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The translation of anthropological data: *How to approach the chaos of fieldwork?*[1].

Abstract The translation of anthropological data into ethnography is a fuzzy process which all ethnographers go through. The process starts with the methodologies and techniques used in the field to gather data. These are used to get a grip on and order an often very chaotic reality. Through the process of analysis and description this ordering continues. The assumption behind this type of research is that people and societies can be understood as objects of knowledge. In this article the author offers some insight into the process of gathering data and its translation into ethnography, using her own experiences in dry land Mali among cattle keepers. She concludes that anthropologists should leave more room in their fieldwork for the non-ordered realities, for moments of silence. Anthropologists should not be afraid not to understand everything they see and meet. These incomprehensible moments and events should probably be made the core of research.

> Anthropology, at the outset, is the art of relating. The experience of others, whether they be from there or over there, requires an effort at understanding, an openness of consciousness that allows itself to be penetrated by the world to the point of certain forgetting, or at least a relativization of oneself (Gibbal 1994: 163).

Anthropologists are primarily specialists in translation. However, translation implies many risks. When we take the metaphor of language, the many meanings of one word must make us doubt our endeavour as anthropologists. These doubts are more and more raised in the era of postmodernism. Translation of what is observed, written down and experienced in the field is no longer guided by very strict rules and norms of our own discipline. Neither is there a clear idea of how society works. The clear discrepancy between fieldwork and the result - ethnography - has been illustrated by the confessional literature in anthropology. Only few anthropologists dared to write down their experiences under their professional name, presuming that distance between the real world and the written version is not so very big. Practice translated with the help of theory has become the trap of the anthropologists and the anthropology.

Palsson (1996) highlighted this point for theorising in the human ecology school: 'In some cases, ethnographers idealise and relativise the world of their hosts, representing their relations in terms of a protective contract. Despite the argument of protection, such a position only maintains the orientalist distinction between the observer and the native' (Palsson 1996: 70). But he realises that today anthropologists are more open for experiment: 'Recognising the importance of trust and communalism, anthropologists engage themselves in a serious ethnographic dialogue with the people they visit, forming an intimate rapport of communication. The communalism of fieldwork may be characterised as a project in which anthropologists and their hosts engage in meaningful, reciprocal enterprises, as the inhabitants of a single world' (Palsson 1996: 73). But if data are collected in

dialogue, how must they be represented in ethnography? The way out proposed by Palsson may be a good idea but it is not very practical: 'The metaphor of romance (...) allowing for some degree of future hope, in a world with contesting perspectives, conflicting interests, and unexpected turns' (Palsson 1996: 76). This still leaves us with the problem of translation. As pointed out in this essay, it is very difficult to translate the experienced practice, emotions, etc. in an ethnographic text because the methodological tools that are most frequently used by anthropologists and of which they are very proud - participant observation, the open interview - can hardly get a grip on and hardly help us to understand the apparent chaos of society. This may partly be explained by the assumption behind this type of research, that people and societies can be understood as objects of knowledge, which in my opinion they cannot (cf. Gibbal 1994: 170).

My argument is based on research carried out with my husband, Han van Dijk, among the Fulbe in Central Mali, Sahara. Being there and trying to understand their life was not easy, neither for us nor for them. The confrontation with our opinions and our views of society made us realise the impossibility of anthropology (mission impossible) and made them realise even more the difficulties of the reality they lived in. Nevertheless, we did our job and wrote a dissertation on the Fulbe in Central Mali (De Bruijn and Van Dijk 1995). Trying to provide insight into the process of learning the culture and society is not stimulated by the rules and norms imposed on ethnographic writing. It is not easy to get rid of the theoretical and descriptive models and terminology that are very common in anthropology but that obscure the lived reality. This article gives me the opportunity to provide more insight into the process of doing the fieldwork and the chaos that is inevitably part of it. This chaos is the reflection not only of our own worrying, but also of the real life situation into which we were introduced.

