Aspirations and sex: Coming of age in western Kenya in a context of HIV

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Chapter 2

Methodological concerns:
“What makes a useful contribution to the fight against HIV?”

1. Introduction: Arriving

Looking out of the window of the plane, I already could feel the heat and I could certainly see it: the shimmering air above the ground, and the plains and hills coloured in the dry yellow of foliage and fields that have not seen rain in quite a while. What a contrast to the deep green surrounding Nairobi, which I had left behind just 45 minutes ago! Descending rapidly, the plane turned, and there was Lake Victoria (Nam Lolwe in DhoLuo), the largest lake in Africa, and the largest tropical lake in the world. I would come to learn about the tensions that the lake represents: life and death, calm tranquillity and brute force, livelihood and economic decay, natural wonder and pollution, and even national pride and cross-border cooperation.

Safely on the ground after a rather scary landing, in which the plane glided awfully close to the water before touching down on the runway, I made my way out the front of the plane, where the heat hit me as if I had stepped into an oven. I waited with the other passengers, about 50 people, for our luggage to be unloaded, standing in the sun next to the small building that serves as the terminal for the single-runway airport. Then, happy that all my things had arrived after the long journey, I made my way through the gate where a crowd awaited the arrivals. My heart stopped for a split second and I winced (probably visibly): there was a person holding a sign with my name on it, and, in fat, bold letters, the word ‘CDC’, with an official logo.

I then noticed the car: a new, air-conditioned, white 4x4 Subaru station wagon. Leaving the airport, this shiny, metallic vehicle appeared to me as if it was from outer space, such a contrast as it was to the boda-bodas, the beautifully decorated Chinese bicycles, used as bike taxis that crowded the potholed road all the way to town. The driver of this big CDC vehicle was Jorge, a friendly middle-aged man born in Nairobi. We passed through Kisumu’s main road with its three banks, a handful of local restaurants, two cyber-cafes, one post office, two opticians, two pharmacies, and crowds of street vendors, driving alongside the boda-bodas and the more upmarket tuk-tuks (three-wheel, motor-powered rickshaws imported from India and China). At the end of
the street, we turned and drove up a small hill and into Milimani, Kisumu’s upper-class neighbourhood, with its beautiful colonial villas, lush, green gardens, and the town’s two best hotels. We stopped at a high wall and the gated entrance to my colleague’s house, in the direct vicinity of the State House, the regional residence for the Kenyan president.

At the gate, a small window opened and a guard inspected us. He then came out, fully attired in a black army-style uniform, bulletproof vest, and holstered gun. He inquired about my identity and the purpose of my visit, and disappeared for what felt like a very long time. Finally, the gate opened and three German Shepherds barked at our car. Behind them was a big one-storey house, surrounded by a beautiful garden. My colleague came to greet me and, after Jorge helped us with my bags, she brought me to one of their bedrooms and invited me to take a shower. I tried to chat with one of the maids, but she was shy and ducked away. Once I was refreshed, I tried to find my way back, but I was a bit lost in the big house with its many bedrooms. I finally ran into another maid who brought me to the kitchen, where a man who I assumed was my colleague’s husband was busy reading the newspaper. He briefly looked up and greeted me with a faint ‘hello’.

I waited awhile until my colleague resurfaced and brought me to the nearby Hotel Nyanza Club. It turned out that the hotel check-in began at 11 a.m., which was why I had had a few hours to spend at my colleague’s house. The Nyanza Club is the local watering hole for the CDC’s visitors and expats living in Kisumu, who come there for dinner or their children’s swimming and tennis lessons. For me, the Nyanza Club was a colonial kind of place that lacked any connection to ordinary life in Kisumu. I looked forward to finally meeting ordinary Kenyans.

Being part of a medical research team that was carrying out a development project in Winam quickly introduced me to the ‘expat’ way of life. It would have been easy to become part of that world, with its luxury and security. But if I wanted to obtain an in-depth understanding of the daily life of young Kenyan people in order to contribute to the HIV/AIDS prevention, I had to find a different way of setting up my research.

As anthropologists it is worthwhile to ask, as Bolton (1995) urges, whether what we contribute is useful: Does our research further the struggle against the HIV/AIDS epidemic? Throughout my fieldwork, I asked myself this question quite often. At the onset of the HIV/AIDS epidemic, epidemiologists dominated HIV research, while anthropologists were rather slow to respond. But in recent years, and certainly during
the writing of this dissertation, many anthropologists have contributed to a more holistic and contextualized understanding of the HIV/AIDS epidemic. In this chapter, I explain how anthropological methods can make unique contributions to both HIV/AIDS research and interventions. Despite the difficulties I encountered as part of a ‘multidisciplinary’ research team, I strongly believe that anthropologists can engage constructively with quantitative researchers, and that we can help improve HIV-prevention efforts when we deliver high-quality ethnographic, qualitative data.

