In the Shadow of the Sheraton: imagining localities in global spaces in Dar es Salaam, Tanzania
Moyer, E.M.

Citation for published version (APA):

General rights
It is not permitted to download or to forward/distribute the text or part of it without the consent of the author(s) and/or copyright holder(s), other than for strictly personal, individual use, unless the work is under an open content license (like Creative Commons).

Disclaimer/Complaints regulations
If you believe that digital publication of certain material infringes any of your rights or (privacy) interests, please let the Library know, stating your reasons. In case of a legitimate complaint, the Library will make the material inaccessible and/or remove it from the website. Please Ask the Library: http://uba.uva.nl/en/contact, or a letter to: Library of the University of Amsterdam, Secretariat, Singel 425, 1012 WP Amsterdam, The Netherlands. You will be contacted as soon as possible.
Introduction:
(Re)searching for *Maisha*

The joy of this world has an end
We are born, we meet joy and trouble in the world
The first thing a person should think about
Having a good life
Eating well
Dressing well
Nice house
Money in your pocket
Life, life, life
Go slowly (in a steady, orderly manner)
If you want to have the good life like important people
Like the farmer, choose the hoe to be on the farm
Like the businessman, run to your office
Or if you are a student, to your school
Life it will come later
Life, life ...
Go slowly (in a steady, orderly manner)
Life, you should not hurry
Life, step by step
Life, you should not hurry
Go slowly (at a slow pace)
Life, you should not hurry

*Maisha (Life), Remmy Ongala*
When I arrived in Tanzania in September of 1999 the whole country was abuzz with one thing: Mwalimu Julius Nyerere, the founding father of the county, was ill. Newspapers, radio and television broadcasts were reporting that he was being treated for leukemia in a hospital in England. Citizens of Dar es Salaam, always willing to read between the lines, suggested that it was likely that he was dying. Some even said that he was already dead, and that the news was being kept secret in order to allow the Tanzanian military and police guards to prepare themselves for the uprisings that were sure to accompany its announcement. During these uncertain times, I found the presence of so much conjecture, so much guessing, and so much gossip comforting; as an anthropologist, I was pleased to find that dominant discourses were being challenged so openly and so loudly. One particular rumor, which I first overheard on a local city bus, was that Nyerere had AIDS. People noticed how the already thin Nyerere was rather quickly becoming emaciated, and pointed out that in recent press photos it was possible to see small discolorations, or lesions on his skin. That AIDS was the result of Mwalimu's great love of women was often insinuated, and occasionally mentioned directly—an occurrence that increased upon the eventual announcement of his death a few weeks later. It has never been proven that Nyerere had AIDS, and in fact it was never even mentioned in official accounts. There are some who would say, however, that, then again, it was never dis-proven either (Fig. 1.1-1.5).

Despite suggestions by some researchers that AIDS is a taboo topic rarely discussed in public in Tanzania, people do talk about it quite openly and regularly within the context of their everyday lives. Perhaps, however, I should say everyday deaths, because it is usually when someone is dying, or has recently died, of something that people suspect of being AIDS that one is likely to hear people talking about it, as the example about Nyerere illustrates. The research for this book was initiated in order to understand how ordinary Tanzanians make sense of what is often portrayed as an extraordinary disease in the context of their daily lives. In many ways it departs from what one might consider conventional AIDS ethnographies, a difference that is as much a result of the methodologies employed as the material gathered. What follows in this chapter is a three-part discussion that addresses methodological and theoretical issues relating to the project's conception, undertaking, and representation in the form of this book.

The Emergence of Doubt

In September 1999 I set out on my third trip to Tanzania, this time with the explicit aim of undertaking a year of ethnographic pre-dissertation research among disadvantaged urban youth in Dar es Salaam. With an estimated population of three million, Dar es Salaam is the largest city in the country, serving as both the primary port, as well as providing a home base for most international organizations and businesses operating within Tanzania. Dar es
Salaam is the economic and cultural capital of the country, while the city of Dodoma, located in the center of the country, is designated as the political capital. Parliament does indeed meet in Dodoma, but most politicians prefer to do business in Dar es Salaam. On prior trips to Tanzania in 1996 and 1998, I conducted two separate studies. The first attempted to examine tensions between so-called traditional and modern public health techniques employed to increase awareness of modes of prevention and treatment for HIV/AIDS (see Moyer 1997a; 1997b), while the second pilot study focused on gaining a greater understanding of why some prevention/treatment approaches are more effective than others.

During the 1998 research period, which had the second aim of helping me to formulate a coherent dissertation proposal, I found myself increasingly frustrated with various aspects of conducting research focused on HIV/AIDS. Specifically, I found it difficult to come to peace with what I perceived as certain ethical dilemmas regarding the nature of this research. Confronted daily with the relative failure of most public health initiatives geared toward prevention and treatment of AIDS, I just could not see how another research project, mine, was really going to make a difference in the lives of the people who agreed to participate. This rather banal observation confounded me both ethically and morally. I was becoming increasingly concerned that public health initiatives related to HIV/AIDS in Africa, whether sponsored by local, national, or international agencies, were employing a rhetoric stressing the emergency aspect of the pandemic to justify research and treatment protocols that clearly fell far short of international standards.
ethics standards. In recent years, this issue has increasingly come to public attention with the simultaneous availability of more effective treatment regimes (i.e. the use of AZT to prevent mother-to-child vertical transmission of the HIV virus (e.g. Susser 1998, Fadem and Kass 1998), and the demand for less expensive/generic versions of protease inhibitors. The concerns I had, however, extended beyond the problematic of bio-medical treatment regimes and policies of multi-national pharmaceutical producers to include ethical questions related to conducting quantitative research, specifically, applied ethnographic research.

As an anthropologist engaged in a pursuit that often blurs the lines between applied and theoretical research, such concerns are of particular importance to me for professional (ethical) reasons, as well as personal (moral) reasons. The ethical questions at stake ask whether or not such research methods should be employed, while the moral questions at stake ask whether I would want to participate in such research, ethical or not. I make this distinction because I would hope that ethical questions which confront disciplinary methodology would be of interest, but I am well aware that my own moral dilemmas may not be of the greatest interest to the scientific reader.

I never grew comfortable with the degree of power that my position as a white, Western researcher with a research grant in my pocket gave me. That I could walk into nearly any HIV/AIDS treatment ward in Tanzania and expect dying people to talk to me about the most intimate aspects of their lives despite my increasing belief that such methods would fail to produce any new insights on the pandemic was a contradiction I was never able to resolve. The more time I spent conducting AIDS related research, the less inclined I became to try to resolve this contradiction. Dying people deserve to be treated with dignity and respect and, unless an invasion of their and their family's privacy can be justified by the reasonable belief that it will prevent further suffering and death, such an invasion must be avoided. I could rationalize my own research by arguing that, if nothing else, it served to train me as an ethnographer, but I wonder how many of those who spoke with me would have done so had they believed this to be the primary objective of my research, had they realized that despite my white skin, my relative position of wealth and power, and my genuine desire to improve their situation, their participation in my research project would bring about little if any insight into new or better ways to prevent HIV/AIDS. I might also be inclined to rationalize such research methodologies by waving signed consent forms: if people agree to participate then my ethical responsibility is fulfilled. But did my forms, or my words ever acknowledge my limited power? Aside from the solace of deathbed company and the occasional small personal favors I could perform, the need for my presence was not justifiable and, for that reason, at least in my mind, unethical. Although not medical practitioners per se, surely those engaged medical anthropology must observe the most basic of medical tenets: first, do no harm. Perhaps I would not consider this an issue of such great importance if it were not for the large numbers of researchers current
ly attempting to undertake such studies in Africa (and throughout the developing world as a whole). In many African countries there is minimal, if any, legislation in place to protect patients’ rights. Additionally, it is often quite easy to bend and/or break the rules that are in place (some money here, a small favor there), but this does not mean that they should be broken. It is precisely because of a lack of official measures for patient protection in many countries that researchers, medical and non-medical alike, must make efforts to observe the prescribed (if not practiced) ethical standards of international health agencies. These standards may not in fact be the best guidepost for ethical practices, but for the time being, it is the only one available.