The research outcome, the data one comes home with, are all gathered in a process of interaction with the people there. Another important process that continually goes on during the fieldwork is the definition of the other, from both sides. This goes together with accepting each other and with the inclusion of the fieldworker in certain domains of the culture: social groups, ideas, etc. It is also part of trying to understand each other's language and trying to understand the different layers of meaning of all kind of actions and events (individual as well as communal), trying to understand each other's ordering of the world. In this sense the anthropologist becomes part of a process of inclusion and exclusion (De Ruijter 1995). However, as researchers we should not be too afraid of not understanding everything. And we should have the courage to leave it that way. As Gibbal (1994: 171) put it: 'It is ... not necessary to idealise people, nor to pretend to understand them totally, in order to love them. One must also know how to accept them in their obscure and incomprehensible difference. Yet among those faced with the task of revealing the real, a widespread attitude consists of ignoring or contesting the

existence of that which is not comprehensible in terms of established knowledge'.

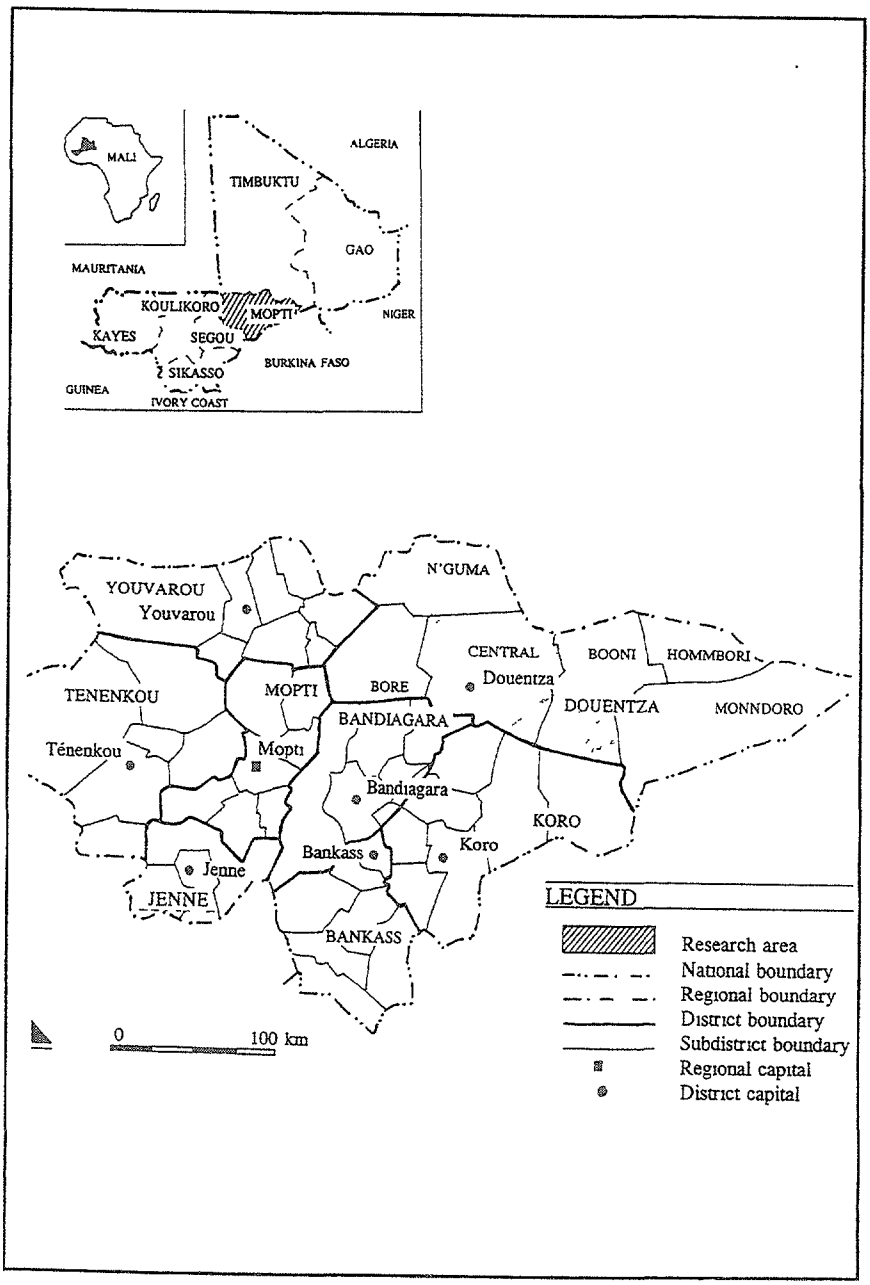
Although the problems I describe in this article are partly universal in the field of anthropology and related disciplines, the situation was probably more extreme than most other fieldworkers come across. The Fulbe in this part of Central Mali were confronted with many calamities and hazards during the last three decades. This influenced their culture, social life and daily realities enormously. Poverty was widespread in the region. The confrontation with poverty was very direct and hard, and influenced profoundly our time there and the time of writing at home.[2]

