking in.

It is indeed novelists and poets who hide their identity behind pseudonyms, but it is unusual to do so in academic work.[7] Why? Academic work needs to be transparent and verifiable. Literary authors write mainly about themselves – either directly or indirectly. By using a pseudonym they protect themselves. This may be an accepted practice in the world of literature, but an anthropologist writing about others is not supposed to do so. Apparently, protecting others is a less pressing motive for using a pseudonym.

When I tried to publish an article on self-help abortion in *Population Studies*, it was rejected because: "… the editors would like to publish your paper … but they cannot agree to your using a pseudonym." No reasons were given, but one can imagine why the editors were uncomfortable with an anonymous author: it seemed irreconcilable with their concept of scientific work. My argument about protecting people's identities did not convince them, in spite of the ethical statement cited at the beginning of this note. After two more failed attempts to have a pseudonymous manuscript accepted for publication I started to submit my articles without mentioning my use of a pseudonym. It worked. The results of my research on witchcraft and abortion appeared in international and Dutch journals and editors and colleagues started to correspond with 'W. Bleek'. In the course of the correspondence I usually revealed my real name but asked them to respect the pseudonym in their published work. As it turned out, the pseudonym in no way prevented me from discussing the content of my work with others and exchanging views on the social, cultural and moral aspects of witchcraft and induced abortion.[8]

The irony of my ethical concerns about confidentiality was that the academic world of publications overruled them and let its own 'scientific' rules prevail: complete openness about identity of the author even if this could harm the participants.[9]

Sharing results with research participants

My triple disguise (informants, location and author) in Ghana did work.[10] Twenty years later my measure proved still effective. There was no copy of my thesis in a local library and no one had established a link between my publications and the community. I should have been content, but I was not. My decision to 'go into hiding' had a number of consequences that I found both unethical and simply annoying. I had kept the outcome of my research study from them, 'for their own good'. On the one hand, I had respected their wish (and the first article of the anthropological ethical code) to keep delicate information confidential; on the other hand, I had deprived them of the possibility to read what I had written about them (which, surprisingly, is not demanded by the anthropological code). They would never be able to 'talk back'. Trying to make their voices heard by writing about them, I had effectively silenced them. Lairumbi et al. (2008) rightly comment that informed consent also implies that the participants are informed about the results of the research. I could not inform them.

When in 1973 I finished my fieldwork in 'Ayere', I asked the head of the family to assemble as many relatives as possible in his compound. I took a number of pictures of the entire group and promised the old man that I would send him a large print of the picture to hang in his house. The man's reaction was significant: A photograph would be nice, but the book was more important. I nodded, but knew that I would never be able to

give him 'the book'. There was too much in the book, which would upset him and his relatives. My concern about protecting people's anonymity prevented me from giving them the text, which would betray their identities to other members of the community. It also prevented me for many years from returning to the town and the people who had become my friends. I knew that people would ask me where 'the book' was.

Twenty-one years after I had left Kwahu Tafo, I went back to the town to study social and cultural meanings of growing old. I reasoned that 21 years was a long period and that things of the past had lost their pungency; the elderly who had been most accused of witchcraft had all died and the teenagers who had told me about their romantic encounters and the subsequent pregnancies and abortions were now parents and grandparents. I brought a few copies of my dissertation along and formally handed them over to the new head of the family and some others. The head, a former schoolteacher, expressed his disappointment that the name of the town was not mentioned on the cover or inside the book and that his name or any other did not appear in the text.

Had I been too concerned? Was my worry about confidentiality an attitude typical of my own culture with its emphasis on individuality and privacy? Had I made the wrong decision when I chose not to share the outcome of my research with them because of confidentiality?