What follows in this chapter is a description of methodologies I employed than it is an attempt to explicate the connections between methodological choices and the thinking behind them. I believe that the methodologies employed should reflect a researcher's beliefs regarding how knowledge is constructed and meaning made. Believing in the power of stories to situate knowledge in the everyday narratives of people’s lives, and rather disbelieving in the power of numbers and statistics to do so, I have chosen to conduct my research in a way that tries to open up and complicate discussions regarding the connections between poor health and people's lived experience, rather than in a manner that is geared toward finding specific answers to specific questions.

There are many who would argue that we need greater demographic and statistical understanding of AIDS in Africa if we wish to bring about changes in policy. While I am not opposed to the continued compilation of such data for epidemiological purposes and for the strengthening of economic arguments for combating AIDS, I find it unlikely that mere numbers will ever instigate meaningful changes in the way those who control the planning and funding of HIV/AIDS prevention and treatment programs think about AIDS. It would seem that the numbers we do have, despite their shortcomings, should be enough to instigate action. If numbers had the power to bring about change in this case, then one would think that a statistic such as 8500 people dying of AIDS every day would be enough to turn people's heads.
My attempts to impart a more situated understanding of how AIDS and AIDS discourses have become woven into the everyday narratives of people's lives is, in part, motivated by my view that such narratives have a greater chance than do numbers of challenging what we think we already know about AIDS, of planting seeds of discontent regarding the status quo, of haunting those who read and hear them—in effect, of bringing about a change in perspective that will be greater than the effects of changing any individual policy. I am less interested in bringing about changes in the way local, national, and international agencies approach AIDS, than I am in coming to a better understanding of what could help to bring about structural changes. This is not an attempt to "put a face on AIDS," an unfortunate euphemism all too often implied in media and by politicians, but rather an attempt to face AIDS, to face its everyday-ness, its banality, and, eventually, our failures in regards to it. For if there is a "face" to AIDS, we all share in its construction. The question now is whether we can all share in its deconstruction.²

"Young Urban People at Risk"

Not surprisingly perhaps, I chose to change my focus before returning to conduct research for my dissertation. I had not, however, given up on trying to formulate an ethnographic study that might provide new insights for understanding HIV/AIDS, nor had I given up on trying to discover a more ethical path for doing so. Rather than working with those who were sick and dying in clinics and hospitals, or with those who were engaged in treating such individuals, I decided to work with a population that many consider to be at the greatest risk for contracting HIV/AIDS: disadvantaged, urban youth. Working with young people (approximately age 15-25) would allow me to see how people are living today in the age of AIDS, to begin to comprehend how the first generation of Tanzanians born in the age of AIDS experience and make sense of the disease, and also permit a possible glimpse into the future of the pandemic in urban Africa. I chose to work with this population in part because I expected to learn how they viewed HIV/AIDS. Many prevention programs on the continent expressly focus their attention on helping what has euphemistically become known as "young urban people at risk," but very little in-depth qualitative (or for that matter, quantitative) research has been done regarding their lives.

This is, no doubt, partially explainable by the perceived difficulties of working with urban youth. They are young, marginal, and constantly on the move, hard to pin down for any length of time, often difficult to communicate with (as the researcher must become familiar with local vernaculars that are often heavily inflected with slang), and considered by many as annoying hustlers or dangerous predators in the extreme. Add these elements to the fact that young people, in general, are rarely consulted regarding their views on the best ways to keep them safe and healthy, especially in many places in Africa where the opinions and views of elders are routinely sought out at the
expense of the views of young people. This bias is a result, in part, of past and current power structures geared toward controlling intergenerational conflict, which all but require that outsiders holding positions of power consult with local leaders before receiving permission to interact with members of the local community. I provide the following example to illustrate my point.

I traveled with my comparatively young research assistant, Derrick Mbelwa, to conduct an ancillary research project on the small coastal island of SongoSongo. Upon our arrival we were required to check into the local headquarters of Chama cha Mapinduzi (CCM), the ruling political party, where we were required to present the reasons for our visit to village elders. After begrudgingly granting us permission to conduct interviews with specific elders whom they would select for us, we were told to retire to our hotel. Although we were not in SongoSongo to conduct research on youth, they were the people we were most comfortable with socially. On our way back to the hotel we decided to stop and chat with a group of young men hanging out on the steps of a small restaurant. They were naturally curious about the reason for our visit to SongoSongo, as well as about our larger research project. After talking for about fifteen minutes many people began coming out of their houses in response to the evening call for prayer and we were publicly confronted by an old man who had already heard the details of our research and knew that we did not have explicit permission to talk to people on the streets, specifically not to young people. Having spent too much time in Dar es Salaam and having forgotten the rules of gerontocracies and patriarchies that all Africanists are taught, I actually began arguing with him on the street. Thankfully, Mbelwa, despite having been born in Dar es Salaam, was aware that I was overstepping certain boundaries and endangering our entire visit. He quickly stepped in front of me, apologized for my ignorance and begged forgiveness. Eventually the man was appeased, but following this exchange no one would talk to us in public again. Several people did seek us out, though never where we would be seen by elders.

One of the advantages of working in a city as large as Dar es Salaam is that it is often possible to forgo the need to clear one's activities with local elders once permission from the national government to conduct one's research is granted. Having worked in both rural villages and less cosmopolitan urban centers in Tanzania during previous research trips, it is clear to me that the size and diversity of Dar es Salaam permits a certain anonymity that would be impossible to achieve in other places in the country. Such anonymity translates to fewer people being interested in your research and fewer community leaders/elders attempting to control it. Simply put, aside from the occasional police officer ostensibly concerned with my safety, no one really cared that I chose to spend my days sitting on street corners in the city talking to young folks. Stranger things have surely been seen in Dar es Salaam, and no doubt those wishing to align themselves with foreigners in positions of power knew from experience that there were bigger fish to fry than me.
The Relevance of Language: It’s not what you ask, but how you ask it

Among the issues that make research among disadvantaged urban youth difficult to undertake, I found the transience of those I worked with, and the limits of my own language skills most challenging. Although my Swahili has become increasingly passable over time, Kiswahili cha mtaani, or street Kiswahili, changes rapidly and constantly, providing endless entertainment for those who employ it daily, and endless frustration for me. Initially, I was also concerned both for my safety and the safety of those who agreed to work with me. Although I do not discuss this at length here, such fear and the limitations resulting from it should not be underestimated. Having chosen to work with the more suspicious characters of the city, I was constantly warned to be careful by Tanzanians and expatriates alike. In fact, my safety was never threatened in Dar es Salaam and, as time and my local and linguistic knowledge progressed, I became increasingly comfortable going just about anywhere in the city. The importance of the “local and linguistic” knowledge to which I refer will resurface later in this text and, in fact, forms the basis of many of my arguments.

