

DOING FIELDWORK AT HOME: SOME PERSONAL EXPERIENCES AMONG THE TUMBUKA OF NORTHERN MALAWI

ALISTER MUNTHALI

Department of Anthropology

Rhodes University

South Africa

email: alistermunthali@hotmail.com

ABSTRACT

The bulk of anthropological theory grew out of western anthropologists studying "exotic" cultures. The end of colonialism, the reduction of funding for academic institutions, the increase in student enrolment and difficulties in accessing the field are some of the factors that contributed to the practice of anthropology at home in the west by western anthropologists. However, most anthropologists from Third World countries have in most cases conducted fieldwork in their own countries and among their own people during training and professional work. In this paper I examine the problems of working at home, where being a native, studying fellow natives, I was branded as a foolish person asking silly questions because I was expected to know the answers. My extended stay at home was interpreted differently by my own people and different identities were given to me: a member of the CID, a physician, a person who had been sacked from his place of work (and who hence had nowhere to go apart from home) and someone who was after "their" women. Since my home village is only 15 km away from the research site, my relatives did not understand why I had to stay in my research site, claiming that it must be that I did not like my own village. Before I began fieldwork, the idea that while I would be trying to study the behaviour of my fellows, they would at the same time be trying to understand me never occurred to me. The major conclusion in this paper is that though I was at home doing research, I was in essence not really at home because my long absence from home and the choice of my research topic had somehow de-familiarised me from what was supposed to be familiar.

Introduction: The Anthropology at Home Debate

Since the foundation of anthropology as a discipline in 1884 (Jackson 1987:2), the field has been dominated by Euro-American anthropologists. The development of anthropology was been closely linked with colonialism and the expansion of the western influence when the anthropologist provided information on how the west could manipulate and control the non-western world (Lewis 1973:582). In this context, therefore, the field of anthropology was characterised by fieldwork carried out in far away countries, in cultures which were radically different from those of the anthropologist. At the time, it seemed natural that budding anthropologists should follow the footsteps of their fore-bearers (Mewett 1989:73-90, also see Messerschmidt 1981:197-198) by doing fieldwork in far away exotic cultures.

Conducting fieldwork has become a *rite de passage* by which the student of anthropology becomes a professional anthropologist (Razavi 1992:160 & Hayano 1979:99-104). Even though the interest in carrying out anthropological work at home started after the Second World War, the practice of anthropology at home really developed in the 1960s (van Ginkei 1998:261-267). While anthropologists from the western world have indeed carried out the bulk of their anthropological research in exotic cultures, anthropology at home now calls for these western anthropologists to do research in their countries after a Century of studying the exotic. A number of reasons have been advanced for this homecoming of anthropology (which some authors have dubbed the partial repatriation of anthropology; see van Ginkel 1998). Previously Europe and other western countries were viewed as domains of sociology and not anthropology and Jackson contends that the failure of sociologists to explain their own societies gave rise to the hope that anthropologists might do a better job (1987:7).

Many countries which previously acted as fieldwork sites for western anthropologists have put severe restrictions on foreign researchers (Fahim and Hermer 1980:644-650, Messerschmidt 1981, Mewett 1989 & Jackson 1987:8). Possibly because of its association with colonialism, anthropologists are sometimes banned by governments or rejected by the intellectuals of the host country (Lewis 1973:581-602 & Levi-Strauss 1966:125). Other anthropologists suggest that the exclusion of foreign anthropologists is a result because of the fear that they would reveal the new forms of discrimination and the widening gulf

between the rulers and the ruled (Brokensha 1973:592-3). Though it is still possible to gain entry to research sites, Mewett says that the procedures have been made more complicated and tiresome (1989:73-90). Razavi adds that in post revolutionary Iran, it is virtually impossible for non-nationals to carry out village level fieldwork, where even local researchers are subject to suspicion especially if attached to western institutions (Razavi 1992:152-163; see also Goward 1984). There are some groups also that have chosen not to be studied by western social scientists, preferring to be studied by one of their own (Cassel 1977:412-416). As regards employment, Third World countries prefer the employment of native western trained anthropologists and these native Sociologists are encouraged to conduct development oriented anthropological research (van Ginkel 1998).

In addition to the restrictions noted above, I would also add that political uncertainty and civil wars, common in Third World countries, discourage western researchers from working in such environments. At a time when countries such as the Democratic Republic of the Congo, the Sudan, Liberia, Burundi, Rwanda and other nation states in Africa are rife with civil wars and political uncertainties, western anthropologists would not want to conduct fieldwork there for extended periods. Hence to avoid the bureaucracy common in the Third World countries as well as political uncertainties and civil wars, western anthropologist would rather do fieldwork in their countries where there is relative freedom (van Ginkel 1998) and easy of access to one's own society (Jackson 1987:8). The disappearance of exotic cultures and the end of colonialism (and the concomitant association of the profession with colonialism) are some of the reasons why some anthropologists predicted the demise of the field of anthropology in the 1960s (see Lewis 1973:588). The practice of anthropology at home can be seen as a means to revitalise the profession (Messerschmidt 1981).