In this situation research on social and human security had to be turned into research on human and social insecurity. This insecurity was not only reflected in poverty, in illness and in a very high rate of mortality, but also in the rapid change of social networks, of rules and norms in society. The extent of these changes has been described elsewhere (De Bruijn 1994, 1996a, 1996b, De Bruijn and Van Dijk 1992). Research into these issues confronted me especially with the distance between the worlds of the researcher and the Fulbe. Talking about misery, and poverty and the disorganisation as experienced by the people themselves was not easy at all.

In this article I will try to provide insight into the way I did research in this situation and came up with some results. First I will present the Fulbe and their life in the early 1990s, and their perception of this reality. The following sections will focus on the practice of fieldwork under these circumstances. How does a researcher cope with interviewing the poor, with severe illness, with death, with the disturbed social realities people live in, i.e. important issues of human and social security in the area? I will describe a few incidents or events that occurred during our research in Mali (1990-1992). I concentrate on my experiences in a settlement of herders (Jallube) and former slaves (Riimaybe).

Fulbe in Central Mali: coping with insecurity

The Fulbe are one of the largest groups in West Africa. They extend from the Western Sahara (Senegal) to Sudan and Ethiopia. They are considered as one group by most researchers because of their linguistic unity, though there are many different dialects (Breedveld 1995). Their lifestyles do not reflect a unity. They live in urban as well as rural regions and their activities vary from being pure cattle and camel nomads, to agricultural pastoralists and to urban merchants or Islamic scholars. In history the Fulbe have themselves formed states or lived on the periphery of states (see Azarya 1996). The various groups also differ in their cultural expressions, not only in relation to their lifestyle, but also under the influence of the contacts with neighbouring ethnic groups, and of the political structures of which they were a part. The (agro)pastoral groups living in the Sahara


MALI
ALGERIA
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KOULIKORO
MOPTI
NIGER
KAYES
SEGOU
BURKINA FASO
SIKASSO
GUINEA
IVORY COAST
YOUVAROU
Youvarou
N'GUMA
CENTRAL
BOONI
HOMMBORI
Douentza
TENENKOU
MOPTI
BORE
DOUENTZA
MONNDORO
BANDIAGARA
Ténenkou
Mopti
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KORO
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Jenne
JENNE
BANKASS
0
100 km
LEGEND
Research area
National boundary
Regional boundary
District boundary
Subdistrict boundary
Regional capital
District capital

have all been the victims to a greater or lesser extent of the droughts of the 1970s and 1980s. This article discusses a group of agropastoral Fulbe of the Jallube lineage who are part of a historically formed social hierarchy, the Hayre (region of Mopti, *cercle* Douentza, see map) in Central Mali.

The dry period of 1984-1985 was still remembered very vividly by the Fulbe, and especially by the Jallube, in the Hayre. For them this drought did not end. There had been hardly any good years after the drought. Only 1988 was a good year. Consequently people had no reserves, and every year the period before the rainy season was difficult to come through. The pastures were of inferior quality for most of these years although this varied substantially. However, most herders have not been able to rebuild their herds after the enormous losses of 1984-1985.[3] Among the Fulbe in this area this meant that they still considered themselves as paupers in comparison with the past.[4] Their main asset, cattle, that form their material, social and symbolic wealth in many respects, perished. This fact was hard to accept for many of them. It was the most difficult for the group of pastoralists, who depend most on cattle.