Perhaps the most important framing premise of the research is the assumption that one of the least effective techniques for understanding how people live with HIV/AIDS is to ask direct questions about it in a fashion out of context with their daily lives. As mentioned earlier, I no longer wanted to document AIDS deaths, or to focus my work on the way people who have the disease experience it. Much research has already been conducted in this arena, which I believe was especially important in the early stages of the pandemic when so little was known about modes of HIV transmission in Africa, or about people’s views regarding the disease. The sort of knowledge gained from focusing attention primarily on those suffering from a disease and those working in the health care professions, however, tells us little about the lives of the general population. Given that the great majority of those infected with HIV on the African continent will eventually die from opportunistic infections associated with it, it seems to me that a great deal of effort should be focused on preventing people from becoming infected with HIV in the first place. In turn, it would seem impossible to develop effective prevention measures without taking into account the views of those at whom such efforts are directed. This may at first seem a fairly straightforward assumption, but many well-meaning prevention programs fail to do just this.

Those who are in the position to develop and implement prevention programs rarely take the time to conduct in-depth qualitative research, despite recognizing how much more powerful their efforts would be if they made sense to the people they were set up to help. We see more and more lip-service paid to the importance of ethnographic research in the social, as well as the biological sciences; but in reality the time constraints that define most health related projects help to guarantee that ethnographic efforts are normally limited to rapid assessment techniques and/or knowledge, attitude,
and belief surveys. In-depth ethnographic research takes a great deal of
time, as does the writing-up of such research, and some may feel that such
methods have limited value in the face of life-threatening situations. As a
result, and I believe partially in relation to the rise of the AIDS pandemic,
those who may or may not be trained in ethnographic methods are increas­
ingly condensing the actual research portion of their work, and in many cases
putting forth recommendations for change before research is completed, as
well as advocating implementation of ideas based on findings that may or
may not be accurate. In the end, there are many HIV/AIDS prevention pro­
grams that wave the flag of cultural sensitivity, all the while failing to ensure
that the research results employed to back such claims are in fact valid.

At this point, I would like to stress that I position myself as a strong propon­
nent of applied anthropological aims that are engaged in trying to establish
meaningful applications of our knowledge in order to bring about social
change. Despite all the modernist entanglements such endeavors must by
definition find themselves, I am convinced that the work we undertake should
attempt to make the world a better place. This view is hardly counter to
those held by most practitioners of anthropology —applied and non-applied
alike. I do not believe, however, that having such views will automatically
allow me, or anyone else, to have particular insight on how to accomplish
this. The pitfall described above —making recommendations before com­
pleting a research project without the benefit of perspectives that may be
gained from taking time and distance from the project —is one of the easi­
est ones to fall into as an applied anthropologist. Therefore, I have made a
conscious effort to separate my own research from implementation proce­
dures whenever possible. In the end I decided to refrain from making recom­
mendations or from championing the implementation of interventions until
completing the ethnographic research portion of my project. I know there are
many who will disagree with this perspective and see my refusal to offer
clear-cut recommendations at this stage as ethically irresponsible in itself,
but I hope that these views will not end the discussion on the subject, nor
close the reader's mind to the rest of this book. Many governmental and
developmental agencies responsible for the implementation of prevention
and treatment regimes have too little time and money available for undertak­
ing research that attempts to understand the "big picture" of AIDS, that is,
how social and economic, as well as political and personal factors can syn­
ergistically contribute to higher HIV rates. It is this vein that I hope this book
will offer the most insight. Of course, it would be absurd to suggest that the
research I conducted did not influence and affect the lives of the people who
agreed to take part in it. In addition to the obvious ways an outside
researcher's presence can affect local social and power relations, I also
believed it my responsibility as a researcher to provide information to individ­
uals who seemed unaware how their actions might be increasing their
chances of becoming infected with HIV or any other disease.

The most basic premise of my research was that the way to find out about
AIDS was not to ask about it. I realized that I knew very little about what people did think about AIDS (given that most of what I did know was gained from employing the very research techniques I had grown to be suspicious of) and, that I was not even sure how to find out about it. As a result I attempted to develop an open and fluid methodology, expecting that it would change as my research progressed. In an effort to elevate the status of those I worked with from the position that is traditionally called "informant" in ethnographic research to a position of enhanced inter-subjectivity, I welcomed the opinions of those with whom I was working regarding continued methodological developments. I encouraged people to tell me when they thought I was doing something wrong, or if they thought I could do it better. I did not always agree with such critiques, nor did I always incorporate them into my work, but I always considered them. Simply put, I had to take the time in the beginning of my research to allow those with whom I was working to teach me how to ask questions; I had to learn how to frame both my observations and questions within local ways of knowing, sometimes at the expense of Western modes of inquiry that characteristically mark scientific research. In hopes of achieving a deeper understanding of AIDS among urban youth, I sometimes found it necessary to forgo scientific attempts to order and classify information, and trust that a kind of mimetic understanding would ensue. My trust was in part inspired by the Taussig's (1993) writings on the subject of mimesis, but also by phenomenological perspectives on the ways we acquire knowledge (Casey 1987; Jackson 1989, 1996; Roberts and Roberts 1996; Seremetakis 1994; Stoller 1997).

Working Together

Although there was never a question that this was my research project, it should be clear by now that the methods I employed were often dialectical to the extreme, informed as they were by ongoing discussions between myself and those who participated in the research. Besides my own, perhaps the loudest voice contributing to the evolution of the methods employed was that of my colleague, assistant, and friend, Derrick Mbelwa. I met Mbelwa rather fortuitously during my 1998 trip to Tanzania, but I knew him by reputation beforehand having read in local newspapers about his and a handful of other students' efforts to organize medical students at the University of Dar es Salaam's medical school (Muhimbili) around issues relating to low standards of instruction. In the end, the strike and the movement were quashed by the Tanzanian government, and Mbelwa was expelled from medical school with the understanding that he would be readmitted only if he formally apologized and signed a statement retracting his views. As far as I know, he has yet to do so. Of course, I did not know all this when I met him; I only knew that he was one of the student organizers, and that I was rather impressed by the fact. Mbelwa was not then, or at any time since, particularly proud of his efforts or his refusal to acquiesce. His quiet acceptance of his fate has led
me to believe that he could hardly have imagined doing otherwise. At the time of our meeting he asked me to help him get into medical school abroad—preferably South Africa, but anywhere would do. I confessed that I doubted that I could be of much help, but that I would keep my ears and eyes open on his behalf. I told him about the project I was thinking about undertaking and he told me about his own research on a similar subject. He was not particularly interested in AIDS, but more generally in documenting the lives of the young men he had come to know who lived and worked in the streets of Dar es Salaam. I was immediately drawn to Mbelwa and believed his unusual combination of intelligence, seriousness, self-righteousness, and a sense of humor would make him an ideal assistant. He was the right age (23 at the start of the research period), spoke near perfect English, had a background in medicine, and seemed to have a genuine concern for his fellow man. Before leaving Dar es Salaam in 1998 I proposed that we might work together when I returned the following year, assuming the project received funding and he did not find a better opportunity in the meantime. We shook hands, went our separate ways and had only minimal contact in the interim before my arrival in September 1999. The proposal I eventually wrote incorporated many of the ideas we had talked about during our brief interchanges in 1998, and was written with the underlying hope that Mbelwa would be able to work with me in the end.