My principal research method was participant observation, the basic research practice of anthropology. Fieldwork was carried out for a period of 20 months between 2005 and 2006, with preparatory field visits in September 2003 and March 2004. I returned to the field for two months in June 2007 to verify my data. During my fieldwork, I mainly used participant observation as an anthropological method to gain insight into the context of Winam; the organisational structure and the activities of Yeshica, the community-based component of the HIV/AIDS Youth Intervention Program (YIP) in Winam; and to capture the dynamics of the interactions that took place between different people, especially the young people of Winam. From March 2005 until October 2006, my boyfriend (now husband) Sven and I lived in a rented two-room house that was part of the compound of Mama Daky, a community health worker who had worked for the CDC since 1984. We were living in a neighbourhood where many youth gathered and lived. Our room was situated near the busy market centre of Dhonam, behind a primary and secondary school, and close to the fishing beach of Dhonam. Through spending a lot of time with the youngsters and sharing in their daily life activities together with my research assistant Petronella, it was possible to get a rich understanding of their sexual relationships and their livelihoods. To complement this research method, I also carried out informal and formal in-depth interviews (including family genealogies), collected life histories, organised focus group discussions and read one young man’s diary. The strength of my research methodology was founded on the triangulation of multiple qualitative methods.

2. Methodological approach

My fieldwork took place under the auspices, and was but one portion, of a large, PEPFAR-funded YIP, a collaboration between three medical institutes: ITM, the CDC, and KEMRI. The YIP was led by Belgian epidemiologists, including the program coordinator who was based in the city of Kisumu (two hours’ drive from Winam) and my
In September 2003, my ITM supervisor, the YIP program coordinator, and my predecessor, a Belgian anthropologist who formerly worked at ITM, organized for me to visit the field site, as a way to orient me and to introduce me to a number of people who could be of importance during my fieldwork. Since my employment at ITM coincided with the start of the implementation of Yeshica, I was able, in that initial visit, to observe the setting up of Yeshica, and to have conversations with some of their first youth participants. Six months later, in March 2004, I returned to Winam for one month, staying in the house of one of the local field staff of Yeshica, which was located at the Yeshica compound. During that period, I obtained a good impression of Yeshica participants and staff, its organisational culture, and the work of the CDC in collaboration with KEMRI. During these early visits, I realized that conducting research on a topic as sensitive as sexuality, in a locale where the CDC has conducted a great deal of research, and, moreover, an area that is dominated by medical research in general, could have an impact on my research findings. With this in mind, I realized that my approach had to be different if I wanted to gain access to people’s everyday lives, namely, I needed to distance myself from the CDC, live in the village, and find ways to build trust with local residents.

2.1. Distance and independence

When I mentioned the name of the village where I was planning to carry out my research, it was immediately obvious to Dr. Onyango-Ouma, an anthropologist at the University of Nairobi whom I interviewed before beginning my fieldwork that I would be working with an affiliate of the CDC that was known for its medical research in Nyanza Province. The best advice he gave me was “Do not make use of the CDC transportation”, knowing that simply being seen in the CDC vehicles would mark me as part of the medical establishment, and thwart any attempts to establish rapport with the people of Winam.
I had already realised during my preliminary research that JoWinam (people of Winam) had a substantial distrust of CDC research programs. In order to collect good, valuable data, I realised I had to make clear that my research was different from the CDC’s medical research—and that maintaining distance from CDC and YIP was key to doing so. Moreover, since one of my objectives was to analyse the perceptions and practices of young people in order to find out why only some youngsters participate in Yeshica’s programs, it was important to distance myself from Yeshica’s implementation team.

Alongside maintaining distance from the staff of the CDC and Yeshica, I also tried to minimize the use of the CDC office in Kisumu and to avoid the use of the CDC cars that commuted between Kisumu and Winam. On a daily basis between 20 and 30 white Toyota Land Cruisers with diplomatic license plates travelled the earthen roads from Kisumu to the rural area and back (a two-hour drive), with medical researchers or foreign visitors on board. Sometimes the cars contained only a driver, sent to pick up some material and to bring it back to the city. Locals complained that they could not get a ride in these vehicles—not even the urgent cases of AIDS-infected patients who needed to be brought to the hospital—while the vehicles were driven empty. Empty cars passed local residents at high speed, and, with understandable feelings of animosity, they are left behind with plenty of time to ponder the purpose of all these vehicles while they walk to their destination. Since I wanted to socialise with the people of Winam, I travelled on the local buses back and forth between Winam and Kisumu. In this way, I also quickly realised how mobile young people in Winam are and how mobility is used as a means to improve their livelihood. This observation became an unforeseen but important topic of interest, one that I had not included in the initial design of my research. This is one of the benefits of the anthropological method—observation on the ground allows one to refine research questions in real time.

