

No. 12, August 2006

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Social Networks Project Working Papers are circulated for comment and to inform interested colleagues about the work in progress on the survey data collected by the Project in Kenya and Malawi. The Social Networks project is funded by National Institute of Health Grants HD/MH41713 on "Gender, Conversational Networks and Dealing with AIDS" and HD044228 on "AIDS/HIV risk, marriage and sexual relations in Malawi." The Project has also received support from NIH/ORI; NIH/NIA; The Rockefeller Foundation; The Transnational Cooperation Foundation, Government of Germany; The Research Foundation, University of Pennsylvania; and the Carolina Population Center/USAID Evaluation Project.

The views expressed are those of the authors and not necessarily those of the Project, the National Institutes of Health or any other of the Project's funding agencies.

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Social Networks Project
239 McNeil Building
University of Pennsylvania
3718 Locust Walk
Philadelphia, Pennsylvania 19104

Email: social_networks@pop.upenn.edu
Website: www.pop.upenn.edu/Social_Networks

**“Azungu” in the Village: An Ethnographer’s
Impressions of Fieldwork in Malawi**

Anat Rosenthal

Department of Sociology and Anthropology
The Hebrew University of Jerusalem, Israel

26 September 2005

“Azungu” in the Village: An Ethnographer’s Impressions of Fieldwork in Malawi¹

All young anthropologists leave for the field with Malinowski’s call to get off the mission balcony and learn more about the lives of the “natives” echoing in their heads. Moreover, we learn from the very early stages of our professional training that fieldwork is what defines anthropology as a discipline and ourselves as anthropologists. That said, fieldwork should not be seen only as an historical development or a as means of achieving professional identity, but as a practice that echoes an inner theoretical discourse concerning the multiple meanings of fieldwork. In the following pages, I describe my impressions from fieldwork in Malawi conducted during the first half of 2004. First, I discuss the theoretical discourse on fieldwork in terms of a few of the main issues that characterize it. Following that discussion, I offer an account of the fieldwork process, my own experiences, and several important questions arising from them. I conclude with a return to the theoretical framework of fieldwork as a research practice, its effect on my work, and the close relationship between theory and practice in anthropology.

It would take a white man arriving in Malawi² a day or two to learn the term *Azungu*. Several explanations as to the origin of the term can be identified. According to one, it is a distortion of the Chichewa verb *Zungalira*, which denotes going round and round in circles; others claim that it derives from a language of the neighboring Mozambique or that it is a corruption or mispronunciation of the

¹ The author thanks The Malawi Diffusion and Ideational Change Project at the Population Studies Center at the University of Pennsylvania, Susan Watkins, Hans-Peter Kohler and Jere Behrman for the opportunity to take part in the project and to Alex Weinreb for the initiative to get an anthropologist on board and his support of whatever it was I ended up doing in Malawi and well after. The MDICP3 team is thanked for their welcome, companionship and support, I learned from them as much as I learned from my days in the field. The Africa department at the Truman institute at the Hebrew University of Jerusalem and Eyal Ben-Ari are thanked for encouraging me to organize my thoughts on the experience of fieldwork and Raanan Amir and Yael Maoz-Shai are thanked for their helpful comments.

² Malawi is a small country of 118,480 square kilometers in the southeast of the African continent. It borders on Tanzania to the east, Mozambique to the east, south, and west and Zambia to the west. The most obvious geographical feature of Malawi is the lake spreading to the east, its waters covering almost one-fifth of the territory. Malawi’s population was about 11 million people in 2003, with a growth rate of 2.4% and a GDP per capita of \$625. Infant mortality rate per 1000 live births was 139.8; life expectancy at birth was 40.7 years for both females and males; and 57% of the population had access to improved water supply (Population Reference Bureau 2003). According UNAIDS reports for 2003, HIV prevalence in Malawi was 14.2% at the age group of 15-49 with low estimations at 11.3% and high estimations at 17% (UNAIDS 2004).