It was not until after my return when I formally interviewed Mbelwa for the job of assistant and translator that I realized the degree to which I would be forced to challenge many of the unwritten rules of fieldwork. He insisted from the very beginning that his opinions regarding research methodology would be heard, and would be heard often. In fact, on that first day, I often felt that it was me who was being interviewed instead of him. Mbelwa, who was born and raised in Dar es Salaam, has witnessed first-hand the rapid “NGO-ization” of his country since the introduction of economic liberalization policies and nominal multi-party politics in the mid-1990s. He has also observed the abuses of power enjoyed by many of those employed in the newly emergent hierarchies that accompanied the changes. He was highly suspicious of organizations and individuals who claimed to have the interest of the people of Tanzania at heart, and he never failed to note how they too benefited from their positions. The rise in both status and power of non-governmentals in Tanzania has been no more remarkable than in many other underdeveloped countries in the world, the only difference is that it has occurred so rapidly and so recently. It is difficult for anyone to ignore either the ways in which local and national power structures have been transformed as a result, or the increasing differences between rich and poor that are reinforced by a system that compensates its employees rather generously. Development is big business in Tanzania, and projects related to AIDS are no exception. So Mbelwa questioned the system, and he also questioned my (and eventually his own) position in it. I would not have had it any other way as I was not interested in working with someone who was going to agree with me without asking
questions. I wanted to be challenged in my norms and in my beliefs about Tanzania, about AIDS, and about science, and it was clear from the outset that Mbelwa would rarely fail to do so. He was as committed to my research as I was, and on some occasions when I grew tired and disillusioned, it seemed he was even more so.

Mbelwa is not an anthropologist, and prior to our working together had never read an ethnography. After reading Philip Bourgeois’ *Searching for Respect: Selling crack in El Barrio* (1996) and Paul Farmer’s *AIDS and Accusation: Haiti and the geography of blame* (1992), at my suggestion, Mbelwa stopped by my home to let me know that he could not see how the research I was doing was going to shape up into a “classic” ethnography like those he had just read. Apart from being amused by his anxiety and excitement, which in many ways seemed to be mirroring my own fears, I was impressed by the carefully thought-out suggestions that he made regarding ways to change my research methods to improve the quality and depth of the information gathered. This incident occurred about a month after our agreeing to work together, but in many ways was characteristic of the manner in which we worked together throughout. I do not want to suggest, however, that there were never any problems between us, or that the resulting tensions did not affect the research. Quite often, we found ourselves disagreeing. At times, it was only with extreme effort that I was able to prevent myself from invoking my position of power, from reminding him that this was my research, and that I was paying the bills. Of course, our mutual awareness of these facts did to some extent structure our relationship, but I am pretty sure that had I not been committed to promoting a relationship based on equality and respect, Mbelwa would have refused to work with me, and he certainly would not have engaged in the project with as much commitment and integrity as he did.

**Networks in the Streets of Dar es Salaam**

We agreed that the best way to begin would be to take advantage of networks that we had already established in Dar es Salaam, those that I had identified on previous trips to Tanzania, and those that he had built and participated in throughout his life in the city. Between the two of us, we had more than enough to keep us busy. Most of my contacts were with those officially involved with AIDS research: NGO and medical personnel, government officials, as well as some artists and musicians who had been employed by such organizations to assist in efforts to embed prevention messages in culturally appropriate forms. Mbelwa had many formal contacts as well, but his preference was to talk to his friends, to young men with middle class backgrounds similar to his own, as well as those living and working in the streets of Dar es Salaam whom he had come to know over the years. For the first three months of the research —this can be called the preliminary phase — we tried to balance our efforts by working with persons formally involved in projects geared toward assisting disadvantaged youth and instituting
HIV/AIDS prevention and treatment programs, and individuals who such projects were trying to help. It became increasingly clear, however, that we would have to focus our efforts if we wanted to reach the sort of in-depth understanding we hoped to gain in the year we had set aside for the project. For a variety of reasons we gradually decreased the amount of time spent on working with persons producing and promoting official AIDS discourses and turned our attention to the streets and the popular discourses emerging there.

The “streets of Dar es Salaam” is a fairly vague reference. In urban settings it is always difficult to define communities. For the purposes of this study, I found this particularly to be the case when working with groups of young men and women who, for the most part, had no permanent dwellings that they could call home. Rather than using home life and domesticity as parameters of study, I employed notions of work and leisure. Rather than focusing on family or neighborhood networks, I began my study by “hanging out” on one particular street corner in the city center that serves as a locus of business activity —legitimate and otherwise — during the daytime and as a make-shift campsite in the evening for several of the young men who work there. This corner was not randomly chosen; in fact it was a corner where Mbelwa himself had been hanging out off and on for years. It was the place where I first met him, and the place where I first met many of the people who were to serve as the core of my study group. In effect, I benefited from the relationships of familiarity and trust that Mbelwa had built up over a number of years among those who frequented this corner. And, of course, I was also incorporated into a well-established social hierarchy as Mbelwa’s mzungu (white person), a position that was far from neutral.

Mbelwa had begun his own research documenting the lives and experiences of members of this population a couple of years before I began mine, but by the time I returned in 1999 he had abandoned that project. The main reason, he explained to me, was that as he got closer to the young men who agreed to help him and as their relationships evolved from one of researcher/informant to one of friendship, he found it more and more difficult to listen to and record their stories. For as he became more familiar with the circumstances that brought many of them to the streets in the first place, he developed a more acute awareness that, regardless of his desires to improve their lives, he was not in a position to bring about structural changes to the system that helped to produce an environment where young, undereducated men born in the rural areas of the country have little opportunity to succeed economically. While I did not ask for, or gain direct access to the tapes that Mbelwa recorded during his own preliminary research, he did share much of what he had learned with me during our conversations in an effort to fill in some of the gaps in my own understanding of the lives we had begun documenting together.
Maskani

In Chapters Three and Four, I provide a more detailed description of the street corner that remained the primary locus of our research and will, therefore, limit my discussion to a brief overview here. The corner itself, as might be expected from the title of this book, was situated in the immediate vicinity of the Sheraton Hotel in downtown Dar es Salaam. The young people I worked with there all knew one another to varying degrees. Some of them worked or hung out together, while others lived together, and some only knew each other another in passing. Many came from the same regions of the country, and a great proportion of them from either Arusha or the southeastern coast. The street corner where I met them was like many other loci of activity in the city where young men gather for both work and pleasure, where female food vendors serve up hot meals, and where any number of people may pass on a given day to hang out for a while, eat lunch, read the newspaper, and hear the news that may not be fit to print. But, this corner also exhibited a certain degree of specificity celebrated by those who called it their own as a civilized oasis in an otherwise chaotic city, as a place where peace reigned. Swahili speakers might call it a “kasheshe-free zone,” an ideal place for a young woman on her own to conduct research.