2.2. Entering village life

Being white and an outsider caused the locals to suspect that I was with the CDC, as no other organisation—even not the handful of local churches who usually have Kenyan pastors and, sporadically, white visitors—is so active in the area. It was the first issue I had to deal with when introducing myself to the inhabitants of Winam. People were also shocked at first, seeing me walking around rather than sitting in a car. They yelled to me from their homesteads that I should take care because my feet would get hurt, since they assumed I was not used to walking around. Their concern was genuine, and
everyone was friendly and curious. As I walked along country paths, every person I passed approached me to say hello. Whenever I went to buy something at the market, it took at least an hour due to the constant need to stop and converse. Greeting is a very important aspect of social life in Luoland (Cohen and Odhiambo 1989). JoWinam asked me the typical questions: “Idhi nade?” (How are you?), “Ichiew nade?” (How did you wake up in the morning?), and “Idhi tedo an’go?” (What are you going to cook?). Before waiting for an answer, locals joked that they were planning to visit me, so that they could enjoy my food. People loved to hear me talking in their local language, Dholuo, for which I had taken classes in Kisumu, and it really helped me to communicate with them and to win their sympathy.

Everyone I met wanted to know where I came from and what I was doing in Winam, which gave me the opportunity to introduce my study in nearly every initial conversation. Right from the start I decided not to spell out in full the purposes of the research and all the procedures to be employed to everyone involved in the research. I was sure that doing so would influence their behaviour in such a way as to invalidate the findings (Hammersley and Atkinson 1995: 72–73). As the people of Winam are over-researched, and thus wary of any form of research, I decided to leave out what seemed to me to be the more delicate aspects of the study until trust had been established. As Hammersley and Atkinson (1995) explain, unless one can build up a trusting relationship relatively rapidly, people may refuse access in a way that they would not do later on in the fieldwork (Hammersley and Atkinson 1995: 72–73).

I was not surprised to find out, right at the start, that people who otherwise hardly spoke English were familiar with the English words ‘research’ and ‘investigation’, even though the real meaning of the words was not clear to them. They mostly asked me as a way of introduction if I came to Winam to carry out some research for CDC. The way they asked me, sometimes with a clearly suspicious voice, made me realise their fear of another survey or CDC randomised-control study in Winam. Their fear was associated with their distrust in medicine (see next chapter). I began by explaining that I was carrying out a study for the University of Amsterdam (UvA) and the Institute of Tropical Medicine (ITM); I told them that I was collaborating with the CDC people at Yeshica. Secondly, I emphasised that my study was different, compared to other studies that have been carried out in Winam, because I was not taking urine or blood samples. Finally, I explained that my study was about understanding how young people live today—learning about the problems they face, what their needs are, and what is important in their lives. After this introduction, most immediately started to tell me
their thoughts about the daily life of young people, and invited me to visit them in their house.

My boyfriend Sven joined me in Winam during the longer period of my fieldwork, from March 2005 to October 2006. However, he also regularly stayed in a room we used to rent in Kisumu since we lacked electricity in Winam. Sven was writing his PhD thesis—on a totally different topic—at that time (see Harten 2011) and regularly travelled to his university in London and back home to be with his father in Germany, as his mother had died one month before we left for Winam, in February 2005. Although Sven and I were not married yet, we presented ourselves as ‘husband’ and ‘wife’. Sometimes Sven accompanied me during my walks in the village, which helped people see that we were a ‘married’ couple, and which made a good impression since people respected us. In this way, people were aware that we were engaged and that prevented men from pursuing a relationship with me. The local male youngsters also liked to hang out with Sven, which only improved our relationships with them. Sven was a big support to me when I encountered difficult situations in the field, including my difficulties explaining my methods to my medical colleagues. Together we took private classes to learn the local language, Dholuo, and he also taught me and my research assistant Petronella, who joined me after six months, how to drive motorbikes.

2.3. Building trust

I carried out the first five months of fieldwork alone, without the help of a research assistant. I spent time at various places in Winam, such as the fishing beaches, and stopped at every place where people wanted to talk to me. I made my first contacts with young people on my own, and it helped me to improve my Dholuo knowledge. If I needed translation help, I asked whoever was around to translate for me, and when going to meetings, I asked a secondary school graduate to join me. I later found out from the Belgian program coordinator that I could get a research assistant who would be employed by CDC/KEMRI.

From the sixth month of fieldwork onwards, I received the help from my female research assistant Petronella, who held a Bachelor’s degree in anthropology. She had applied for the position based on an advertisement I had published in the daily newspapers. I had called for a female Luo speaker who held a Bachelor’s degree in Social Science, preferably in anthropology, and who was willing to spend a year with me in the village. I preferred a female assistant to a male one since during the first five months of my fieldwork I had observed that I found it difficult to get close to the young
women—mainly due to the language barrier (see also later). Petronella was ultimately chosen from a short-list of five female candidates because she had a good understanding of anthropological research methods, had never worked with the CDC before, and came right from university (a plus for me since I wanted the assistant not to be biased or used to certain working styles). Petronella was 25 years old when she started to work with me in 2005. I presented her to JoWinam as my friend from university who was carrying out a study for her Master's thesis, as we believed that one day she might use her experience for her Master's studies (which she did, successfully).