Swahili word *Wazungu*. Whatever its origin, the term *Azungu* appears as early as David Livingstone's description of the Zambesi expedition first published in 1865, in which he notes that "it probably means foreigners, or visitors, from *Zunga*, to visit or wander" (Livingstone 1865).

The term, associated with foreigners and especially with whites, and heard everywhere, from the children on the road to the women in the market, troubled me throughout my fieldwork: Was I more an "*Azungu*" or an anthropologist? To the people I met, I was first and foremost an "*Azungu*, but what did that mean for me? What were the implications of my identities as I saw them and as I was seen by the people in the field? Did they conflict or contradict? Does it matter if they did?

In the following pages I describe some general issues relating to these themes of fieldwork identity, drawing on fieldwork that I conducted in Malawi during the first half of 2004. I do so, not from a single theoretical position, and paying no more than superficial attention to key factors in social and political life in Malawi, but rather, as the title suggests, by building on some of my impressions conducting fieldwork in this setting. My description proceeds in three main stages. First, I discuss the theoretical discourse on fieldwork in terms of a few of the main issues arising from it. Second, in the main body of this discussion, I offer an account of the fieldwork, my own experiences, and several important questions arising from them. Finally, I conclude with a return to the theoretical framework of fieldwork in as a research practice, its effect on my work, and the close relationship between theory and practice in anthropology.

Ethnographic Fieldwork: A Discourse of Theory and Practice

All young anthropologists leave for the field with Malinowski's call to get off the mission balcony and learn more about the lives of the "natives" echoing in their heads: "This goal is, briefly, to grasp the native's point of view, his relation to life, to realize his vision of himself" (Malinowski [1922]1950:25). Moreover, we learn from the very early stages of our professional training that fieldwork is what defines

anthropology as a discipline and ourselves as anthropologists and it is that which separates us from the rest of mankind.³

That said, fieldwork should not be seen only as an historical development or a means of achieving professional identity, but as a practice that echoes an inner theoretical discourse dealing with the multiple meanings of fieldwork and the importance of the relationships anthropologists experience in the field. In recent decades, the discourse on ethnographic fieldwork has been broadened with questions of positionality, past experiences, personal history, gender and ethnicity that are reflected in the fieldwork anthropologists conduct. This discourse had changed the nature of fieldwork, from a means of gathering data to something encompassing not only knowledge of the researched society but also of the anthropologist's own experiences and their effect on his understanding of the field.

Three themes central to ethnographic discourse came to mind as I was trying to turn my fieldwork experience into a text. The first was the experience of strangeness. Leaving one's society and entering a field in which he or she will always be a stranger is one of the first difficulties an anthropologist encounters and one that may not be resolved even after long periods of fieldwork. The anthropologist in the field is first and foremost a stranger in all sorts of dimensions (especially if the field chosen is far away from home but also if the field is in a strange social arena of one's own society) and it is apparent that the experience of strangeness is relevant to the process of ethnographic fieldwork. From the time of Malinowski, anthropologists have been aware of their strangeness as an inevitability that crops up time and again during fieldwork and sometimes even assumes a role in the anthropologist's entrance to the field and in the contacts made during the initial period of fieldwork (Geertz 1973, Rabinow 1977, Barley 1983). The anthropologist's strangeness in the field might soften over time but in most cases never completely dissipates.

³ An interesting observation regarding the role of fieldwork in constructing professional identity can be found in the works of Barley (Barley 1983) and Rabinow (Rabinow 1977), who describe the attitude towards scholars who are considered knowledgeable but not "true" anthropologists as a result of their lack of fieldwork experience.

The contacts that anthropologists make with people in the field was the second theme that came to mind. “New acquaintances,” “respondents,” “informants”, are all terms we use to define the relationships we establish in the field in order to help us understand what we experience and interpret the information we collect. Anthropologists who have studied the nature of relationships in the field have characterized those relationships in many ways, finding codes that allow for them, often depicting their asymmetrical nature, and so on. Examples of anthropologists engaged in the process of making acquaintances in the field and in the art of finding informants can be found, again, in the methodological writings of Malinowski, who instructed anthropologists not to be seen with whites, fearing it would make the natives suspicious (Malinowski [1922] 1950). Much later work, however, dealt with encounters with informants and their position in their own society (as central or marginal, accepted or rejected) and the influence of the informant’s social position and interpretive skills on the ethnographer’s work (Turner 1960, Rabinow 1977)⁴.