In Chapter Three I further elucidate why this particular characteristic, which made this street corner appealing to me, also made it appealing to those who worked and lived there. In Dar es Salaam, these corners are known as either maskani or kijiweni depending on whether an individual recognizes them as a place of leisure or work. Although for me this corner was a place of both leisure and work, I always referred to it as my maskani and will continue to do so. In order to reduce confusion for the reader, whenever referring to the Sheraton street corner I will capitalize and refrain from italicizing the term (Maskani). All references to other maskani will not be capitalized and will remain in italics. At times I will also employ the neologism “Wamaskani,” meaning the people of maskani, to refer to the many different people with whom I worked who made their living in and around the Sheraton Maskani. This is done in an effort to reduce wordiness and to limit the degree to which I have to make use of cumbersome expressions like the one offered in the previous sentence.

Although people throughout Tanzania gather together in certain places for both social and economic motives, the term maskani is not used everywhere. In rural areas, the term kijiweni is more popular. Kijiweni, literally, the place of a small stone, originally referred to the stones that were piled between fields in the countryside where farm laborers gathered at the beginning and end of the day to discuss work and life (see Weiss 2000 for further discussion). Before the term was co-opted by young people in Dar es Salaam, maskani was employed by CCM, the ruling political party of the country since independence, to refer to local party headquarters—a practice that still exists in some parts of the country. In Dar es Salaam, however, CCM now prefers to call its offices tawi, or branch. Originally, I had thought that there
might be some connection between the words maskani and maskini (poverty), both of them being derived from Arabic, but my attempts to demonstrate a connection beyond evocative alliteration have so far proved unfruitful (Fig. 1.6). The word maskani comes from the Arabic maskaan, house or residence, while maskini is related to the Arabic maskiin, which is an adjective meaning pitiable. Whereas the ma- prefix in maskan is placed there to denote the place where one lives (sakan), the ma- in maskiin is part of the original root. Thus maskan has one root, skn, while maskiin has another, mskn.¹

As stated above, the Sheraton Maskani was Mbelwa’s maskani before it became a locus of our research and at the start it was not even clear that it would become so. In the beginning, I usually only visited it in the company of Mbelwa but as time passed I became more and more comfortable with the young men and women who worked there during the day. I began dropping by more often on my own and gradually I came to see that it was on these people and on this place that I should be focusing my attention.

Phases One and Two: Getting started and getting to know

After approximately three months of conducting preliminary interviews with people formally involved in programs geared toward assisting so-called street children and providing information on HIV/AIDS, we gradually shifted our attention in a more focused way toward the Sheraton Maskani. There I came into contact with many different types of people working in the informal economy, including street and food vendors, both male and female, car washers, small business operators, duka (store) owners, guards, and street artists. Many who worked in the more formalized business establishments in the neighborhood often visited as well. Some of those who frequented the corner viewed it primarily as a place of business and thus as their kijiweni. In most such cases, they would also have another place where they would gather for social reasons, most likely closer to their homes. For some, the Sheraton Maskani was only a maskani, and for others, it was both maskani and kijiweni.

A large portion of the second phase of our research period, which lasted about
three months, involved getting to know many of these people on a more intimate level. On average, I would pass by Maskani four or five days a week, some days just to greet people and exchange news on my way to other business in the city. On other days I would sit and talk for a while, eat some lunch, or share the newspapers I usually brought with me. In this way, people there slowly got used to my presence while I grew to understand some of the important issues in their lives, and the language used to frame them.

During this second phase of our research we expanded the focus of our interviews to include individual artists and performers contributing to the production of urban popular culture. To this end, we talked with musicians, painters, radio disc jockeys, comic strip artists, and numerous members of the Dar es Salaam Rastafarian community who were involved in businesses selling paraphernalia loosely described as “culture,” including music, hand-beaded jewelry and other handicrafts. I had intended to include popular culture, and its producers as subjects of my study from the beginning, but it took several months to identify, how to build sufficient contacts to undertake this portion of the research.

The last twenty years in Tanzania have been marked by rapid political and economic changes that have also helped to bring about large-scale social and cultural changes. This is particularly so in regards to the production of popular arts and music, which in the past had relied almost exclusively on the socialist government for patronage. In recent years patronage has been sought out in other arenas, most notably from NGOs and international donor agencies. At the same time, however, artists have become more concerned with the tastes and desires of local audiences, especially as their role as paying consumers of art and music becomes more evident. Along with the economic liberalization policies that had come to Tanzania in the late 1980s, there was also a relaxation of state-sponsored policies that controlled artistic and media production. Since the mid-1990s, there has been an explosive rise in the production of music, television, theater, and graphic arts, along with the emergence of several independent television and FM radio stations, and a free and vibrant press. When conducting my research I found that the information gained from interviews conducted in relation to the emergence and production of new and changing forms of popular culture helped to shed light on many of key ideas that arose from our work with the young people who are the primary consumers of it. With various sections focusing on language, music, painting, street art, and fashion, nearly every chapter of this book reflects the fruitfulness of this avenue of exploration. In Chapter Three I discuss both practical and theoretical issues related to the role played by popular culture in this research.

Phase Three: “Life stories” from five groups

With five months remaining in my research schedule I began to think about the best way to conduct more formalized interviews among the young men
and women who were supposed to be the subjects of my study. Until this time, our conversations, while enlightening, had been very informal. I had not recorded and/or transcribed any of our discussions, and aside from the occasional scribbled note on language use and the journal I was keeping in the evenings, I had very few notes.

After careful thought and much discussion with Mbelwa and some of the people with whom we hoped to work more closely, we finally settled on a plan. Among the many and varied people who passed through the Sheraton Maskani everyday I began to identify certain core groups of people who were engaged in the same types of work, specifically car washing, street vending, and food preparation/vending. For the third phase of research, I decided to try to form three different focus groups at Maskani, based on the income-generating activities of the members of each group. Those who we approached and invited to join one of these three groups had, for the most part, already demonstrated interest in the research during the preceding months. After discussing the format of the proposed interviews with members of each group, it was decided that each group would consist of four members plus Mbelwa and myself (Fig. 1.7). We would meet once a week to discuss a series of issues that we agreed were important for understanding the primary concerns of urban youth. The discussions would last approximately two hours and be recorded and it was agreed that, during the research period, Mbelwa and I would keep the contents of each group’s discussion confidential. Of course, there was no guarantee that the other members of the group would observe this confidentiality and it is my belief that there was rarely anything discussed that could be considered private.

I have given a great deal of thought regarding confidentiality in the writing of this book. With the exception of public figures and those who have expressly requested otherwise, I have chosen to use only first names or nicknames of the individuals who took part in this research in order to offer them some degree of protection. One of the main reasons I do not use pseudonyms is out of respect for those with whom I worked. All of those who agreed to take part in the third phase of the research expressed the desire to be recognized. Pseudonyms, I believe, serve to efface identity, and a significant part of this project involves the recuperation of identity for individuals who are routinely marginalized in their own societies, as well as in academic literature. Not wanting to further ossify the identity of the individuals involved in this research as “street children,” or “AIDS victims,” I attempt to unite with
them in their struggle against anonymity and name them in this text. As a result of this choice, there are many things that I decided not to write about because I believed they would either serve to put those involved in the research at risk or contribute to the further exoticization of their lives. So, while many of them were honest enough to share intimate details about their sexuality, their forays into the illegal, and their drug addictions, I will not reproduce these details in this text. This sometimes results in writing that may seem too general and lacking in detail. I have struggled to find a balance between the general and the specific that will satisfy the academic desires of the reader while still paying respect to those whose stories figure so prominently in this work. When I was in doubt I tried to err on the side of respect for the people without whom this book would have been impossible.