The decrease in funding for anthropological research (Messerschmidt 1981:196-7, van Ginkel 1998, Hayano 1979:99 Jackson 1987:8 & Okely 1987:56), tight academic budgets at universities and other institutions of higher learning (Fahim & Hermer 1980), the rise in student enrolment (van Ginkel 1998 & Jackson 1987:8), the shortage of jobs in the academic field (Messerschmidt 1981 & Fahim and Hermer 1981), the easy access to one's own society (van Ginkel 1998:251-267), disappearance of exotic cultures (Hadolt 1998:314) and the realisation that anthropology is the study of all humankind and not just of exotic peoples (Jackson 1987) are some of the factors that have

forced anthropologists in the west to start conducting research in their own countries. Messerschmidt argues that the shortage of jobs in the academic world led to the development of applied anthropology which in most cases is equated to doing anthropology at home (ibid, also see van Ginkel 1998:254). In addition to the scarcity of jobs, Conducting anthropological research at home has also been enhanced by the fact that western governments and NGOs have also started financing applied and policy research into areas such as ethnic minorities, marginal groupings, crime etc (van Ginkel 1998:251-267). This change in policy has resulted in many western anthropologists staying at home. It was also realised that western anthropologists did not know much about their own culture (Jackson 1987:8) and when they started doing research in their own backyards they discovered that within their own culture they were also able to identify the primitive within cultures in them (see Lofgren 1987:74 & Hadolt 1987). Anthropologists in western countries (for example Sweden) which did not have any colonies also conducted their studies in rural areas and marginalized communities (see Lofgren 1987:74). The ability to find the exotic and the unfamiliar at home points to the heterogeneity of societies which can be exploited by the anthropologists.

For the western anthropologists, studying exotic cultures is a way of examining themselves and their society (Diamond 1964:432), recognising their own peculiarities (van Ginkel 1998:251 & Lewis 1973) a voyage of self-discovery (Jackson 1987:10). These factors have, however, forced western anthropologists to know themselves and their cultures conducting anthropological research at home, which, as far as the west is concerned is a relatively new practice. Fieldwork at home, especially for the western anthropologist, is a departure from the old tradition that entry into the profession required field experience in another culture, preferably one quite foreign to one's own (Messerschmidt 1981).

Conducting Anthropological Research at Home in Africa

The study of primitive society which formed the basis for the development of the field of anthropology, was a means to fill the gaps in the westener's knowledge about himself (see Lewis 1973:582 & van Ginkel 1998). This, inevitably, led to the assumption that the study of the non-western world could only be done by a westerner or an outsider (Lewis 1973:581-602). Levi-Strauss has argued that allowing

natives to study themselves is *not anthropology* but *history or philology* because anthropology is the science of culture as seen from outside (1966:126; Lewis 1973:581-602 & also see Diamond 1964), the observation of people of one kind by people of another (Hughes 1974 in Cassel 1977). In his work, *Ethnographic Atlas*, Murdock states that Europe is not fully represented in his sample because it is the domain of the sociologist and not the anthropologist. According to Murdock, anthropologists were not supposed to do fieldwork in Europe; they had to do it elsewhere (Chilungu 1976:457-481). Because of the history of the development of anthropology and its emphasis on the study of the other peoples some African anthropologists like Chilungu were at one time advised by their advisors to major in sociology because anthropology was for outsiders and this meant, as an African, he could not study Africans (Chilungu 1976:457-481; also see Hayano 1979:101). Jackson says that Third World anthropologists have referred to themselves as sociologists until the recent collapse of the credibility of sociology (1987:7). This, I presume, was largely due to the association of the discipline to colonialism.

One thing that needs mention, however, is that while western anthropologists have been busy doing research in Third World countries, anthropologists from the Third World countries have all along been doing research in their own societies and in some cases in their own culture. One classical example is Jomo Kenyatta, the late former president of Kenya, himself a Kikuyu who did research among his own people which culminated in the publication of his thesis *Facing Mount Kenya* (Kenyatta 1938). Despite the criticism that it got from Leakey at a Malinowski seminar in London (Wax 1976:331-333, Murray-Brown 1972: 192 & Hayano 1979), Malinowski¹ himself says that *Facing Mount Kenya* was one of the first competent and instructive contributions to African anthropology by a scholar of purely African origin. Malinowski was one of those who advocated that anthropology begin at home ... we must start by knowing ourselves first, and only then proceed to the more exotic primitive societies (Malinowski 1938:xii). It, however, took time before the western anthropologists started studying western societies.