The third theme of ethnographic discourse essential to my understanding of fieldwork experiences was the issue of gender. The fact that I am a woman influenced the ways I experienced the field, contacted people, and probably also had an effect on the “data” I collected. I was aware of the gender factor before entering the field, but I never realized just how important it would be. Powdermaker claims that a woman alone in the field has many advantages. Her access to the company of women is easier and opens a world that is closed to male researchers. But the price, as Powdermaker concludes, might be access to the company of men, especially in societies maintaining a separation between women’s and men’s worlds (Powdermaker 1966). Nadar, a researcher who had worked in Lebanon (Nadar 1986), claimed that during her research, the fact that she was a woman made it possible for a family to adopt her. Once adopted, her social possibilities increased, as did her feeling of safety. Nadar asserts that, in the community she worked in and at that time, strange women were more acceptable and aroused less suspicion than strange men.

⁴ For more on anthropologists and their informants, see Casagrande’s book “In the Company of Man” (Casagrande 1960).

The ethnographic discourse dealing with the effects of gender on fieldwork is conscious of the ways in which gender directs relationships in certain ways while concealing and disclosing different worlds. More than once I found myself wondering what sort of world a man doing my job would have encountered.

To be truthful, I was well aware of all those aspects of the ethnographic discourse. Thoughts of their meaning and significance were part of my fieldwork even before I packed my bags and left for the field, and they were to reappear many times while I was in the field in all sorts of situations, expected and unexpected. Knowledge of the complexity of the discourse and its various meanings turned the fieldwork experience into a more conscious experience in which theory and practice are no longer neatly separable. On the other hand, this inability to separate theory written by others from my own fieldwork practice, confusing as it was, gave my private contextual difficulties the wider perspective of others. I wasn't alone in my field; the writings of others accompanied me.

Doing Fieldwork: The How's, When's and Where's

I arrived in Malawi as the student ethnographer of the MDICP (Malawi Diffusion and Ideational Change Project) research group of the University of Pennsylvania (more details at: Malawi.pop.upenn.edu). The aim of the research (it was the third round of data collection after previous rounds in 1998 and 2001) was to examine the role of social networks in changing attitudes and behavior regarding family size, family planning, and HIV/AIDS in three research areas in rural Malawi. The project focused on two key questions: The role of social interactions in the acceptance or rejection of modern contraceptive methods and of smaller ideal family size, and also the diffusion of knowledge of AIDS symptoms and transmission mechanisms together with acceptable strategies for protection against AIDS.

In contrast to other researchers, primarily sociologists and demographers collecting survey and other standardized types of data, I was asked to devote my stay to answering questions about community reaction to the study. The MDICP is a quite unusual research project. For the last 6 years it has been returning to the same respondents in three rural areas of Malawi. The "season" that I was there, the MDICP planned to add to their normal battery of questions, supplementing the

collection of biomarker data on HIV and STD status. The question was, beyond estimates of general response rates and item response error, how would respondents react: to the questions themselves? To the project's gifting strategy? To the study in general?

Before I left for Malawi it was decided that due to the nature of the work I was about to do, I would not travel with the research group between the three sites (in the north, center and south of Malawi) but would stay permanently in the mainly Yao and Muslim villages of the southern site. In this area I was supposed to find a village from the survey as my site and visit it regularly, getting to know the village and its people. To answer the questions posed, I planned to arrive in the village before the research team, stay there while all stages of the study were conducted, and remain after the team had left the area.