The reader may also be surprised by the limited number of direct quotes supplied in this text given how heavily its arguments claim to rely on exegetic material. This results, in part, from the fact that many of the discussions undertaken for this research were not recorded. Many of the stories I present in this text are summaries that result from the union of various fragments, which might have been told over a period of several months. At times, different fragments of the same story would be provided from several different speakers and it was only through transcription and Mbelwa’s helping to fill in the gaps that I would begin to fathom the outlines of a larger narrative. In light of these constraints it seemed best not to attribute direct quotes to people unless they had been recorded or written down at the time of the interview.

In general, people told stories to illustrate their points and to further my understanding of the issues being discussed. This narrative style, what Tanzanians refer to as *kupiga* story, largely defined the format adhered to during most sessions. Many of these stories are about efforts to pursue a meaningful life, *kutafuta maisha* in Kiswahili, meaning to search for life. In certain respects, they are “life stories,” but they were never elicited as such. Although there is a great deal of anthropological literature available regarding the gathering and use of life stories as a methodology, I must confess that I was largely unfamiliar with it while conducting this research. The primary reason I chose to structure interviews around personal narratives about the pursuit of a meaningful life was that I had grown familiar with this particular genre of storytelling during my time in Tanzania. I would not have known how to elicit a “life story” per se from the people with whom I worked, that is a narrative starting at birth and continuing into the present, but it made perfect sense to ask them to tell stories (*kupiga* story) about their daily struggles (*kutafuta maisha*).

In many ways, I considered all of those who eventually agreed to participate in these group discussions as field assistants in their own right. For this reason, I had only to pause briefly when they asked me if they would be compensated for their time. Their argument, which was a sound one, was that they would have to take time off from work in order to talk to me and would there-
fore be forfeiting a portion of their daily income. Whenever possible I tried
to schedule interviews during the times of day that were thought to be less
profitable for those I was interviewing but the fact remained that most con-
sidered themselves “on the clock” when they were at Maskani. In the end I
decided to pay each person who participated in the interviews a sum we had
come to know was equal to the amount one would have to earn to get off the
streets for a day and into a guesthouse. Not everyone who worked with us
had this objective in mind, but it seemed a fair sum to offer, and no one
turned it down. 11

As it became clear that Maskani would serve as the focus of our study, we
began visiting a second site where some young men from Maskani lived, and
where others visited when searching for things the street could not provide
for them. This area, known locally as Uwanja wa Fisi, or the field of hyenas,
is the primary subject of Chapter Seven. Departing from the focus on the
subject of work, which in many ways frames everyday life at Maskani, this
chapter, along with Chapters Eight and Nine, explores ideas related to leisure
and pleasure. It took several months of visiting Uwanja wa Fisi once a week
or so to feel comfortable selecting it as a site of focused research for the
third phase of our work. Our primary contacts were people living in the area
who largely involved in illegal income-generating activities including prostitu-
tion, drug dealing, gambling, and the sale of homemade alcohol. One of the
greatest difficulties of conducting research at Uwanja wa Fisi was the high
degree of transience among those with whom we worked there. After sever-
al failed attempts and a fair degree of frustration, though, we were able to
identify a core social group centering on the household of one young man.
Eventually, we settled into a pattern of conducting group interviews in his
back courtyard with his girlfriend, brothers, friends, and customers, some of
whom we met there and others who were invited to join us. These group
interviews were conducted along the same lines as those at Maskani, meet-
ing once a week for two hours of recorded discussion for which those who
participated would be paid.

Along with the four groups already mentioned —three at Maskani, and one
at Uwanja wa Fisi — there was a fifth focus group that we also met with once
a week during the third phase of research. This group was only tenuously
connected to Maskani through a friendship between a few members of the
group and Mbelwa and, through him, to a few others at Maskani. A couple
of them, having been born in Dar es Salaam, had been to school with Mbelwa
when they were younger. Their primary link, aside from living in the same
neighborhood (Kijitonyama) and being friends with one another, was that they
were all addicted to heroin, something that is increasingly being experienced
by young people in African cities throughout the continent. The young men
in this group came from middle class families and had more schooling than
most from Maskani. Their parents were mainly civil servants and shop own-
ers. In these respects, they were not unlike the majority of male heroin
addicts in Tanzania. In general, there seemed to be a greater degree of dis-
illusionment among young middle class male Tanzanians - especially those born in the city - than among those coming from less privileged backgrounds, an issue which I will address at greater length in Chapters Four and Nine. Of the young people, with whom I finally decided to work in Dar es Salaam, the members of this group were the only ones to approach me (via Mbelwa) and request that I meet with them, witness their lives, listen to their stories, and in some small way try to improve their lives.

In the end, there were several reasons why I chose to work with the group of young men from Kijitonyama, not least of which was my being flattered by their request. Since I had arrived in Tanzania I had seen and heard a great deal of evidence pointing to a rise in both heroin trafficking and use in Dar es Salaam, including at both Maskani and Uwanja wa Fisi. One of my initial justifications for deciding to include them in my study was my hope that perhaps they would provide middle class and more educated views on the topics we were discussing at the two other research sites.

I had tried to solicit middle class perspectives through the administration of surveys to students at various advanced academic institutions in Dar es Salaam, but found that the information gathered in this way was not very useful. One of the interesting things that did emerge from those surveys, however, was that when asked to list the top three problems faced by youth today in Tanzania, drug use was almost always listed as number two, preceded by unemployment and followed by AIDS. In itself, this was enough to convince me of the need to try to discover more about heroin use in Dar es Salaam and to decide to work with the young men at Kijitonyama despite any reservations I might have. I was confident that the focus groups from Maskani and Uwanja wa Fisi would provide much insight into issues relating to unemployment and AIDS, the other two significant problems most often identified by young people, but I suspected they would not be able to provide much information about the growing heroin trade within Dar es Salaam.

**On Translation and the Ways Meaning is Made**

Following our interviews with each of the groups, Mbelwa and I would sit down together and listen to the tapes again during which time he would translate things that were unclear to me while I frantically took notes in English. Often words or phrases would come up that would be difficult to translate. In these instances Mbelwa would tell me, “I can’t translate this, you’ll just have to learn what it means in context.” Insisting that the words had a meaning deeper than was possible to convey through translation, he would instead go to great lengths to explain how people used such words, the contexts in which they came up, and how and when they had become popular in the first place. I would be lying if I said that I did not often find his refusal to translate frustrating. More importantly, however, I found it didactic. It certainly did not stem from an ignorance of English on his part or a lack of ability, but rather from his deep respect for language in general and Kiswahili in partic-
ular. His profound insight into the difficulties of translation combined with my somewhat awed reading of Walter Benjamin’s (1968) essay on the subject served as a constant reminder that translation is more often a poetic and political matter than a scientific one. Problems of translation, of course, extend beyond semantics to semiotics. Understanding how meaning is made remains central to any ethnographic undertaking, and I am forever indebted to Mbelwa (and Benjamin) for an idealism that reinforced my own belief that one should always try to get it right, even when knowledge of the impossibility of such an undertaking is staring you in the face.