A number of anthropologists have pointed out the to difference in emphasis in the training of western and Third World anthropologists in western universities. African students in western universities, in most

¹Malinowski wrote the forward in Kenyatta's book *Facing Mount Kenya*.

cases, leave for their respective countries to do fieldwork there while, at the same time, students from western countries are encouraged to do fieldwork in cultures other than their own (see Jones 1970:251-259 & Fahim 1977:85).

Conducting research at home in Africa is not a new phenomenon. This has been happening for decades. African anthropologists as well as other Third World anthropologists doing research in their own cultures have been criticised mainly because of the lack of objectivity in their works. As insiders, they cannot critically study their societies (see Hayano 1979 for Leakey's criticism of Kenyatta's work). Finally, however, since Euro-American anthropologists have started doing research in their own backyards for the reasons stated above, the time has come to discuss the problems of doing anthropological studies at home (Jackson 1987:10). Should we say that the western anthropologists, by doing anthropology in the west, are *not doing anthropology* but *history* or *philology* (Levi Strauss 1966)? Or this is only applicable to African and other Third World anthropologists? Are they more objective (objectivity being the ideal approach in classical anthropology) than their Third World counterparts doing research in their own culture? Sarsby answers these questions succinctly, saying that "the fact that anthropologists have sought the unfamiliar at home as well as abroad has meant that many of their problems are the same as those of anthropologists doing fieldwork in their societies" (1984:131). It can be presumed that this includes the question of objectivity. The irony, however, is that African anthropologists (unlike their counterparts in the western world) often feel it necessary to justify whenever they write about their own society (Malinowski 1938:xii)

While many African anthropologists have carried out research in their own countries and in their own cultures, what is lacking in their ethnographic works is the description of their experiences when they were doing fieldwork. In most cases, as Hayano (1979:101) points out, "auto-ethnographers simply go about ethnography as usual in their roles as objective reporters and analyse their data according to their own special interests and theoretical inclinations. They do not indicate the problems and potentials of doing anthropology as native insiders" (also see Fahim & Hermer, 1980). This paper, however, attempts, describes and analyses some of the experiences that I went through when doing initial fieldwork among the Tumbuka of northern Malawi. Crick says that it is good to talk about fieldwork since one can better understand a text if one knows something about the writer, the

experiences upon which the text is based and circumstances of its production (1989:29). The sharing of fieldwork experiences also enables comparison and the construction of appropriate generalisations on the conduct of fieldwork at home.

Some Personal Experiences of Doing Anthropology at Home

In deciding to carry out my research at home I was doing what was expected of African anthropologists. The decision to study my own people was arrived at partly because I discovered that I lacked knowledge of my own culture and that there was a shortage of ethnographic literature on the Tumbuka of northern Malawi. One of the challenges of African anthropologists doing research in their own backyards is therefore to describe their experiences on the field and thus contribute in the debate on the need to conduct anthropological work at home. In this paper, therefore, I depart from the African tradition of anthropology at home by essaying a reflexive approach based on initial fieldwork experienced among the Tumbuka of northern Malawi. While we spend a lot of time trying to understand the behaviour of our "objects", we should also know that our "objects" also try as much as possible to understand our behaviour. In this paper I therefore contribute to a reflexive anthropology by presenting a few anecdotes which illustrate how my fellow natives interpreted my stay among them.

A Search for Accommodation: Relationships, Connections and the Role of the RA

When I went into the field in early June 2000, I recruited a graduate research assistant (RA) to assist me in administering the household questionnaire. His home village was very close to where I was going to do the research. The plan was that we should spend the first night at the home of the RA and proceed to the research site the following day to look for accommodation. The problem is that this was a rural area and there were no rest houses and restaurants where one could live and eat. At the Rumphu District headquarters, we learnt that the RA's uncle was teaching at the Bembe primary school located in the research site. When we arrived at the school the RA's uncle welcomed us into his house. The following day we went to seek permission from the village head. At lunchtime we went back to our host's house and as we were chatting I found out that my uncle's wife was the younger sister of our host's wife and that his late great grandmother was the

mother of my grandfather (my mother's father). After this exchange, he informed me that I should feel at home.

The strange thing, however, was that though I had known the RA for many years we had never realised that we were so closely related. This knowledge came at an opportune moment because without him I would have taken a long time to establish myself in the field. It also occurred to me that some of the people in the village were my distant relatives. These relationships and connections were an advantage for me. It has to be mentioned that there have been a lot of intermarriages among the Tumbuka people of Rumphu and if you look at genealogies you would find that somehow you are related to most people.

Even after my RA left my hosts assured me that I could stay with them for as long as I wanted since I was related to them. When I met my mother some time later, she also confirmed the relationship.

Employment of a Female Assistant: A Confirmation that I was after Women?