Although she spoke the Dholuo language fluently, Petronella was unfamiliar with village life and 'chike Luo', which translates as 'Luo customs or rules', and means how people structure their daily life in rural 'Luoland', the home of the Luo ancestors. Petronella was born in Siaya District (Nyanza Province) and lived there until the death of her mother in 1994, just around the time Petronella had finished primary school. She moved to Nairobi for her high school and undergraduate degrees. Two years after the death of her mother, her father died and she became an orphan; from that point on she was taken care of by her eldest sister, who was living in Nairobi. Petronella identifies herself as a ‘town girl’ and has not returned to Siaya since 1994. It was only her work with me that brought her back to Luoland. But her inexperience with village life did not hamper our collection of data—on the contrary, JoWinam were very willing to explain anything and everything to her, and in detail. The same happened to me, being a white foreigner, coming from abroad. Petronella and I were treated as students, outsiders who were eager to learn while doing.

The first months while Petronella was still looking around for an appropriate place to stay, my boyfriend and I shared our two-room house with her. Later, she moved within the same compound to one of the rooms in Mama Daky’s house, which was also used for Mama Daky’s chickens, and thus not very comfortable. Finally, she rented one of the first rooms that received electricity in January 2006, situated on the other side of Dhonam market where also other members of CDC staff happened to be staying. Whenever we travelled to Kisumu, we travelled together, and, as Petronella was new in Kisumu and had no relatives to stay with, we also shared our two-room studio in Kisumu with her. In this way, we worked long hours and spent plenty of time together, during which we were able to reflect on different topics related to the field.

I trained Petronella how to carry out participant observation, in-depth interviews, and focus group discussions. It helped that she was fresh from university and that this was her first job. It was not easy to find someone who had not worked yet for CDC/KEMRI in
Kisumu, and who was willing to come and stay in the village. For most people, once a graduate, coming from Nairobi, it is considered somehow beneath your status to live in the village's basic conditions. Petronella understood and realized that a simple dress code was needed in the field in order not to differentiate us too much from the people of the village. Whereas most CDC/KEMRI staff wore high-polished shoes, white shirts, and black trousers, I emphasised that we as anthropologists should wear clothes suitable for every possible situation. We would meet the youngsters who were doing the cooking and cleaning, and we would help them with whatever they were doing; during these times we would have informal conversations. I repeatedly stressed the importance of becoming seen as one of them so that they would share their sorrows, problems, and moments of joy and happiness with us.

Before we started the intensive fieldwork, we introduced ourselves formally to the four chiefs in Winam and attended two barazas (public meetings) where we clearly introduced the purpose of our stay in Winam. After this formal introduction, we walked together all over Winam; later on, we also made use of a motorbike. The motorbikes were the property of the CDC and I felt reluctant to use them at the beginning, for the same reason as it created a distance with the locals. In order to build friendships, I preferred walking and at times biking. For long distances, Petronella and I used the local buses. But when friendships and trust were built, in order to save time—as local buses do not stop frequently, and are usually overcrowded—we decided to take the motorbikes for long distances from the tenth month of fieldwork onwards.

JoWinam did not seem to feel more or less confident with either Petronella or me. The only big difference was that Petronella came from a neighbouring district and was fluent in DhoLuo. My level of DhoLuo was proficient and I managed to understand 80 percent of what was said, but I could not actively lead lengthy conversations in the local language. People appreciated my efforts to express myself in Dholuo and Petronella always did a great job with translating. Despite my language handicap, joWinam were used to both of us, and shared information with us equally. There were only a few young women, whom Petronella had met on her own, who felt more comfortable with Petronella. In hindsight, it was good that I had conducted fieldwork during the first five months on my own, because by the time Petronella arrived I had already established rapport with a good number of people. Some, certainly the female youngsters whom I had first met without Petronella, were a bit suspicious when I introduced Petronella to them. Even though Petronella was dressed very casually, they saw her as a “town girl” and did not know if she wanted to be associated with “village girls”. But once the ice was
broken, Petronella managed to build strong relationships with a large number of young women and men from Winam. Also the youngsters’ caretakers and parents encouraged them to associate with us, because they saw us as ‘good role models’ since we were ‘educated’. Petronella also managed to have close relationships with the local field staff of Yeshica. She lived in the same compound with some of them, and in the evenings they usually ate together and shared what they had done during the day. Although I was often present, I was unable to follow their conversations as they used a lot of Kiswahili and Luo ‘sheng’ (slang). I was confident that Petronella would later write these conversations down in her diary book, which enabled me to follow accurately with what was going on in the field at Yeshica. I am pretty sure that without Petronella’s assistance, I would have not been able to have such a detailed, rich data on both the youngsters and the Yeshica project.

Petronella was a talented research assistant, and we made a good team. She respected my position, accepted my comments, and followed through with my requests. She was my colleague and, except for my boyfriend Sven, my closest friend in the field. Later, we became like sisters to each other. We built a very close relationship from which we both have learned a great deal.