**Representation: Not your average AIDS ethnography**

When I left Tanzania in late August 2000 to begin writing my dissertation, I was plagued by doubts regarding representation. In part this stemmed from a series of rather traumatic insights I gained preceding my departure from Dar es Salaam, and more generally from the trauma of conducting research and forging relationships with people living in difficult circumstances.

In the weeks before I left Tanzania, I was forced to recognize some of the very real difficulties faced every day by the people with whom I worked. This particular period of enlightenment was initiated by the death of a young woman named Rehema whom I had never met, but with whom I had become acquainted through some of the stories I had heard. In short, she took her own life when it became impossible to conceal the fact that she was dying from AIDS. She had formerly engaged in sexual relations with some of the young men I knew from both Maskani and Uwanja wa Fisi and her death caused many of them to pause for reflection, as it did me. I return to her story and the specifics of what it meant for me and my research in Chapter Eight, but mention it here in order to draw attention to the time when I first became concerned with the difficulties of representation.

Following the interview in which Rehema’s death was first discussed, I began to feel increasingly depressed by the very stories I was collecting, in many ways mirroring the feelings that Mbelwa had encountered when he had attempted to conduct his own research among the young men. No doubt, my feelings of demoralization and sadness were heightened by the fact that the end of my research was drawing near, and the futures of relationships I had developed in Tanzania, which were already unclear, were made increasingly so when I was made to realize how likely it was that many of those I came to know were already, or would soon be infected with HIV.

According to official statistics, their chances of becoming infected hovered around 40 percent, but according to my research, infection was almost certain. How was I going to take their stories, which formed the heart of my research, and “represent” AIDS in Africa? How was I going to reconcile the numbers with the stories? I began to realize that those who entrusted me with their stories had also handed me the mantle of witness, and I was not sure I would be able to convey the connections between everyday struggle
and a global pandemic. How was I ever going to do justice to either the sto­ries or the people? I began to wonder if I would be able to impart to the read­ers of this text the same sort of mimetic understanding I had come to trust. I kept asking myself, would the stories, after all, be enough?

It is estimated that there are 25.3 million active cases of AIDS in Africa today, and already 15 million deaths in Africa alone.\textsuperscript{12} How can we even begin to imagine 15 million deaths? And, more importantly, how can we imagine each and every one of those deaths as that of an individual human being with a name, face, and most significantly, a story? To a shocking degree, AIDS and AIDS deaths have become commonplace, even banal. How to re-ignite interest in a topic that everyone professes is important, but which also induces feelings of hopelessness? During my time in Tanzania people agreed to share some of the most intimate aspects of their lives with me. The stories they told and the supporting materials I gathered were richer than anything I had imagined possible, and yet I wondered if it would be enough to counter the feelings of helplessness and hopelessness that work togeth­er to produce what is commonly referred to as AIDS fatigue or boredom.

From this position of hopelessness, I set out to write an AIDS ethnography I thought was destined to fail. Although I had discarded many of the meth­ods employed by those who had written AIDS ethnographies in the past, I somehow still expected my writing to resemble theirs, at the very least to the extent that AIDS remained the focus of the book. Over several months of writing and presenting chapters at workshops, it gradually became clear to me that if I wanted this book to reflect the research I had conducted, I would have to abandon the notion that AIDS would be the central topic. I kept returning to the stories. In fact they haunted me and, while AIDS was a topic often broached in the stories, it rarely stood on its own. This does not mean that AIDS is not a primary focus in the book, but rather that it is not the organizing theme around which it is built. This should hardly be surprising given that I had not centered my research on the topic of AIDS and that it was not the most important issue for those with whom I was working. Yet it took quite some time for me to realize and accept that this book would primarily be about the lives of young people living and working in the streets of Dar es Salaam for whom AIDS was just one of several important aspects of their lives. I have tried to fit AIDS into this book in ways similar to the ways which those who I came to know in Tanzania fit AIDS into their lives. It is always there in the background; knowledge of its existence informs and affects deci­sions made in daily life, but it is not an omnipotent force around which lives are centered. As a result, there are some chapters in which AIDS is dis­cussed at length, and others where it is hardly mentioned at all. And yet, it seems to me that the entire project and in many aspects this book, as well, are haunted by AIDS. It is a spectre looming. Poverty is caused by and is a cause for it, as is a whole host of related social ills. In the end, I may not be able to disentangle its possible meanings any better than do the people with whom I worked. Yet I hope that what I do offer will help to shed light on the
predicaments faced by disadvantaged urban youth in Dar es Salaam and throughout the world.

Phase Four: A space for place and a quick return to Dar es Salaam

If there is one theme that runs throughout this book it is that of place, a theme whose importance I hardly realized until I returned from Tanzania and began examining my research materials and thinking about how it all fit together. As stated above, my research was specifically em-placed at a particular street corner in Dar es Salaam. Almost all of the work I undertook occurred there at the Sheraton Maskani or at another gathering place like it in the city. The transformation of public spaces into private places of work and leisure, and what such transformations might signify to those undertaking the transforming, was easily the most common theme in both the informal discussions in which I engaged and in the more formal interviews. If anything is important to young people living and working in the streets of Dar es Salaam, it is the contestation of public space, a contest that is played out against both local and national governments, other citizens of the city, and even others who are trying to survive in the ever-narrowing spaces where informal economic activities are permitted.

It was only after several months of intensive research and several failed attempts at collecting data that were more quantitative in nature that I began to realize that people were most likely to talk about difference, especially in terms of economic and political power, through reference to place. Sometimes these references were clear-cut: poor people live in Uswahilini, literally “the place of the Swahili,” while wealthy people live in Uzunguni, literally “the place of the foreigners (Europeans/Whites).” But, even such a seemingly straightforward dichotomy may evoke all sorts of complex economic, political, social, moral, and historical implications. Places do not come into existence naturally, they are actively imagined and constructed by their inhabitants and the people who govern them, entities that may or may not be at odds with one another.

I was unprepared for these insights. Particularly in the sense that I had read little of the rich literature on the anthropology of place and urban geography. This limited the degree to which I could question and test the theoretical points laid out in those works while in the field. At the same time, I think it also forced me to make the effort to understand the ways notions of place come into play in Dar es Salaam on their own terms. Whenever possible, I prefer to try out my theories in the field, giving people the chance to respond, and if so desired, offer countering views. With this in mind I embarked on the fourth phase of my research and returned to Tanzania for a brief research visit in January and February of 2002 to solicit what I had begun calling “popular cartographies.”

The general idea was to ask people to draw maps, or pictures, of the places they were so good at telling stories about. The task itself was easy enough
to arrange, as most of the people with whom I had worked were still living in
Dar es Salaam (despite my fear that many of them would be sick or dead)
and willing to help out in any way they could. In just a few weeks I managed
to gather thirty maps, most of which failed to meet my expectations. It was
not that people did not try, only that were better at articulating their views
about space and place linguistically than graphically. I must admit I was
rather surprised, as I knew of at least two other examples where a similar
method had been used among young people in Africa (one in Tanzania and
one in South Africa). In both instances the mapping projects were consid­
ered a success. In those cases, however, the purpose of soliciting the maps
was to initiate dialogue on sensitive subjects among people otherwise disin­
clined to talk while, in my case, I was hoping to shed new light on subjects
people were already quite willing to talk about. Regardless of the outcome
of the mapping project, I consider the time spent on it well worth the effort,
as the stories people told prior to, during and after making their drawings
reinforced my notions about the importance of place in the way they make
meaning in their lives.