With the assistance of my hosts, I managed to get a young lady who had just finished her "O" Levels. One day, on coming back from work I was told that she was sick and was sleeping at her relative's house nearby. Because of her poor condition, I told her to rest and come back to work after she had recovered. When she fell ill she complained to my male research assistant that she had not been sick for a long time and was wondering why she fell sick soon after starting working with me. All this while, I did not have any idea of what the people in my research site were thinking about the young lady and I until one day a young man told me that people were wondering why I was interviewing mostly women. He went on to say that people lived in fear because they thought that I was going to snatch *their wives*. To my respondents the employment of the young lady as a research assistant confirmed that I was indeed *after women*. I assured them that I was in the area for the purpose of research and that I was already married with one son. This was why, in December 2000 when I went back to do more research, my wife and son visited the research area². Though she was in the research area only for 7 hours, my wife's presence assisted in clearing people's fears about me and *their women* (cf Razavi 1992: 162).

²Yengoyan in fact avoided hiring a female assistant or seeking more intimate female companionship for fear of jealousy (Ardener 1984:124)

Poor Relations with my Relatives as a Reason for not Staying in my Home Village

In June and July 2000, I visited my home village a few times. During these visits, I was being asked by people (from my own and from neighbouring villages) why I had decided to live my research site at Chisinde and not in my own village. I explained to them that I was doing «research» in that area which required me to live in that village and not in my home village. No one was satisfied with my explanation. They argued that it was possible for me to stay at home and leave every morning for Chisinde, do my work and then return home in the evening. They concluded that perhaps I did not like staying in my own village and that my relationship with my relatives in my home village were poor. Some people even told me that I did not want to stay in my village because both my father and mother did not live there. My father lives in Blantyre where he works while my mother lives in her home village. My aunts and cousins are the only ones who live at home. While carrying out research in their culture, some native anthropologists have avoided staying away from their home villages for fear that this would give room for potentially damaging rumours about relationship with their parents or relatives (see Nakhleh 1979:344). My decision to stay away from my village did not make any sense to the people there. I had similar problems trying to explain my decision to stay in my research site and not in my home village which was a few kilometres away to my graduate friends who work in Lilongwe, the capital city of Malawi. They too could not understand why I decided to stay in the area of Chisinde and not in my own village.

Asking Silly Questions?

... the local anthropologist may not be taken seriously by informants if he probes types of behaviour that informants view as commonly shared knowledge, such as marriage customs, or he may be considered intolerably crude in broaching other topics, such as sexual practices. Recognised as a member of the society within which he conducts research, he is subject to the cultural expectation of his informants... (Fahim & Helmer 1980)

Doing research in one's own home is also problematic because your informants think that the researcher already knows the answers to the questions. My two research assistants assisted me in the

administration of the household questionnaires. After we finished the administration of these questionnaires I informed them that they were through with their work. The remaining work, which involved in-depth interviews and key informant interviews, could not be done by them since they were not trained to handle that. The first research component was aimed at determining the socio-economic and demographic characteristics of the villages under study. This component was easy but the difficult part came when I started asking ethnographic questions. What some people then started to say was that since I was a Munthali from a village³ only a few kilometres away from my research site, I was expected to know the answers to the questions I was asking them. I was asking questions for which I already had the answers. Since I was married and had a child I was expected to know the rituals surrounding childbirth and other similar events. I explained to them that, since I had spent most of my formative years in town, I did not know those things. After this, my informants became very open and started telling me whatever I wanted to know. In addition to this, I also told them that while I might have had some slight knowledge of *our* cultural practices and beliefs, I wanted to document them so that, in future, our grandchildren would be able to read about the Tumbuka culture. This aroused a lot of interest in the elderly people as they felt that the youth were no longer doing things the right way. Young men and women were not adhering to the Tumbuka ways of living and their lifestyles had been highly "Europeanised".

Since it was expected that I should know the customs of my own people, I was considered as someone who asks stupid/silly questions (*mafumbo yauchindere*) by a number of people. This was because I was asking questions that they never expected would come from me. This is one of the challenges that anthropologists have to face when doing anthropology at home⁴. Despite the fact that my questions were considered silly, they were nevertheless answered. The situation would, of course, have been different an "outsider" had asked the questions. As van Ginkel puts it: "they had excused for their ignorance because they are outsiders (1998:256). A lot of people practising anthropology at home have experienced this. Jones, a black

³See also Nakhleh 1979:346 who did his research among his fellow Israeli Arabs and was being asked, "You are the son of Rameh, need I tell you?"

⁴Unni was considered a fool because despite being a Nayar he did not know Nayar matriliney (Unni 1979). By describing me as a person who asked me silly questions since I was expected to know the answers, in a way meant that I was also "a fool".