Since, I have written articles and a popularising little book about the elderly of this rural community. I describe how they spend their days and how they are cared for. The articles deal with respect and reciprocity, with money, building a house, wisdom, loneliness, death and funeral; they also discuss topics that were anonymised in my earlier research: witchcraft accusations and sex.[11] I have changed my policy, however. The name of the town, Kwahu Tafo, is now boldly mentioned in the articles and so are the names of the old people with whom I conversed about the pleasures and pains of growing old. I have made it a rule to dedicate each publication to one of the elders. I want them to be proud of the fact that their life histories – good or bad – and their reflections about being old have been published and are being read by people in different parts of the world. Of course, the information they provided is less threatening than what their relatives told me in 1971 and 1973, but also when less favourable things are said by or about them, I do not hide them. They said they did not like my 'confidentiality'. They wanted to see their names on paper. Their main worry was that after death they would sink into oblivion. My writing about them would help them to be remembered.

Conclusion

Are anthropologists subjected to unreasonable and counter-effective ethical rules? Do they face ethnocentric instructions that are ignorant about local cultural ideas concerning 'correct' and 'wrong'? The answer needs some nuance. Ethical guidelines proposed by anthropological associations are phrased in such a way that they leave room to anthropological researchers to act in accordance to local cultural values and to react reasonably to unpredictable situations in the field. The manner in which these carefully worded guidelines and suggestions are applied in concrete situations, for

example by editors of journals and books and by evaluation committees of doctoral theses leaves, however, much to be desired.

The anthropological code of ethics, as formulated by the American Anthropological Association (AAA 2000) shows sensitivity to situational variations and does not impose a judicial type of ruling. The Code wants to provide the anthropologist "with tools to engage in developing and maintaining an ethical framework." But the way in which some players in the academic powerhouses apply the Code, shows a remarkable lack of sensitivity to local, social and methodological conditions, up to the point of acting unethically and potentially harming the participants in the research. The examples I presented showed the (seemingly) 'whimsical' nature of what is good and what is bad in research situations. Fundamentalist interpretations and applications of the ethical code may thus bring about the opposite of what the code wants to achieve and promote ethnocentrism in the heart of an academic tradition that many years ago started as a reaction against ethnocentrism.

Notes

1   This name is a pseudonym to protect his identity. I named him after a character in a famous Highlife song. Yaw Berko, in the song, was a tragic figure; he had travelled from town to town, but never found his luck. 'Yaw Berko' died in 2010, at home in Kwahu Tafo.
2   See my chapter on 'etic' and 'ethic' (Bleek 1978: 80-92), and my discussion with Wim van Binsbergen on dilemmas of instrumentality and friendship in fieldwork (Bleek 1979, 1980; Van Binsbergen 1979). 'Wolf Bleek' was the pseudonym I used for myself to protect the identity of the family members among whom I did my research (see further below, and: Van der Geest 2003).
3   Most of Thierney's accusations were later on proved false and slanderous, but his book did bring out some other – less dramatic – dubious practices of the researchers.
4   I see the absence of people asking questions as the core experience of loneliness among older people in Kwahu Tafo (Van der Geest 2004).
5   This case was also presented in another article (Van der Geest 2003) and discussed in Cash et al. 2009.
6   The next paragraphs draw on an earlier article on confidentiality and use of pseudonyms (Van der Geest 2003).
7   I know only a few examples of anthropologists (and sociologists) using a pseudonym: Carl Withers (James West 1945), Laura Bohannan (Elenore Bowen 1964), and Karla Poewe (Manda Cesare 1982).
8   The only problem I encountered was when the organizers of a conference invited me to present a paper and sent me a prepaid air ticket on the name of Wolf Bleek. Having no of-

ficial *nom de plume* identity (as artists have) I almost failed to have the ticket changed to my real name.

9   Interestingly, at the time, the supervisor of my master thesis at the University of Ghana did not even allow me to use pseudonyms for the town and the informants of my study. It became a 'case' in the department that I won.

10 To be very exact: I also changed the dates of my research and some insignificant details in the life histories of a few informants.