My fieldwork experience can be divided into two periods. The first, which lasted three months, began when I arrived in Malawi before the other members of the team and was spent in the village area even after the team arrived to conduct training sessions and finalize their questionnaires and other fieldwork preparation (I met with them on weekends). During this period, I was doing my ethnographic work in the villages while the team was setting up in Zomba (an hour's ride by bus from the place I was staying, assuming I could get a bus, that it didn't break down along the way, that it wasn't raining, and that the bus wasn't delayed somewhere on market day). During this period, I worked independently while living in a motel located half an hour from the villages of the survey.⁵ The possibility of my living in the village was rejected due to the living conditions in the village, where no running water or electricity could be found and where the rainy season made malaria a potential threat. Another issue was that of my own personal safety, which could have been compromised by living alone in the village.

⁵ Later I learned that this place I called "home" had been a lot cleaner and safer when the team was there in the past, which was the reason I stayed there in the first place, expecting these to be the conditions under which I'd be working. Even after team members found better places to reside, this motel was nevertheless the closest one to the site, and because I didn't have a car, it remained the best available option for me.

The second period of fieldwork, which lasted two months, was spent in the camp built by the research group close to the villages of the southern site. The camp was located about half an hour's walk from the village where I was working. In contrast to the first period of fieldwork, characterized by loneliness and a struggle with living and working conditions, the second period was characterized by intensive working and living with the group, something I had to get used to again. After living on my own in a motel, camp life was a treat. Even though we camped outside in the winter and some of the tents threatened to collapse each time it rained, we had great food, electricity (powered by a solar panel, a generator and a few car batteries) and vehicles I could sometimes use (and after months of public transportation this was a treat). Although camp was a step up in almost every way, looking back on it, the best thing about this period was the company of other researchers like myself. Anthropologists are often portrayed as lone wolves, and maybe they are in many ways: but after a while, it was great to be among other strangers like myself, not only in order to exchange ideas and impressions, but also for more mundane things like watching DVD's together.

Balaka, Spring 2004

Malawi, as it was revealed to me as the plane landed in Lilongwe airport, looked nothing like the country I expected to find. In that rainy February it looked green and fertile, nothing like the poor and hungry place I had read about. The first picture I have of Malawi is that of a man dressed in shorts cutting the grass of an airport lawn with a machete--framed by an airplane window while in the background the voice of the pilot announces "Welcome to Lilongwe International." At that moment I could hear the voice of sanity, the voice that every ambivalent adventurer knows and represses, uttering "oh, what have I done..."

The Malawi of February, 2004, that of the city, the newspapers, and the university, talked about the coming elections in May. The current president, Bakili Muluzi, was prevented by parliament from running for reelection after completing two five year terms, and the battle between his successor, a representative of the UDF governing party, and the candidates of other political parties intensified with each passing day.

Malawi in the Balaka area villages in which I conducted my fieldwork talked about the elections...sometimes. In the village, people talked about village news, funerals and weddings, the latest gossip, and the coming maize harvest. Political interest in the village was voiced among people holding political positions--party representatives and "votes contractors." In my travels, I found myself stopping at local political rallies on the side of the roads to listen to the speeches and look at the women wearing party colors dance and sing. The rumor in the village was that people attending those rallies were compensated with food, alcohol, and even cash. Paid or not, the "side of the road rallies" became more and more common as Election Day drew nearer.

The height of political excitement in the village where I was working occurred at the beginning of April, when the president and his nominated successor visited the local trading center on a day that is remembered as one of the most exiting and most frightening days of the fieldwork. The trading center was colored in yellow (the ruling party's color) and while the president and his successor sat at the back of a truck under a yellow umbrella, the Banda band (one of Malawi's most popular bands whose lead singer is a member of parliament) was playing. Thousands of people came from the area villages to see the president and listen to the band, and for the first time in months I felt like a foreigner, an *azungu*, the only one in sight. The comfort of being known (and of knowing) that I used to feel in the village was replaced by an uncomfortable sense of unrest that grew as people stared at me and demanded to know who was I, what I was doing there, and if I had money to give them. Since the president's visit was well secured, all the roads were closed and no busses ran in the area. And as time passed, I started feeling anxious. Luckily after a while I met one of my acquaintances, who took me under her wing and said: "Come with me, and please don't take out your camera here..."⁶

⁶ The elections were relatively quiet. Riots on the main university campus at Zomba ended in a shut-down that lasted well after Election Day, and on Election Day itself riots occurred in the big cities. Other than that, things seemed quiet and I remembered the words of a Sarah, a friend from Zomba, who to my question regarding her opinion on the election said, "They are going to be quiet, but not fair." European Union election inspectors I met just before elections shared Sarah's feelings, one of them saying, "Democracy is a process, a very long process..."