American studying his fellow blacks in Denver, USA sent his students into a black community to enquire about health practices. One student returned with the information that some women had a craving for a particular type of dirt during pregnancy. Although Jones had grown up in that part of the United States, he was not aware of this fact. Moreover, during his studies, nobody had volunteered this information because it had not occurred to them that he would not already be aware of it since he could readily be identified as both black and southern (Jones 1970:251-259). Nakhleh makes a similar point: some of the information he sought was not easily volunteered on the assumption that as a native he was supposed to know (1979:346).

"He has finally come home because he was sacked from his place of work"⁵

While some people from my village could not understand why I did not choose to live in my home village and while others felt that I was asking silly questions, my extended stay at home was also interpreted differently by other people. Although I had originally planned to stay in my field site, in November 2000, I decided to go and stay in my home village and commute to my research site for four days a week. I then planned to spend the other three days writing field notes and interviewing other informants (in neighbouring villages) as well as doing some observation (attending funerals, rituals, ceremonies for installation of chiefs, visiting diviners etc). When going to my field site I used to get lifts to the Bolero trading centre. My niece had a very old bicycle which I also used whenever the need arose. In most cases I walked to the Bolero Trading Centre. When going to funerals or attending ceremonies I would put on shorts, a t-shirt and a pair of slippers. I was not wearing this because I was at home. In town, I also do put on this attire sometimes. On several occasions some people told me what the others were saying about me. I was told that some people said that I was lying when I said I was doing research. According to them, I had been sacked from my job and had nowhere else to go to and had therefore decided to return to my village after nearly two decades. Such claims arose because of my habit of wearing slippers, T-shirts and shorts and using an old bicycle. In the past, the community had been used to seeing me driving a car or being driven in an institutional vehicle. For many people, therefore, my long stay at home

⁵I work for the Centre for Social Research of the University of Malawi as a Research Fellow. As of now I am pursuing postgraduate studies at Rhodes University in South Africa.

and my behaviour suggested that I had lost my job. Others asked where my wife and child were. I thought they were asking in good faith and answered them that both of them were in Blantyre. This was further confirmation that I had indeed been sacked and that my wife had abandoned me.

A Member of the Criminal Investigations Department (CID)?

In early December 2000 I went to conduct interviews in another village, one of the three villages comprising my research area. On my first day in this village I sought permission (which was granted) from the village head but this coincided with the day when a member of the village committed incest. In January 2001, I decided to ride from my home to my research site on a bicycle since I did not have sufficient funds for any other form of transport. On the way I met a man who resides in one of the villages under study who told me that he had met some people from one of the other villages under study who had told him about me and they were wondering whether I was not after something else (possibly to find out more about the incest) and were afraid that I was a member of the CID. He assured them that he knew me very well and had stayed with my grandfather in Chililabombwe, Zambia for a very long time. Though this man did not know me well, the good relations he had with my grandfather made him clear the misconceptions that people in my research site had about me. This was not the first time I had been branded a member of the CID. In my own village, some people were also suspicious about what I was doing and suspected that I was a CID member. They came to this conclusion because of the fact that I was rarely at home and because of the questions I was asking regarding genealogies⁶. When I asked people in my own village about their genealogies others wondered what I was going to do with the data. I told them that I was just interested in knowing where we came from. One morning, around 6:30am, I was awoken by one of the members of my village and when I came out of the hut and greeted him, he told me that he was also interested in learning about "our history" and wanted me to tell him what I had learnt from the village head. This was an old man of about 70 years and I still wonder what his

⁶Initially I was not interested in the genealogies of people from my own village. However, when doing genealogies of one family in my research site I heard that one woman had married someone from my home village. I therefore wanted to know how the man who married this woman was related to people in my own village and how I was related to that family in my research site.

motive was. It was this man who told me that some people in the area thought that I was a member of the CID since I was asking many questions and was never at home, a major characteristic of members of CID. Other researchers doing studies in their countries have also been looked at as members of intelligence agencies. For example, Goward reports that Gupta, an Indian researcher, stopped taking notes at public meetings in Uttar Pradesh after rumours started spreading that she was an intelligence agent (Goward 1984; see also Razavi 1992:154). Because of this suspicion that they are secret agents, many anthropologists have felt like tricksters while conducting fieldwork; for example, "Bohannan's experience among the Tiv of Nigeria where she felt like a trickster as one who seems to be what she is not" etc (Goward 1984)). Although I made my identity very clear, some people still referred to me as a government worker who "has come to know the health problems facing children in our area". These people claimed it was good that the Government had sent a native of the area to do the research. A black American researcher whose informants hinted that it was good for black social scientists to do research amongst black people as research done by whites is often misrepresented). [cf Jones (1970)]

A Physician?

The very nature of my study led some people (especially those I met at NyaNundwe's compound) to think that I was a medical doctor. NyaNundwe⁷ is a very well known female diviner and I was interested in seeing her though I had not visited a diviner for more than 20 years. NyaNundwe lives at Kaduku, Hewe and is very famous. She has been to most parts of Rumphu, Mzimba and has also visited Zambia many times.