Fulbe society in this area is hierarchically organised. The social hierarchy consists of a political and Islamic elite, pastoralists, who form the nobility; castes, merchants, who are the non-nobles; and the former slaves, who are still defined as the opposite of the nobles (and today also of the non-nobles) because of their unfree status. Related to this opposition is an ideological complex in which rules for behaviour, gender relations, etc., are differently formulated for the free and the unfree. It is important here to mention that the poor of Fulbe society are compared to the former slaves. They have the same status if they do not compensate their poverty (which means having no cattle or wealth, and being unable to do noble work) by being very pious Muslims.[5] Gender categories are very strictly defined. In the elite groups they are guided by Islamic norms and rules, in the pastoral groups by a pastoral code (which includes separation of men and women in the public sphere, special tasks for women and men, but not inequality, cf. De Bruijn 1996a). Next to the social hierarchy groups have been formed and defined through kin affiliation. Patrilineages are dominant in the idiom on social organisation. However, matrilineal affiliation is very important in daily practices. All these rules are in fact ways to define one's identity vis-à-vis each other within Fulbe society at large and vis-à-vis other ethnic groups, i.e. including or excluding people.

After the drought inequality between the rich and the poor increased (compare with Baxter and Hogg 1990). For a pastoralist being poor means having no cattle, or only a few head of cattle, and in some cases a few goats or sheep, but there are also many poor families who are totally dependent on cultivation alone. The problem for most poor today is that their chance of improving their situation in the future is small. Probably only some of the people who migrate to the South will be so lucky, but other reports indicate that this is not the case (Shanmugaratnam et al.

1992). The poor have entered a vicious circle. Their strategies to survive are diverse. They enter manual labour that is looked down upon by noble pastoralists. They may also settle near urban centres in the region, or near cultivators' villages, temporary or permanent. There they will beg or do some small jobs to earn a living. Young men often herd cattle for the cultivators. All this work is considered humiliating; it degrades the pastoralists to the status of slaves. This ideological constraint emphasises a division within the pastoral group between the poor and the rich. It is illustrative that the pastoralists have no extensive social security mechanisms to look after the poor (see De Bruijn 1994, 1996b, Van Dijk 1994).

For the other social groups this division within the group is less extreme. For the slaves being poor means that one has to work harder, but not that one loses one's identity or social esteem. For the elites, who are sedentary, poverty is humiliating, but they can rely on their former slaves. Or they cultivate themselves, which is more common since independence, when their power eroded.

In this article I concentrate on my experiences with the pastoral groups. It is among them that I was most confronted with poverty and all its consequences for the mental and social well-being of people. The settlement which is central in this discussion consisted of eight cattle camps, that were mainly inhabited during the rainy season by semi-nomadic herders, and one hamlet of former slaves who were sedentary farmers. In total it was about 100 families. The herders were from different lineages. One lineage was not dependent on the chief of Booni - these were the Seedoobe; the other lineage was in fact a conglomeration of several small lineage groups, who called themselves Jungo Nyiiwa.

It may not be correct to speak of social breakdown, but among the pastoralists there was at least a feeling of confusion. Of course it is not clear whether this situation was only a consequence of the droughts and the periods of starvation that followed, or whether it is normal for pastoral society, i.e. part of its flexibility. The truth will be somewhere in between. It was clear that for the pastoralists the social hierarchy no longer had a very positive function. Protection of their territory was no longer necessary, nor would they participate in razzias and wars against other groups (i.e. the Tuareg). The elite's position in between the modern administration and their people did not favour the pastoralists (see Van Dijk and De Bruijn 1995). The social hierarchy was mainly used as a justification of the opposition between the free and the unfree, which was still very useful also for the pastoralists to define division of labour, labour relations etc. Nevertheless, this situation was also a bit confusing: who was responsible for what?

The lineage organisation was even less present. The old lineage head of one of the important lineages in the area was a senile old man of over 90 years old. The only practical function of the lineage was its organisational structure to collect taxes. In daily practice people relied more on their neighbours, and on the household (a mother with her children). The definition of kin relations was important in

relation to marriage contracts. The first marriage partners were preferably sought in closely related kin (children of father's or mother's siblings). However, these marriages did not last long and almost everybody ended up being married to so-called strangers.