Fieldworker in Action

One of the first encounters I had in the field, and maybe the most meaningful of all, was with the woman who would later be my translator, research assistant, and teacher. We met during my second week in Malawi and from that day we worked together almost every day until she became ill. Grace⁷ and I met every morning and spent the days in the villages together. She was a widow who lived in a village close to the survey area, a high school graduate, and a mother of three who had worked previously with the MDICP. Grace's familiarity with the project was of value to our work, but in many cases it made things harder to explain. Grace was accustomed to working with sociologists and demographers and the nature of ethnographic work was more difficult to explain to her. In her eyes, there wasn't much point in "guests," as she called all non-Malawian researchers, running around in the villages trying to learn the language and asking questions. That was her job, and if I would only give her a questionnaire or a set of questions and a tape recorder, she would be happy to do it for me.

In spite of the difficulties in working with Grace, her role in my fieldwork was crucial. She was my translator and interpreter and a large part of what I know about village life I learned from her and through her. She opened her home to me and through her life and her family I learned more about the life I wanted to study. When our work was interrupted due to her illness, I knew how hard it would be to replace her as she was the one who accompanied me during my first weeks in the field even when my directions were a mystery to her. The first thing I ever asked of her was to "help me find a village to work in..." and she did.

The first weeks were dedicated to wandering the villages on the survey list looking for a suitable place in which to work. Every morning Grace and I bought bags of sugar as gifts for the village headmen we were about to meet, traveled on the bicycles we rented, and searched the villages for the one most appropriate for our purposes. The visits were much alike. In every village, we were walked to the headmen's house and, wherever the headman was available, we were seated on a mat outside his house where, after the greetings, we made clear our request to stay

⁷ All the names in this report were replaced by the author for the sake of confidentiality

in the village and talk to people about the surveys of 1998 and 2001 and about their lives and thoughts. We were welcomed in every village and after thanking the headman for his generosity, we gave a gift of sugar to his wife or to one of the children. A few of the headmen I met gave their permission for me to tour the village and study its location and size, and on some occasions I was even escorted by the headman's people, who showed me their village and its boundaries.

Although the visits were much alike, each one was special. In one of the villages, I met a very young headman who did not remember the surveys of 1998 and 2001 and called a group of elders to ask their advice. In another village, I could not find the headman but noticed a gathering of women under a tree which turned out to be an "Under 5 Clinic," in which local health representatives would meet with village women and their young children in order to do follow ups on children's weight, to vaccinate babies, and to give lessons about health⁸. A third village was so big that it took a day to walk about and in the case of the fourth, it was raining so heavily we had no choice but to sit under the roof of an abandoned house and wait for hours.

As the days went by, I realized I knew what I was looking for. It was a village which was a part of the survey. Not too small, so that the number of respondents would be large enough to be meaningful, and not too big, so that the survey's impact would be apparent and that I could cross it from one side to another in a reasonable amount of time. Proximity to the main road, I learned in time, was also a great advantage. Finally, the last criterion, which was vague and hard to explain, but I think frequently drives the choice of research sites even if there is not an openly admitted criterion was: I wanted a nice place.

One morning, as I was sitting on a mat under a tree outside one of the headmen's houses, I knew I had found my village. It was the right size and close to the main road. More importantly, all the people I met were kind and welcoming, the first and

⁸ I found the "Under 5 Clinic" extremely interesting and it was a subject I chose to follow because these clinics represent a very active role taken by women in the field of health, women who are usually seen as very passive.

foremost among whom was the headman, who said “I’m happy you want to come and hear what my people have to say.”