When I arrived on that Wednesday morning (7th December 2000), most of the people thought that I was a medical doctor because of the nature of the topic that I brought up. Word went around the compound⁸ that a medical doctor was on the premises of NyaNundwe. Before I met NyaNundwe, a young woman I knew from my home village passed by

⁷Her divining name is Mujovwire which means assist yourself.

⁸NyaNundwe has a very large compound (it can be called a clinic) where people with serious illness are admitted. In my interviews with some of the patients I found out that others have been there for periods ranging from one month to 2 years.

and when I greeted her she did not respond: she just looked at me, waved and then went away. I learnt later that she had “eaten a sacrifice” hence she would not talk to anyone unless that person gave her “*mboni*”. This lady was a sister of a friend I had gone to school with for 18 years (1971-1989). She had been admitted to NyaNundwe's place because she was suffering from *vimbuza*. I then had gone to see NyaNundwe and as I was discussing a few issues with her, the mother of the lady suffering from *vimbuza* came and told me that she had heard from her daughter that I was around and that she just wanted to greet me. She also told me that people in the compound were spreading the rumour that I was a medical doctor. I informed all the women whom I met and interviewed that day that I was not a medical doctor, that I was just interested in knowing how people sought health care. In spite of this, one of the diviners insisted that I was not telling them the truth and continued to claim that I was a medical doctor. She told me that next time I visited them I should also bring some medicines for her stomach ulcers. At that time I thought that she was just joking. On my next visit a week later one of the informants I had interviewed the previous week referred to me as “*awa mba dokotala withu awo tikachezganga nawo sabata yamala*” meaning this is our doctor we chatted with last week. Cheruzgo together with NyaNundwe had left for Zambia where they had very urgent business. Before leaving she had left word that I should leave the medicines for her ulcers with her friend. Despite the fact that I explained to them that I was not a medical doctor people could not believe it, insisting that I was a physician.

Language as an Asset and a Way of Enriching your Vocabulary

Sharing a language with informants is an asset because “it facilitates communication, saves time and enables avoiding distortion by interpreters” (van Ginkel 1998:255). These are some of the advantages of doing research in one's home area. I know the Tumbuka language well. However, during the research process I did realise my own inadequacies in the language⁹. At times, I felt like a child learning to speak. Certain phrases and words were strange to me and I had to ask

⁹See Perry 1989 who thought that being from an English speaking background and doing research among the English speaking whites in Australia. This was however not the case because the research he was doing had its own language and he had to learn it over again (Perry 1989:8-23).

for their meaning. Examples include words such as *chijulamphinga*¹⁰, *loko*¹¹, *kawinga wazimu*¹² and *kupala moto*¹³. My stay at home has enriched my vocabulary considerably. My knowledge of the Tumbuka language saved me the problem of learning a language which is both time consuming and not easy in "the absence of an adequate grammar or dictionary" (Goward 1984). It should also be pointed out that fully understanding and knowing the language enables one to follow discussions and ask questions wherever you need clarification. This is the major advantage of doing anthropology at home.

Discussion: Conducting Anthropological Research at Home: Whose Home?
(Greenhouse 1985)

The problems of anthropological work at home have been raised and discussed by a number of anthropologists. For outsiders, the major problem is to understand the culture, the language and way of life of the people under study. These are some of the major obstacles to obtaining useful information. For those doing research at home, the major problem is assumption of respondents that they know the answers to their questions. In the following few paragraphs, I will present my case which shows that though I am a Tumbuka studying my fellow Tumbuka people, I am an outsider and a marginal native in my own culture and this will enable me to study anthropology at home objectively.

The Defamiliarisation Process

In 1998-1999, as part of an MA programme in medical anthropology in the University of Amsterdam, I was interested in determining how illegal

¹⁰*Chijulamphinga* means sororate.

¹¹*Loko* (lock) is an illness in which the patient is not able to eat or swallow anything even water and porridge. It is believed that witches can lock the oesophagus preventing food from passing into the stomach. When such a patient eats he or she vomits immediately.

¹²*Kawinga wazimu* is a name of the tree that the Tumbuka use when taking the spirits of the deceased from the graveyard into his former house.

¹³*Kupala moto* means getting charcoal embers from someone's fireplace. (Initially, I was not interested in the genealogies of people from my own village. However, when doing genealogies of one family in my research site I heard that one woman had married someone from my home village. I therefore wanted to know how the man who married this woman was related to people in my own village and how I was related to that family in my research site).

immigrants from an African country sought health care since their illegal status made it impossible for them to approach official health care institutions. Fieldwork was due to start in May 1999, but I had already started making contacts with illegal immigrants through friends who were also pursuing postgraduate studies at the University of Amsterdam. However, before I started developing my research proposal, the birth of my son in January 1999 changed my plans. If I stayed in Amsterdam in order to complete my MA programme, I would not see my son until he was 8 months old. It was difficult to wait that long. After consulting my supervisor, I changed my research topic and decided to conduct research in Malawi. On 7 May 1999, I left the Netherlands for Malawi where I did my fieldwork.