11 See for example Van der Geest 2001, 2002, 2007.

References

Bleek, W.
   1975   *Marriage, inheritance and witchcraft: A case study of a rural Ghanaian family.* Leiden: African Studies Centre.
   1976   *Sexual relationships and birthcontrol in Ghana: A case study of a rural town.* Amsterdam: Antropologisch Sociologisch Centrum.
   1978   *Achter de coulissen: Antropologisch veldwerk in Ghana.* Assen: Van Gorcum.
   1979   Envy and inequality in fieldwork: An example from Ghana. *Human Organization* 38 (2): 200-05.
   1980   Envy and inequality in fieldwork: A rejoinder. *Human Organization* 39 (3): 291-93.
Bowen, E.S.
   1964   *Return to laughter.* Garden City, NY: Doubleday.
Cash, R. et al.
   2009   *Case book on ethical issues in international health research.* Geneva: WHO.
Cesara, M.
   1982   *Reflections of a woman anthropologist: No hiding place.* London: Academic Press.
Goffman, E.
   1969   *The presentation of self in everyday life.* Harmondsworth: Pelican Books [1959].
Greenhalgh, T.
   1987   Drug prescription and self-medication in India: An exploratory survey. *Social Science & Medicine* 25: 307-19.
Nijhof, G.
   2001   *Ziekenwerk: Een kleine sociologie van alledaags ziekenleven.* Amsterdam: Aksant.
Pels, P.
   1999   Professions of duplexity. A prehistory of ethical codes in anthropology. *Current Anthropology* 40 (2): 101-14.
Thierney, P.
   2000   *Darkness in El Dorado: How scientists and journalists devastated the Amazon.* New York / London: W.W. Norton & Company.
Tillmann-Healy, L.
   2003   Friendship as method. *Qualitative Inquiry* 9 (5): 729-49.
Tomson, G. & G. Sterky
   1986   Self prescribing by way of pharmacies in three Asian developing countries. *The Lancet* 1986 (2): 620-22.
Van Binsbergen, W.M.J.
   1979   Anthropological fieldwork: "There and back again." *Human Organization* 38 (2): 205-09.

Van den Borne, F.

2005   *Trying to survive in times of poverty and AIDS: Women and multiple partner sex in Malawi.* Amsterdam: Het Spinhuis.

2007   Using mystery clients to assess condom negotiation in Malawi: Some ethical concerns. *Studies in Family Planning* 38 (4): 322-30.

Van der Geest, S.

1989   Censorship and medical sociology in The Netherlands. *Social Science & Medicine* 28 (12): 1339-41.

2001   "No strength": Sex and old age in a rural town in Ghana. *Social Science & Medicine* 53 (10): 1383-96.

2002   From wisdom to witchcraft: Ambivalence towards old age in rural Ghana. *Africa* 72 (3): 437-63.

2003   Confidentiality and pseudonyms: A fieldwork dilemma from Ghana. *Anthropology Today* 19 (1): 14-18.

2004   "They don't come to listen." The experience of loneliness among older people in Kwahu, Ghana. *Journal of Cross-Cultural Gerontology* 19 (2): 77-96.

2007   "It is a tiresome work." Love and sex in the life of an elderly Kwahu woman. In: C. Oppong, Y.P.A Oppong & I.K. Odotey (eds), *Sex and gender in an era of AIDS: Ghana at the turn of the Millennium*. Accra: Sub Saharan Publishers, pp. 211-32.

Van der Geest, S. & S. Sarkodie

1998   The fake patient: A research experiment in Ghana. *Social Science & Medicine* 47 (9): 1373-81.

West, J.

1945   *Plainville, U.S.A.* New York: Columbia University Press.

Willems, D. & J. Pols

2010   Goodness! The empirical turn in health care ethics. *Medische Antropologie* 22 (1): 161-70.

Wolffers, I.

1987   Drug information and sale practices in some pharmacies in Colombo, Sri Lanka. *Social Science & Medicine* 25: 319-23.

Zaman, S.

2005   *Broken limbs, broken lives: Ethnography of a hospital in Bangladesh.* Amsterdam: Het Spinhuis.

Zaman, S. & P. Nahar

2011   Searching for a lost cow: Doing medical anthropological research in Bangladesh. *Medische Antropologie,* This issue.