Within all these organisational structures no clear formats existed for social security or help relations. The small gifts the rich gave the poor were no more than symbolic gifts to prove their noble status, or to adhere to Islamic rules. The poor were dependent on their own efforts and could not easily depend on the others. Help for older people was organised along kinship lines, but as most kin were poor this was not real help. Help between neighbours did exist, but again quantities were small.

The very low grade of organisation can be explained from the lifestyle of pastoralists who have to be very flexible. However, we had the impression that the situation among the Fulbe in the Hayre was also a bit extreme and confusing for them as well, and was explained as being part of the impoverishment that they underwent during the last few decades. An example that illustrates this experience is the interview my co-researcher once had with a herder. This herder had been rich in the past, but from his huge herd only nine cows were left. When he tried to remember the genealogy of his cows he got into real problems. He mixed them all up, and eventually admitted that he had forgotten the relations of his animals. He left without saying a word and did not come into our neighbourhood for several days. He did not want to be confronted with the chaos that life had become for him.

In this paper I will not go into the causes and consequences of this situation but I will focus on the process of doing anthropological research in such circumstances, and on the problem of translation of research data into ethnography. As a researcher educated within (a) paradigm(s) in which the leading ideas are structure and not chaos, whose goal is to order and label cultures and social organisations, it was hard to get a grip on this situation. It appeared that our techniques to do research failed to give the right clues.

Aid

In situations of scarcity a researcher is confronted with an important dilemma: should one intervene, or leave the situation as it is?[6] We decided that intervention was the only way to handle it and the only way for us to survive. Of course we had hardly any means to organize this: a four-wheel-drive car, some money and ourselves We decided to organise grain transport to the village where we did research most frequently. The grain was sold at a very low price. We started to do so at the beginning of our field research. This action revealed some inherent

conflict almost immediately.

An exercise in food aid and trade

On one of our first visits to Serma we were asked to bring food. Having no grain was the most urgent problem, as the inhabitants of Serma made clear to us. The problem was not paying for it (at least not for the people we spoke to then) but getting it there. As we were quite shocked by the situation of the people in Serma, we were glad to do something. Our car proved very useful, because we could use it for the transport of sacks of millet.

By the end of July 1990 we brought the first sack of millet with us. One hundred kilos of millet, our luggage and a lot of passengers in the car seemed to be the load our car could carry. The first reaction of Hadjata (the woman who built our hut) was: 'only one sac, why didn't you bring two sacks, there are many people who want to buy!' We decided to leave half a sack with Hadjata, so that her people could eat. She promised to sell an animal and pay us afterwards. We sold the other 50 kilos in the core hamlet. This only served a few people and many people were too late. We got the impression that only rich people bought grain, and besides only people who were friends or kin of Hadjata and Abdramaane (our host). People were wondering why we gave 50 kilos to Hadjata: was this justice? We did not know how to handle it, so we had to rely on the ideas of the people we knew best by that time, like Hadjata and Abdramaane.

The next time we brought millet the chaos during the sale was enormous. Everybody knew now we brought one sack of millet and that this could not serve the whole village. We wanted to sell it to the most needy people, but as we did not know the people yet, we asked Hadjata and an old man to decide who could buy first and how much millet they could buy. The place where we were selling was always enormously crowded, people were really fighting to get something. Arguing about the sale of millet was impossible. We lost control of the situation and did not know who eventually bought some. It was clear, looking at the disappointed people afterwards, that the division was never satisfactory for everybody. Our purpose was to help the people who could not go themselves to town to buy grain and to transport it to their homes. These were the old women, the poor families, and the disabled. We recognized these people were not the majority of our buyers. But how could we organize the sale so that they would get their share? And how could we know who the really poor were? We thought that the people themselves would understand this and would make place for these people, but this was not the way people in Serma reasoned, as we discovered. The people we asked to be middle-men, a few influential people like Abdramaane, the Imam, of course, took their share (and a big one). Later on we heard gossiping that Riimaybe women bought