During the time I was conducting fieldwork for my Master's degree, I learnt of a number of incidents that had taken place after the birth of my son. Some of these had been communicated to me while I was still in Amsterdam. Our parents wanted our son to wear amulets around his neck and we did not understand the rationale for that. Similarly, when the umbilical cord finally detached itself, a lot of precautions had to be taken in getting rid of it. During the Master's programme, I read that among the Macua of northern Mozambique, the fertility of a woman can be destroyed by "not burying the umbilical cord straight up (Gerrits 1997:39-48). I therefore wondered whether the reasons for taking precautions when disposing of my son's umbilical cord were the same as those given by Gerrits. On arrival in Malawi, my wife also informed me that since the birth of our son she had not been allowed to sleep on a bed or in our bedroom: she had been made to sleep in another bedroom on a mat with our child. The reasons for this were not clear to me. When one day my son was suffering from a terrible cough, one of my aunts arrived with an axe and wanted to perform a ritual aimed at curing as well as protecting the child against coughing. Though both my wife and I are Tumbukas it was really very difficult for us to understand the relationship between an axe and the child's cough¹⁴. Nobody was able to give me convincing answers to all these questions. Such incidences have "estranged me from my own culture, a culture I thought I knew" (see van Ginkel 1998:258)

Though I am a Tumbuka, my lack of understanding of the Tumbuka culture has been worsened by the fact that I have spent very little time in my home village since I left my home to go to boarding school at the

¹⁴For a detailed analysis of the Tumbuka perceptions of coughing see Munthali 2000.

age of 16. During holidays, I visited my father who at that time was working in Zambia. After secondary school, I went to the University of Malawi in Zomba, southern Malawi where I spent 6 years doing undergraduate and postgraduate training, and after this I started working in Lilongwe in 1991. During this time, a year would pass without my going home and when I did go home the visits were very brief. The only times I spent more than a day or two at home were when a very close relative passed away and I went to attend the funeral. At that time it was also not easy for unmarried persons to become acquainted with issues surrounding children, childbirth and other similar events.

Although I have grown up in the village, I have noticed that a lot of ethnographic work has been carried out on the Tumbuka of northern Malawi. Early missionaries attempt to document some of the cultural practices of the Tumbuka (see Elmslie 1899, Fraser 1922, Cullen Young 1932 and Foster 1989¹⁵). This was, however, before the development of modern anthropology. The most recent ethnographic work among the Tumbuka was done by Friedson (1996) who studied the "dancing prophets" of northern Malawi. This well written work reminds me of the period before I went to secondary school when I used to attend divination ceremonies with my friends. Attendance at these ceremonies was often given as an excuse to our unsuspecting parents when, in fact, all we wanted was to be out of the house at night with our girlfriends.

After discovering that I had very little knowledge of my culture, I thought that I should make up for this deficiency by reading some ethnographic material written about the Tumbuka. Such material was, however, very scarce or non-existent. It is out of these shortcomings and experiences that I have embarked on the long journey of re-discovering myself and my identity. I am carrying out my research in my own culture, amongst my "own people", approximately 15 kilometres from my home village. In some instances, I have even drawn some cases from my own village and the villages which surround it. The study I am currently engaged with aims at determining what the Tumbuka consider to be the most dangerous diseases threatening the lives of their children (especially those aged below 5 years) and their perceptions about the aetiology, treatment and the prevention of these diseases.

¹⁵Though Forster's book is relatively recent, his work is a review of the work done by T. Cullen Young when he was a missionary at Livingstonia Mission in the early years of the last century.

The study also examines the changes that have taken place in these perceptions and factors that may have brought about these changes. The study is mainly qualitative in nature, and makes use of participant observation, key informant interviews, in-depth interviews and projective techniques of data collection.

A number of anthropologists have referred to the fact that native anthropologists studying their own cultures are rarely full insiders. Like my case above, most native researchers stay away from their homes when they are undergoing training and only return after several years to study their own people. This defamiliarisation with one's culture allows the objective study of one's own culture. During the time he is away the native anthropologist attains a new status, an occupation, a new residence and a new way of thinking which in most cases is radically different from his "fellow natives". In this context even if he goes back to his home he will view things from a different angle and the things which are supposed to be familiar will turn to be unfamiliar. In my case the way my fellow natives interpreted my stay at home and the different hardships that I had to endure (for example sleeping on a reed mat for 3 months, sleeping in a leaking house, walking or cycling over long distances, the food I was eating et cetera) somehow created a "homesickness" in me. Why a homesickness when I was already at "home"? There were moments when I really wanted to return *home* to Grahamstown, South Africa where I had a house and friends. This raises the question of what is home? In anthropological terms home is a very relative term: Reis says that it refers to shared experiences and processes of identification and continues by saying that gender, age and life events are all constituents of "at homeness" as much as anything else (Reis 1998).