2.4. Dealing with suffering and death

Death and illness were omnipresent in Winam. On our walks through the village we passed many graves of the fathers, mothers, children, and grandparents of the youths I was working with. Every week, several people were buried. In every home there was someone suffering. During my fieldwork, I did not witness the death of any close friend but was continually confronted with the suffering of the affected families. I witnessed quarrels between family members about where to bury the dead, how to pay for a funeral, whether or not to stick to chike Luo rules such as bride inheritance, and more.

In the beginning, I found it difficult, emotionally and ethically, to attend funerals in people’s homes, because usually I did not know the deceased person personally. Mama Daky, our landlord, convinced me that this was actually a good way for people to get to know me, because people truly appreciated my condolences and compassion. Early on, Mama Daky took me to several funerals, since this was one of the main activities in the village between Friday and Sunday. People basically went from one funeral to the other. On a personal level, this was also hard because Sven’s mother had recently died, and each funeral made us remember her, and our loss, time and time again.
Illness seemed to be all around us. It often happened that we visited a home and found a person there showing all the symptoms of AIDS. Sometimes we would find a deathly skinny person sleeping outside the house, but most times they were hiding inside, reluctant to meet anyone. Add to this the usual sight of malnourished children, and people of all ages with malaria, all of them too poor to buy medicine or pay hospital bills. People came to me to show their wounds and illnesses and to talk about their pain. I do not deal well with seeing blood, or hearing in detail about someone's wounds, and my empathy is such that I often remained sleepless after such encounters. But really, no one could remain impassive in the face of so much daily suffering.

We frequently helped pay for the most urgent medical care, and I was glad to help in this small way, but I was frustrated that I did not have more medical knowledge to help people myself. One of the few things I was able to do was to give sick people a ride to the hospital on my motorbike, flouting the CDC/KEMRI rules that forbade me from transporting strangers. The back of a motorbike is not very comfortable if you are deathly ill, but it's better than walking two hours under the boiling sun. I felt that this was simply my moral obligation, nullifying any bureaucratic rule that focused instead on my lack of insurance for passengers. In a way, overriding the CDC's bureaucracy was my silent protest against their practice of racing those empty cars through the village.

Winam is a place that reflects both individual, everyday hardship as well as global injustice and inequality. When one of my academic supervisors came to visit me in the field, she was shocked by the suffering, and by the many people trying to kill their pain and sorrow with alcohol. In my conversations with her, I realized how much I had adapted to this place: the graves, the skinny AIDS victims, the people drunk at 9 a.m.—it all seemed so normal and self-evident to me. “This is just how life is. How could it be any different?” I thought to myself.

The situation in Winam was really hopeless, and perhaps having a return ticket helped me persevere during fieldwork. When I did leave, saying goodbye to the young people was particularly hard. We slaughtered a goat, and together prepared a celebratory meal. It was a lovely and joyful get together, but when we parted I realized that the odds were small that I would find most of them again when I return: I knew that some would have moved in their quest for work, and others surely would have died. I was lucky to be able to return to a safer, healthier world, with its own problems but still with basic security and many more opportunities.

While writing this dissertation, I maintained contact with a number of youth from Winam. During fieldwork I established strong relationships with some of them,
and we are still in contact thanks to the mobile phone. The situation of some of those youngsters has already deteriorated. This shows how fragile their livelihood opportunities are. For Petronella, however, it was not feasible to remain involved with all of the youngsters, and to continue following their concerns and aspirations. Her situation was different than mine: she remained working in the field of medical research in Kisumu after she had finished working with me. Many youngsters sought out Petronella at her new workplace and asked for me, which was somewhat disturbing for her. At the same time, she felt uneasy when they asked her for financial assistance since she also had brothers and sisters depending on her. The youngsters recognized her change in attitude towards them and felt confused since they thought they used to be friends. Carrying out research in one’s home country can make it difficult to draw a line between private life and research, whereas for me, it was much easier. My living situation is generally different, and I have no (grand)parents, sisters, or brothers who are depending on my financial assistance. Whenever I have the resources to assist one or two young people, I do so. At times, Sven and I send some money, or we send new and used clothes. We also have collected money from our friends and families to pay for two of the youngsters to attend secondary school; one young man was able to take three more courses after graduation from high school, and we helped another young woman by sending some starting capital for an income-generating business in selling second-hand clothes. Although our contribution is only a drop in the bucket and will not change the larger, structural problems, at least we can share a bit of our wealth from home.

3. Contributing to HIV/AIDS research and interventions

I was one of the few anthropologists—in comparison to the many epidemiologists—who was invited to participate in HIV/AIDS research at the Institute of Tropical Medicine (ITM). At first, I viewed this invitation, and my position, in a very positive light. I was happy that the Belgian medical team at ITM had thought of hiring an anthropologist to work with ITM’s epidemiologists to improve the implementation of YIP, and more particularly for Yeshica, the community-based component of YIP.