The characteristics of the village were a bit different than those of other villages I visited in the area. It was a Ngoni village (while most of the villages in the area were Yao); its people had arrived in the area years ago from the north of Malawi. Since the people of the village had lived in the area for decades, the Ngoni tradition was known but blurred with the Ngeni language and the patrilocal customs. In its place, Chichewa (“the language of the Chewa”) was adopted, as were the matrilineal customs of the Yaos who lived in the area. The village was also mostly Catholic – in contrast to the mostly Muslim villages in the area – and very close to the mission on the other side of the road.

As soon as I knew this was the village I wanted to work in, I began my daily routine. Every morning I would come to greet the village headman and his family at their home. From there I would be led by one of the children to a family who had agreed to have me as their guest for the day and with whom I would spend the day doing whatever it was they were doing that day. During my time with the families, I learned to peel maize, store it, or grind it into flour (The first two days of peeling maize were painful and resulted in my having two very large blisters on my thumbs, which were the subject of a lot of local jokes). I learned about daily routine and diet; I learned how to cook *N’sima*; and I listened to conversations about children’s education, local gossip, and anything else that was on the agenda. Being a guest of those families helped me learn about the day-to-day routine of village life, and as days went by I learned more and more--as no job is done in silence.

Many conversations were reciprocal in nature, as people often had as many questions for me as I had for them. For example, I would ask about life in the village and was answered--and then they asked in turn about Israel, my home, and my family. People wanted to know who my parents were, if villages in Israel had electricity and running water, if people in Israel ate maize, if Jerusalem really existed, and how I got from Jerusalem to the village. After a few weeks of daily visits, people’s looks were less suspicious and more forthcoming. Children no longer stared at me from a safe distance, but came and sat with me (or on me), and

babies no longer cried when I looked at them. My strange appearance continued to cause laughter, but in time, I learned how to wrap my *chitenge*,⁹ covering my pants, and to cover my long, light colored hair.

But of course, despite my efforts, I remained a stranger. At some point I was able to hold short conversations with people and understand some of what was being said around me, but I was always dependent on interpreters. My hair and skin color remained an attraction to children who, after coming to know me, used to sit in my lap and play with my hair. On the third month of fieldwork, the village headman said with a smile that when I first arrived, I wasn't very pretty but that now I looked much better. When I asked him what had changed, he said "your color is different." He was right: after three months in the sun I had a tan.

Compliments of that nature raised a good deal of laughter among the women and children, but in time I learned to regard that sort of reaction as a show of appreciation. On one occasion, the headman's sister-in-law saw me sitting on the ground outside one of the shacks and said with a smile, "You sit like a Malawian woman," adding, "I heard you also know how to cook *N'sima*--you are a real village woman."¹⁰ As I learned to stop asking questions and listen to what was being said around me, I learned more about village life. I was there and things just happened.

One of the things that I learned from the hours of listening was what people remembered and thought about the MDICP survey. To my surprise, not only were past respondents remembered, but many other people were as well. People in the village remembered the interviewers (very young, with a lot of questions), the questions themselves (very hard, some of them personal), and the gifts (sugar and soap). When the survey team started its work in the village, I found myself in a position of middleman (or middlewomen), explaining why only some people were

⁹ A cloth wrapped around women's waists.

¹⁰ It is interesting to note, in this context, Laderman's discussion of the embodiment of fieldwork (Laderman 1994), as the practice of sitting on the ground with straight legs was very painful for me in the first weeks of fieldwork, but later became a natural pose I find myself in even today.

part of the sample and not others, and listening to complaints about missing gifts and rude behavior toward elders¹¹.

No doubt I was attracted to the company of women and that women accepted me more easily. I spent most days with women, old and young, and through them I met their husbands and sons. As in Powdermaker's experience, my womanhood opened a door to the lives of Malawi's women and helped me maintain a relationship with them¹². When I asked the headman and his wife why was I accepted by people, I was told that after so many weeks people, started seeing me as the *Mfumu* (headman) and *Mayi's* (mother) daughter. Only later was I to understand how great a compliment it was being called a daughter in a matrilineal society, where girls stay close to their families. Apart from that, I was esteemed for my good manners and my willingness to help with work and not to interrupt. "You are a good daughter," I was told by the *Mayi*.