cheap millet from us and then prepared *lacciri* (couscous), which they sold for a profit to the people of Serma and to the herdsmen from the inner delta who roamed the region. This had never been our intention. It seemed impossible to ask them to be reasonable and to organize the sales in a correct way: i.e. if people did not buy this time, then let them buy the next time, and only buy for their own consumption. The person who could arrange this for us was not available, and we got the impression (which was shocking but true) that everyone was fighting for him or herself and not really considering the others. We were angry and thought about stopping this action. Especially at the end of August 1991 the food situation was such that we could not refuse to bring millet. We even decided to bring two sacks, which finished off the suspension in our car, and we had to stop in the most northerly camp to unload the car and bring the sacks further on the back of a donkey. First we did not bother about it. When Abdramaane, who we thought was a respectful and influential person and who was rich enough to care for himself, claimed one sack for his family, we got very angry. How could he do this? He had the means to go to Boni and to buy a sack of millet himself, while some of his people were really hungry and waiting for the harvest next month. It was a problem for us, as Abdramaane was our host and if no longer allowed us to stay in Serma we would have to go home. We discussed the problem with some other important figures in Serma. One of them told us to leave the sack with Abdramaane. We are sure now that he would have had his part. The other (the Imam) told us not to do this but to sell it all to the people. We followed the advice of the latter. Thereupon we decided to follow our own feelings of justice and not give the sack of millet to Abdramaane. We tried again to sell the millet in an orderly fashion, which proved impossible.

These events are the core of our introduction to pastoral Fulbe society in the Hayre. They made us aware of many difficulties we would meet in the months to come and of the differences between their and our world. Furthermore, they were a first step towards understanding society. Through these actions we gained insight into poverty, into relations between the different Fulbe lineages in the village, into the silent starvation of the people, into the relations between the camps. We also gained insight into the importance of grain and of independence, and into gender relations. This experience revealed the differences between the former slave groups and the herders. It appeared to be a very important tool for our research.

After this experience we had better insight into poverty and into the power relations in the village. It also gave us a position in society. We had become part of a social network of mainly the Seedoobe. They did their best to claim us in the beginning. In this effort I also established special relations with some old women, who became my grandmothers and with whom I had a gift relationship as a good granddaughter is supposed to have.

This meant that we/I were included in some social networks in the village. In

those networks we were the people who provided the others with food and help where necessary. In this way we relieved a few richer relatives of those people from their obligation to help them. In fact we played a very useful role in the village.[7] For us it was good to be embedded in this way in the village because it gave us a *raison d'être*.

Interviewing the poor

The open interview is probably the most important and most frequently used methodological tool among researchers. Participant observation is also translated into interviewing in many cases. In this sense our way of working is very verbal, which is a reflection of our own culture, especially the academic culture, in which speech is one of the most important means of communication. However, such a bias in research runs the risk of avoiding situations in which silence is central or in which communication through speech is not possible.

I also went to the field with the idea that I would interview many people. In the first instance I did so. I consulted almost every day the learned men from the village who explained all the genealogies of the inhabitants of the village to me. Reams of paper with family diagrams are the result. These learned men did not represent the majority of the people. I also tried to interview other people. I had the idea of collecting life histories, stories about conflict, and simply interviewing people about the many things an anthropologist is curious about. So I went together with my assistant to the people who were by then already part of my social network. I directed my part of the research more and more to old people, young women and the poor. When we entered the hut and started questioning them about the past, about life in the past, we did not dare to talk about the future. Some women simply refused to talk. Others started crying. Remembering their beautiful past (probably a little exaggerated in their minds) was too difficult for them, let alone thinking about the future. In the first instance my reaction after a few such experiences was to stop the research. It was too difficult. Finally I accepted not to interview, but to talk when it was time for talking. And to be silent when it was time to be silent. It proved to be very useful to listen to the silence, and I realised that this was probably the core expression of being poor. There are no words to express one's feelings in a situation of endless difficulties. 'There is no meaningful way to articulate the continuous experience of starvation' (Hastrup 1995: 119).

Strangely enough, I fell back on the use of questionnaires, a form of research that would never have come up in my mind before starting the research. Nevertheless a questionnaire, i.e. a form with prefabricated questions, was a good tool and very helpful to continue research. It gave me a feeling of doing something constructive It yielded information on which one chapter of my dissertation is based.