The ITM team could not afford to hire an expert in anthropology with the same years of experience as most other members of the ITM team; they hired me as a junior anthropologist who would be allowed to use the data for a PhD dissertation. This was an attractive position for me, because being part of a medical team at ITM would give me the opportunity to participate in their meetings and get an insider’s point of view on
Yeshica’s implementation. I considered myself lucky to be offered this privileged position as there are few medical teams who acknowledge the value of an anthropological perspective (see also Streefland 1990).

I was eager to dive into this topic and to contribute both to the work of ITM and to the wider academic debates in anthropology. I was also insistent on carrying out my work the way I was trained as an anthropologist. I vowed not to repeat the earlier errors committed by anthropologists before the 1990s: diluting our anthropological research methods into ‘rapid appraisals’, focusing only on focus group discussions or in-depth interviews, and excluding participant observation from the research methodology (for more on this critique, see Farmer 1992b; Bolton 1995; Parker 2001; Schoepf 2001). Participant observation is viewed by some as too time consuming, a perspective that appears absurd in the context of a 30-year epidemic and the long timeframe of development work. Unfortunately, both the HIV/AIDS epidemic and development work continue to be articulated as imminent crises requiring quick responses, a standard argument made by grant applicants and donors alike. Nevertheless, as an anthropologist, I was determined to use anthropological methods and triangulation to confirm my findings, as the combination of several research methodologies could cross-verify my data. My adherence to participant observation is not a question of scholarly pride or a matter of methodological fundamentalism, but is, rather, motivated by my belief in anthropology’s potential to add valuable insights to the fight against HIV/AIDS. But in some contexts, anthropology’s methodological legitimacy (see also Streefland 1990) is not yet won, and working together with epidemiologists remains a challenge.

4. The uneasy marriage between anthropology and epidemiology

Epidemiology is concerned with the distribution of illnesses and health in a population, and with understanding how, where, and why someone becomes sick. Human behaviour can play a role in this, but there is a large range of other (for example, genetic) risk factors. A common objective of anthropology and epidemiology is thus their understanding of human behaviour. A more integrated approach between the two disciplines would make a significant change in health care today. Nevertheless, cooperation between the two fields does not yet yield most of its potential synergies (Trostle 1986, 2005). There is still a discrepancy between what epidemiologists expect from anthropologists, and what anthropologists offer to epidemiologists. Epidemiologists call in the help of anthropologists in recognition of the limitations of their own
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discipline, but then find it a challenge to use the empirical material delivered. The anthropological work does often not fit the research and funding frameworks of epidemiology, nor the requirements of policy makers (Streefland 1990). Over the last decade, anthropologists and epidemiologists have tried to overcome the “benign neglect” (Trostle 1986: 80) and “missed opportunities” (Trostle 1986: 79) that have characterized their earlier relationship. But their current partnership is still imbalanced: epidemiologists have greater authority in the space of health and illness, by far, than do medical anthropologists (Hahn 1995).

4.1. The origins of an integrative anthropological-epidemiological approach

Until the last quarter of the 20th century, with a few exceptions, anthropologists and epidemiologists rarely cooperated despite the disciplines having much in common (Trostle 1986, 2005; Hahn 1995; Streefland 1990; Inhorn 1995; Leclerc-Madlala 2000). The pioneering work in South Africa by the Karks, in conjunction the Pholela Health Centre in 1940, and by the epidemiologist John Cassel, in Appalachian communities in 1964, led to the incorporation of anthropological methods and theory into epidemiology (Hahn 1995: 100; Leclerc-Madlala 2000: 138–139; Trostle 2005: 26–30). Moreover, in the 1950s and 1960s, a time when human migration—mainly male labour out-migration—increased in scale and distance, raising concern about the spread of chronic diseases, epidemiologists pushed for more interdisciplinary work (Trostle and Sommerfeld 1996: 257–258; Trostle 2005: 33–34). According to Streefland (1990: 13–14), only at the end of the 1970s, a decade marked by the launch of primary health care in many developing countries, epidemiologists started to recognize the consistency between poverty and disease. In this way, they created space for cooperation between anthropologists and epidemiologists. In practice, as Trostle (1986: 68–69) and Digiacomo (1999: 442) highlight, such studies did not really contribute to further theoretical developments either in anthropology or epidemiology, nor did they contribute to social structural change (Trostle 2005: 24). Little was done to link sociocultural factors with the epidemiology of diseases. As Trostle (2005: 34) explains, sociocultural factors “served as context” rather than being analysed for their “relationship to human health and disease”.

Although Inhorn (1995) tries to demonstrate that the areas of divergence between epidemiology and anthropology may be reconceptualised as areas of convergence in which the two disciplines could have exchanged insights, the power relationship between the two has been unequal and beset with difficulties. Medical
researchers, because they secured more funding, usually took a dominant position and, frustrated, anthropologists were included only as consultants, which gave them less institutional leverage than full-time staff (Streefland 1990: 18; Trostle 2005: 34). Anthropologists had little control over the nature of the questions being asked, over the content of the research, or the application of results. Over time, changes have occurred and progress has been made to bridge the difference between anthropology and epidemiology. Although I myself had no control over the protocol or content of the Youth Intervention Program (YIP)—of which my study was part—since it was already designed before my arrival at ITM, I at least managed to have control over my own anthropological research.