For the mapping project, I decided to work with a different assistant since
Mbelwa was engaged in another project. Athumani Amiri, the young man who
helped me, was a good friend who had figured rather prominently in my prior
research and, although he did not speak English nor have the experience
Mbelwa did, he dove wholeheartedly into the work. One of the first problems
we encountered was in regards to terminology. We found that when we used
ramani, the Kiswahili word for map, we would almost inevitably get maps that
looked suspiciously similar to those given out by the Dar es Salaam tourist
center. People seemed surprised to learn that I had been hoping for some­
thing more imaginative. I soon realized, however, that if I used the word
picha, for picture, rather than ramani, I would get renderings that were more
graphically rich, containing the sort of details missing from more formalized
ramani, such as fences, individual houses, people, and, most significantly,
trees. I initiated this phase of the research to determine the extent to which
I might have misaligned Western theoretical notions with the perspectives
offered among those with whom I had worked most closely. Despite the
degree to which my theorizing on place remains dominated by Western social
theory on the subject, I believe it also remains reflective of the ideas put forth
by those who worked with me on this project.

What This Book Is About

In the end, this book is about the lives of the young people in Dar es Salaam
who guided me during my stay. More specifically, it is about the ways they
attempt to make meaningful lives for themselves in an environment that
seems to be becoming increasingly uncertain as a result of economic and
political changes in Tanzania, HIV/AIDS, and heroin, among other problems.
It seems that perhaps more than ever, the pursuit of life in urban Africa leads
to the direction of early death. I opened this chapter with a reference to a song from Remmy Ongala, a musician born in what was then Zaire who has for many years made his home in Dar es Salaam. Ongala sings about life, maisha, suggesting that life is a nice house, nice clothes and good food, the very basics of survival that characterize the aspirations of so many young people longing to make their way in the world. These seemingly banal observations were constantly echoed by those with whom I worked, forcing me to recognize that as obvious as such statements might initially seem, it is important to examine them closely if one hopes to come to a better understanding of how people make meaning in their lives.

This book is organized in a series of chapters that are roughly paired. Chapters Two and Three grapple with the historical data and theoretical concepts that frame this book, with Chapter Two focusing on issues related to the emergence of contemporary (post-1985) class structures in Dar es Salaam, and Chapter Three focusing on a theoretical discussion on space, globalization, and popular culture. Chapters Four and Five respectively look at peace and violence as social constructs. Both rely heavily on data collected at Maskani but are also informed by material gathered throughout the city. Although these chapters are specifically concerned with understanding how discourses of peace and violence shape the social space of Maskani, they are more generally concerned with elucidating the dialectical relationship between these discourses. Chapters Six and Seven are concerned with issues of transience and locality. Chapter Six relies mostly on data gathered from the street vendors who worked at Maskani and Chapter Seven on data gathered at Uwanja wa Fisi. Chapter Seven also departs from the previous chapters in that it introduces a discussion on leisure and play. Most of the earlier chapters are shaped by discussions on work. Chapter Eight continues in this vein while focusing on the connections between popular language and culture, and between desire and sexuality. This chapter also attempts to provide a space for female viewpoints in this text, which is otherwise marked by its male bias. In many ways, Chapter Nine can be paired with the third chapter in that it returns to questions concerning the relationship between the local and the global. This chapter, which focuses on the growing use and trade of heroin in Dar es Salaam, is primarily shaped by the interviews undertaken at Kijitonyama. Some overall remarks and a return to the question of AIDS are provided in a brief Afterword.

Through an examination of specific spaces and/or places, I hope to illustrate how ideas related to modernity, globalization, violence, and desire are enacted locally. The choice of these four factors is not arbitrary. I focus on them mainly because they are the underlying issues that tie together the central concerns of young Tanzanians today: lack of employment opportunities, increasing drug use/abuse, and AIDS.
notes

1 In making this distinction, I refer the reader to Veena Das' (1999) and Arthur Kleinman's (1999) Daedalus articles on the subject.

2 Mary Poovey (1998) has eloquently drawn a connection between the birth of statistics and the birth of the post-modern fact. By this, she makes reference to the effacement of individuality that results from the production of statistics. It is against this very effacement, specifically of the millions infected with HIV, that I put forth this call to “face” AIDS.

3 This project was reviewed by the National AIDS Control Program and the sociology department at the University of Dar es Salaam before research clearance was granted for it by the national authorities at the commission for science and technology (COSTECH). With this clearance I was able to undertake my research with very few difficulties. I was never asked to present documentation of such clearance when working on the streets of Dar es Salaam, but was required to show it before being granted interviews in certain governmental agencies.

4 See Singer (1994a; 1994b) for discussion of what he calls the AIDS Syndemic.

5 Theoretical views for this approach have been influenced by my interpretation of Antonio Gramsci’s ideas regarding organic intellectuals, and continually inspired by Zora Neale Hurston’s Mules and Men, in which she masterfully succeeds in simultaneously elevating her informants while effacing her own role as a producer of knowledge.

6 This “mode of inquiry” was, of course, inspired by James Clifford’s recounting of Renato Rosaldo’s statement that “deep hanging out” was what made anthropological ethnography distinctive (1997:56).

7 Kasheshe is a difficult term to translate. I have been told that it is a term borrowed from Kihaya and that it generally refers to the sort of hassle and hustle that is sometimes associated with urban life. The city is not, however, considered to be continually in this state. Kasheshe is most often used in association with individual incidences and outbursts.

8 I thank Anouk de Koning and Mustapha Mohammed for helping me to understand the “non-relationship” between these two terms.

9 The types of informal conversations to which I refer bear striking similarities to those outlined by Dipesh Chakrabarty (1999) in regards to adda in Calcutta, specifically so in relation to links between informal discussions and place (i.e., maskani) and to the degree that such discussions are considered essential to a specific culture.

10 See, for example, Linde (1993).

11 Most of those we worked with reported earning on average between 2000 and 5000 shillings a day (about 3 to 8 USD at the time of research), so we decided to offer each “assistant” 3000 shillings for a weekly 2 hour interview. Along with this amount, which was paid at the end of each discussion, I would, of course, be expected to stand a round or two of drinks.

12 UNAIDS Surveillance Report (2000a). A 2002 UNAIDS update for Tanzania suggests that AIDS rates continue to rise in that country. It should be noted, however, that the numbers provided are estimates based on sentinel studies mainly carried out among pregnant women, sex workers and tuberculosis patients. See Setel (1999) for
discussion on the reliability of AIDS statistics in Tanzania specifically and UNAIDS (2000b) for a more general discussion.

13 Research relating to these projects has not been published to my knowledge. The one in Tanzania was undertaken by TANERA (Tanzania Netherlands Research) in Mbeya (see web site for further information) and the one in South African among gay men in Johannesburg (Leap 1999: paper given at University of Iowa anthropology colloquium).