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Mis en forme

This was my routine for months until one cold day in July I parted sadly from the headman, his family, and my other friends, wrote down their addresses and gave them mine, and left for a place "so far that one airplane is not enough," as the headman explained to his youngest daughter.

Words of Conclusion, or, "Azungu" vs. Anthropologist?

In my opening remarks, I tried to address the role of the discourse on the practice of fieldwork on fieldwork itself. Discussing issues troubling anthropologists in the field, such as strangeness and relationships with people in the field, is not unique to one work or another. These questions, as a part of fieldwork experience, enhance the researcher's awareness of fieldwork as a practice that cannot be separated from theory. However, helpful as that awareness may be, a well-versed anthropologist would know that the discourse of fieldwork is mostly one of questions only half

¹¹ I discuss the subject of community reaction to the MDICP3 more broadly in the field report summarizing the ethnographic project.

¹² I truly understood how important gender was when I was forced to replace my research assistant and for a brief moment considered hiring a young male college student. As I was interviewing him, I realized I could not sit with him on a mat with the women of the village. Once I realized that, I hired a village woman who was slightly older than me but who was a high school graduate and a fluent English speaker.

answered, at best. Proficiency in the discourse of ethnographic fieldwork may assist the researcher in dealing with the difficulties and complexities of the field, but this discourse is, by its very nature, one of unanswered questions. Still, it empowers the personal nature of the fieldwork experience, in which one has to find her own moral and professional way. This is the hardest part of ethnographic fieldwork, but therein lies its magic.

As for the question that bothered me regarding my own place --as an "Azungu" or as an anthropologist: I guess I will always be both as the tensions shift between the two, though for the people I met in the field, I suppose I would always be an "Azungu," due to the color of my skin and my tendency to "go round and round in circles." As for myself, I suppose that now, after having survived the "right of passage" of fieldwork, I'm more of an anthropologist.

Work Cited

- Barley, N. (1983). *The Innocent Anthropologist: Notes from a Mud Hut*. Penguin Books.
- Casagrande, J.B. (1960). *In the Company of Man*. New York, Harper & Brothers.
- Geertz, C. (1973). "Deep Play: Notes on the Balinese Cockfight". *Interpretations of Cultures*, New-York, Basic Books.
- Laderman, C. (1994). "The Embodiment of Symbols and the Acculturation of the Anthropologist" In Csordas, T.J. (ed.) *Embodiment and Experience: the Existential Ground of Culture and Self*. Cambridge, Cambridge University Press. Pp.183-197.
- Livingstone, D. (1865). *The Zambesi Expedition: A Popular Account of Dr. Livingstone's Expedition to the Zambesi and its Tributaries and the Discoveries of Lakes Shirwa and Nayassa 1858-1864*.
- Malinowski, B. ([1922] 1950). *Argonauts of the Western Pacific, an account of native enterprise and adventure in the archipelagoes of Melnaesian New Guinea*. London : G. Routledge & sons.
- Nadar, L. (1986). "From Anguish to Exulation". In *Women in the Field: Anthropological Experiences*. Golde, P. (ed). University of California Press. Pp. 97-116.

Powdermaker, H. (1966). *Stranger and Friend: The Way of an Anthropologist*. New York and London. W.W Norton and Company.

Population reference Bureau. (2003). *Country Profiles for Population and Reproductive Health: Policy Developments and Indicators 2003*. (<http://www.prb.org>).

Rabinow, P. (1977). *Reflections on Fieldwork in Morocco*. University of California Press.

Turner, V.W. (1960). "Muchona the Hornet, Interpreter of Culture". In Casagrande, J.B. (ed). *In the Company of Man*. New York, Harper & Brothers. Pp. 333-355.

UNAIDS. (2004). *Epidemiological Fact Sheets on HIV/AIDS and Sexually Transmitted Infections*. (www.unaids.org).