4.2. Anthropology and AIDS research

The rapid rise in reported HIV cases at the end of the 20th century encouraged greater cooperation between anthropologists and epidemiologists. Anthropologists had initially been rather slow to respond to the HIV/AIDS epidemic, and only during the 1990s did they begin to contribute, emphasising the cultural meanings of sexuality and the political economy of structural forces that construct sexual experiences and practices.

Early on, HIV research was strongly shaped by biomedical and epidemiological perspectives, and relied on psychological theories of individual behaviour change (such as the ‘Health Belief Model’, ‘Social Cognitive Learning Theory’ and the ‘Theory of Reasoned Action’) (Waterston 1997: 1383). Such theories are based on a rational behaviour model, leading to the collection of certain kinds of data: partner numbers, sexual networking, and frequency of intercourse. Based on these theories, behavioural interventions aimed to give individuals knowledge in order to motivate them to change their behaviour in ways that would reduce the risk of HIV infection (Parker 1987). These rational models had disappointing results since they examined sexuality as an independent variable, decontextualized from the everyday practices and meanings of sexuality (Spronk 2006; see also Vance 1991; Davis and Whitten 1987). In addition, categorizing certain groups of people as ‘risk groups’ misled others who were also potentially at risk, leading them to think they were immune because they did not belong to one of the so-called risk groups (Trostle and Sommerfeld 1996: 261).

These biomedical and epidemiological approaches, based on individualized notions of risk, were “searching for individual ‘deficiencies’ to explain seemingly irrational behaviour” so that individual (mis-)behaviour could be blamed (Bolton 1995: 293; for more on this critique, see also Farmer 1992a, Treichler 1999). Certain cultural
patterns such as polygamy or widow inheritance, among others, were also seen as obstacles to behavioural change and development (see, for example, Suda et al. 1991; Prince 2007). Many who were implementing HIV/AIDS interventions perceived culture as a fixed and immutable force (for more on this critique, see Sobo 1999). The dominant paradigm left little scope for understanding how broader socioeconomic, cultural, and political factors mediate behaviours, and why some persons are more vulnerable to the disease than others (Schoepf 2001; Parker et al. 2000a; Altman 1999; Farmer 1999).

From the mid-1980s onwards, anthropologists were invited to collaborate in large-scale surveys of risk-related sexual behaviour and the knowledge, attitudes, and beliefs about sexuality that might be associated with the risk of HIV infection. These research projects were mostly sponsored by public health agencies. The anthropological portion of such research was usually relegated to a subservient role (Schoepf 2001). Anthropologists were usually only “called in after the quantitative data [were already] collected and the number crunchers [could not] make sense of them” (Bolton 1995: 299). In effect, as Parker (2001: 173) describes, anthropologists often became subservient to others who dominated the field and who received “the lion’s share of funding and prestige”.

Partially as a consequence of this and partially because of a lack of interest among top anthropologists in the issue, the early research did not always provide high-quality anthropological data, as qualitative methods were often limited to focus group discussions and rapid appraisals (Bolton 1995: 298; Farmer 1992b, 1997). “At best,” Bolton (1995: 293) writes, “an ‘anthropological approach’ consists of calling for ‘cultural sensitivity’”. Such an approach tries to uncover cultural meanings to better understand what sexual practices mean to the persons involved, the contexts in which they take place, and how they are conceived within the local conceptual categories of the diverse sexual cultures within different societies (see also Farmer 1992b). During the 1990s, this focus led to culturally sensitive, community-based HIV/AIDS programs that aimed at transforming social and cultural norms, rather than individual behaviour (Schoepf 2001). However, Bolton (1995: 293) correctly asks: “Have we [anthropologists] nothing else to offer?”, stating that “calls for cultural sensitivity are problematic too”, since they stigmatize certain groups of people.

In the mid-1990s, book-length ethnographies that examined the broader socioeconomic and political factors responsible for the HIV epidemic started to become an important contribution to the battle against AIDS. The work of Farmer (1992), in particular, was groundbreaking, as he demonstrated how the ‘structural violence’ of
poverty and other inequalities contribute to people’s vulnerability in contracting HIV/AIDS. The idea that HIV spreads because of people’s normal responses to everyday life situations, such as dealing with economic hardship and uncertainty (Schoepf 1992; Barnett and Blaikie 1992; Farmer 1995, 1999), became dominant. Scholars recognized that unequal gender power relations and their relation to poverty also play an important role in the prevalence of HIV in most African countries (Farmer 1995; Setel 1999a; Schoepf 2001).