Illness

How are you to cope with your own illness and that of the others in the field? It is strange to say and difficult to admit but illness became an 'eye opener'. I am aware of the trickiness of this statement, because one should not use the misery of others to become better oneself. In this case this was unavoidable to a certain extent. It became my goal to tell the story of the poor, of the hidden hunger, and of the silent starvation of those people, which cannot be separated from illness, because illness is an expression of poverty and starvation (Shipton 1990).

How much illness was part of the life and of the experience of life of the Fulbe in the Hayre may be illustrated by my own experience. So I start here with my own illnesses. My acceptance in society was partly defined by illness. In the last month of our stay I ended my medical history in Serma with conjunctivitis (inflammation of the eye), which is very common among the Fulbe in the dry season, after I had suffered from *henndu* (a spirit-caused illness), backache and headache. This conjunctivitis, as I was told, made it clear that I had become an integral part of the community, as I had undergone the same illnesses as all Fulbe and had become skinny.[8] Of course I never intended becoming ill to have such a positive influence on the research.

The illness of the Fulbe was always present. They came to us for help, with their children for aspirin and for advice. As we were not doctors we were very reluctant to give too much help and always told them to go to the hospital, which they hardly ever did. They considered the hospital as the outside world, a world that is per definition hostile to them. I ended up taking many women to hospital, with different results. Some got better, others died afterwards. Each time this going to hospital and trying to get through the medical bureaucracy with a *broussard*[9] meant an enormous frustration and disappointment with medical help in Africa.

In the last months of our stay in the area I had come to a point that doing research was no longer my prime reason for being there. In fact I lost my real *raison d'etre*. I got more and more involved in the misery of the people. This was of course a logical consequence of doing research and knowing the people better. When our sister fell seriously ill we took her to hospital in Douentza. This meant that we would be occupied with her illness and her recovery for the next month. Research was less important then. However, it turned out the opposite way. Being with Aisata in Douentza and negotiating with her family and her husband's family about her treatment; listening to her own explanations of illness; and hearing all the stories of the doctors; being in hospital and discovering the social networks of Aisata in town offered an enormous insight into social life, ideas about cosmology, religious ideas and practices and what it meant to be a Pullo (pl. Fulbe). Language was no longer a real barrier by that time, which enabled me to discuss freely with Aisata and with other women who decided to come to the hospital with us after

Aisata. I am still very grateful for the insights these women have given me into their society and culture. And the only thing I hope is that they still recognise our efforts to find a remedy for their illness as worthwhile, although some of these women have died. In our opinion, the treatment came too late for them; for the Fulbe their time had come and despite all our efforts and despite the knowledge of doctors, healers and Islamic specialists, they would have died anyway.

Death

This brings me to the last impressive experience I want to discuss here; a very emotional experience and one almost impossible to express - the death of a few good friends, mostly young women, still at the age for bearing children, which is indeed a time full of risks. Bearing children is feared as a mortal disease by the Fulbe (Riesman 1992).

The fact that it the first time I have seen people die makes it a very difficult and emotional experience. This made me realise the difference between them and us. This feeling was further emphasised by the way the Fulbe mourn: no public signs at all. The expression of emotions is almost forbidden by the Fulbe moral code, and they adhere to this rule for the loss of people too. The burial ceremony is very simple, and executed according to the Islamic rites (there are almost none). After the burial (just after death) people may come along to bring their condolences. This is all in silence: people do not cry, they do not express anything. When young children die the mother hurries to continue her normal life. Why bother about such a young child that did not yet have use value?

I have long wondered how to interpret this expression of loss, or rather this absence of expression of loss. Was it my own frustration that made me explain it very negatively in the beginning? Explanations of social character did not really help me to accept this lack of public emotions. A few incidents after the death of people made me realise that lack of public emotion is not the same as lack of emotion.

The difference between the death of a Tuareg woman in the village (a good friend whom we took to the hospital) and the death of a daughter was enormous. For the Tuareg woman no mourning at all was seen, not even condolences were made. This woman had no family in the village, being considered by most as a stranger, so why bother about her death? Some more expression was given to the death of a woman of 21 who had just given birth to her third daughter and who was Pullo. She was an insider. Many people went to pay their condolences to the family. Is this the expression of emotion? To me it seemed very hard to exclude a woman from this honour because she is an outsider, although she had lived for many years in the village.