Being a Tumbuka, my people accepted me as a member of the society: my participation in funeral ceremonies, rituals aimed at bringing the spirits of the deceased home, divination ceremonies, installation of chiefs etc was never questioned. This is what was expected of me and this, despite the fact that I was asking silly questions, distributing gifts etc. My long absence from home enabled me to look at issues objectively.

The choice of the topic has also enabled the creation of distance between me and my fellow natives: I am an outsider as regards the world of childhood diseases and how the Tumbuka people perceive and manage these illness. My interaction with women, traditional healers and biomedical staff at the two health centres in my research

site yields new material everyday (cf van Dongen 1998). Among the Tumbuka, women are the ones who spend a lot of time with children and in most cases they are the ones who first identify that the child is sick. Their knowledge about childhood diseases, treatment and prevention is enormous. These women have welcomed me into the territory to *teach* me about childhood diseases. When asked about childhood diseases most men referred me to women saying they are the ones who are more knowledgeable. Since gender is also a constituent of at homeness, in this context I was an outsider, a situation which allows for objective research.

Lastly, in this research I am looking at the change in the management of childhood diseases among the Tumbuka of northern Malawi. In this case, I compare the perceptions of young men and women with those of old men and women. When it comes to examining illnesses that afflicted children 50 years ago, and how these were being managed I talk to very old men and women. These old men and women are able to compare what used to happen in the past and what is happening now. By looking at the historical dimension of childhood illness management, and asking old men and women who are knowledgeable, I am in a way excluding myself from being at home. So though I was doing anthropology at home I can not say that I was at home since so many things were unfamiliar.

Some Concluding Remarks

Regardless of whether an anthropologist is conducting research in his/her home area or in some other exotic culture, it is inevitable that people in the site will come to their own conclusions about his/her role. During the whole research process I was very honest about my intentions: to determine what the Tumbuka people of northern Malawi considered to be the most dangerous illnesses threatening the lives of their children, their perceptions about the aetiology, treatment and prevention of these illnesses and how such perceptions have changed over time. While I, a "native", was trying to understand my fellow "natives", they were also busy trying to construct their own perceptions about my personal identity. This is why I was labelled as a CID member, a person who asks silly questions, someone who has been sacked and abandoned by his wife and child et cetera. Crick had similar experiences while he was doing his fieldwork in Kandy, Sri Lanka. He was an anthropologist doing research on tourism in Kandy

and at the same time a Research Fellow at the University of Peradeniya. However, because he was "white" and most of the times was to be found "hanging around" on the streets as do tourists, people said that he was a tourist though he was not (Crick 1989:24-40 & see Goward 1984:113; for more examples, see Hayano 1979:101-2). Among his fellow blacks, Jones was thought to be a Black Panther or connected with the establishment in some way (Jones 1970:254). People's past experiences may also affect the way they think about an anthropologist. For example, Jones, an American anthropologist working among the Lahu in Thailand, was thought to be a missionary because most of the Americans who had worked there were missionaries. It was not easy for him to convince them that he was just an anthropologist (Jones 1970:251-259).

My own people's attempts to give me an identity have at times made me think that perhaps I am no longer a native, no longer part of them! I have been away from my people for so long that all that used to be familiar has now become exotic. In this respect, my experiences resemble what M.N. Srinivas calls being thrice born (Nanda 1987:16). Despite all the problems I met in my fieldwork I found that being a native conducting research among my fellow natives was good because it allowed full exploitation of the anthropological method of participant observation. My participation in funeral ceremonies and other ritualistic ceremonies was something that my people expected me to do. My participation in these ceremonies was never questioned and had I not attended some of these ceremonies people would have asked why. When they needed financial contributions towards holding certain ritualistic ceremonies, as a member of the society, I was also approached and I paid accordingly. When a funeral happened when I was away I would come back and condole with the bereaved. On these occasions, I would sometimes I would call an elderly person to accompany me. My people did not question why I was interested in attending all the ceremonies. The problem only came when I started asking what they termed silly questions. While anthropologists still criticise anthropology at home because of lack of objectivity, Razavi reminds us that Third World anthropologists conducting fieldwork in their own countries and cultures/societies are in most cases not completely outsiders (Razavi 1992). This position allows them to participate actively (being empathetic) in the lives of their own people and such a position can easily be achieved if you are familiar with the

local setting. Doing fieldwork at home is a challenge and in my case it was an important step towards discovering my own identity.

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