When I visited a woman and was just chatting a little bit she asked me why I did not see anything. She seemed a bit angry. I had heard nothing about any problem in her family, nor about herself. She told me that her youngest child had died. Her grief was enormous, and probably she only made me aware of it, other people reacting to it in silence.

This incident made me reflect on my observation of the mourning. What did I expect? Did people not say much more without using words? Why did I expect some kind of extended ceremony? This was of course what I had learned from studying anthropology. However, silence may say more, both for them and for the listener, which I was not until I returned to the Netherlands. As Rosaldo (1987) remarked, the study of death is formalised by most researchers as touchable things, as understandable objects, i.e. funerals or mourning rites. The emotional side of death is almost never part of the description.

Discussion

The methodological tools anthropologists use in the field are still part of a positivist image of the world. Societies and people can be described as objects of knowledge. However, as I have tried to show in this article, this approach will not help us to really understand these societies and people. The problem is not that in the process of doing fieldwork we are not confronted with complex realities and diversity, but in the act of translating it into ethnography we often leave these elements out because they are difficult to translate.

I do not want to argue for a refinement of our instruments. That will not avoid the problem of translation. But I am arguing for a change in attitude of anthropology and anthropologists. The standpoint that a society as such can be described should be abandoned. Anthropologists like Gibbal, Stoller, Rosaldo and Riesman have tried to do so. They have written their ethnographies as dialogues and as real experiences of them and of the people with whom they lived. Their books do not present *the* society, or *the* kinship system, but a dialogue, a complex reality that cannot be simply understood. In anthropology more room must be created for moments in fieldwork that do not seem to convey information: moments of silence, of emotion, of interaction, of worrying. Furthermore, as Gibbal noted, we must not be afraid to present chaos and events we do not understand. Not everything is understandable.

Our methodology of research is very verbal and visual. But do we listen to silence, do we really interact? Why do we reduce complex emotions like those experienced with the death of our dearest to something abstract like a funeral, or a mourning ritual, as if we could get a grip on it?

In the translation of our data and of our experience into ethnography we should

not be afraid to step aside, to leave scientific rules and norms for what they are and to describe what we really saw, felt and experienced. I realise that this advice may confuse many anthropologists. To end with Palsson:

> (...) A more appropriate image of contemporary anthropology would be that of a former convict scratching his or her head in the open air, liberated from the Platonian cave, puzzled by the ruins of the prison house - its perceptual illusions, its strict codes of conduct, and its bizarre architectural design. Not only must such ex-prisoners wonder, in Kafkaesque fashion, why they were locked up in the first place and how they eventually got out, but more importantly, how they could possibly enjoy the new freedom in the apparent absence of any kind of idealist agenda but faced with unavoidable materialist constraints and an ecological crisis (Palsson 1996: 78-79).

Notes

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2. This is not to say that this situation was new for these people. Droughts and other calamities are recurrent phenomena in the Sahara. It is insecurity that guides their lives. The reality of daily life is very complex and cannot be ordered easily. This has always been the case and is not per se only a consequence of globalisation, as is often stated, and thus a problem of recent times. It is not only now that our instruments to do research do not give enough clues to understand reality, but this seems to me to be a general problem of research and a problem of all times. We have only presumed to understand the complex and chaotic realities by using our standard terminology and analytical frameworks (cf. De Ruijter 1995).
3. In the region 62% of all cattle and 55% of small ruminants perished in the drought of the 1970s (Diallo 1977). During the drought of 1983-1985 approximately 75% of all cattle perished.
4. Other groups in the region are Dogon, Hummbeebe, Sonrai, and a few Bambara.
5. For further elaboration of this subject see De Bruijn 1994, 1996b.
6. Other anthropologists have written on such experiences (cf. Spittler 1992), but their number is limited. This field is reserved for aid agencies and development workers.
7. We were considered more useful than the organized development activities in the area.
8. I was not so very ill. They were minor things but the people (mainly women) identified it as part of the process of becoming a Pullo.
9. *Broussard* is the term used by the nurses themselves when they talk about these people with us. Literally it means people from the bush. This makes them the opposite of the people from town, the civilized world, the knowledgeable people, the educated people, stressing the backwardness of rural people.

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