Although the notion of structural violence leaves little scope for agency among the poor, it has widened our response to the fight against AIDS. Setel’s work (1999) for instance, tries to capture the tension between structure and agency by writing about African youth at the time of AIDS, showing how the crisis of poverty wiped out young people’s attempts to reduce their sexual risks (see also Schoepf 1992). Thornton (2008: 55) goes even a step further and asks for a radical shift in perspective, as he wants to “redirect our attention from the scale of the individual to the scale of the social network”. So far, epidemiologists have mostly stressed the number of partners for the transmission of sexually transmitted infections and the visible links between different people (who has slept with whom). With his valuable concept of the ‘unimagined community’ (Thornton 2008: xx), Thornton invites us to focus on the structure of large-scale sexual networks and why the community of the sexual network cannot be fully imagined: he states that “the change in HIV prevalence is primarily determined by the differences in the configuration of large-scale sexual networks rather than by the cumulative effects of behaviour change, [which is] a necessary but not sufficient condition” (Thornton 2008: 1). Comparing Uganda and South Africa, Thornton (2008) tries to understand how and why sexual networks in South Africa are densely interconnected, pervasive, and extensive while in Uganda, they were more fragmented and isolated until 1992. He argues that, after 14 years of warfare in Uganda, previously separate sexual networks came into contact with one another, which resulted in an increase in HIV transmission. The later sudden decline in HIV prevalence was possible due to the fact that some (relatively few) people no longer were linked to the densely clustered network (Thornton 2008: 77–78).8 In South Africa, the majority were “multiply linked to all parts of the network, both near and far, and across most social categories” (Thornton 2008: 77). South Africa is characterized by a high degree of urbanization and physical mobility, which has caused a densely interconnected sexual network that is highly randomized (Thornton 2008: 71–77). A randomized network means that “large numbers of people in a population are linked to two, three or four people and all have
multiple links to others through circles, chains” (Thornton 2008: 63). In his book, Thornton (2008) also explains how HIV prevalence can be reduced when we reconfigure sexual networks (see also Chapter 5). Thornton’s contribution is an important one in HIV/AIDS research: through the analysis and understanding of sexual network configurations in a certain area, we might be able to develop HIV/AIDS interventions accordingly.

To conclude, anthropological research on AIDS has thus helped to demonstrate the limitations of some HIV/AIDS prevention theories and provided contextualised data that might contribute to the development of an alternative paradigm (Waterston 1997). Around the world, the HIV/AIDS epidemic has most often affected those who are economically and socially disadvantaged. As anthropologists, our role is not to merely bear witness and record this catastrophe (Bolton 1995: 288), but rather to create an in-depth understanding of people’s lives in a context of uncertainty. As anthropologists, we have to continue to improve the quality of our work, uphold our methodological rigour, take the lead in crafting research proposals, and not let others dictate our methods. My experience working in a medical team demonstrates the need for anthropologists to be clear about the need for and value of long-term, participant observation-based, ethnographic research.

5. The ‘black sheep’: An anthropologist in a medical team

The freedom to design research or to utilize methods according to professional standards greatly depends on funding (Mosse 2006) and on the degree of collaboration between the different team members. Since my research was embedded in a large quantitative study that was funded by PEPFAR, my anthropological research study was subjected to the same guidelines as the other medical studies. Being the only anthropologist surrounded by medically trained people in ITM was often challenging since there was a lack of interdisciplinary exchange in the Belgian team.

My intent was to conduct ethnographic research using methodological triangulation, but I was challenged in doing so by a number of factors. First, there was a misconception of anthropology serving as just a ‘toolbox’ of methods, from which one could pick and choose. There were additional, related difficulties in the approval of my ethnographic research proposal by medical and ethical committees (e.g., Institutional Review Boards), especially that of the CDC. I was also continually confronted by the
team’s prejudices against anthropological research, specifically their belief that it lacks empirical rigor as well as their misunderstandings about participant observation in the field. Finally, my Belgian colleagues had difficulty understanding that in addition to being co-investigators in a research team, they could also be my research subjects for some aspects of the research. Though most of these challenges were resolved during the course of the fieldwork, they never completely disappeared; there lingered in the background some suspicion about the ‘unscientific’ nature of anthropological work. Defending the methods of anthropological research was not only a time-consuming activity, on top of the daily fieldwork activities I was undertaking for my research, it was also as Streefland (1990: 18) nicely describes it: “a tough fight for recognition”.

5.1. Anthropology: A discipline or a ‘toolbox’?

Two anthropologists had preceded me at the ITM, and I assumed that there was no need to explain the basics of anthropology to my Belgian medical colleagues. However, my predecessors—following the instructions of my medical colleagues, like many other anthropologists who had carried out HIV/AIDS research during the 1990s—had used anthropology as a ‘toolbox’ (Streefland 1990: 16–17). This basically means that they pragmatically picked out the tool, or research method, that suited their particular purpose, rather than using a holistic approach where different qualitative methods were used with the final aim of carrying out methodological triangulation. Such research, according to Bolton (1995: 290) could hardly be called ‘anthropology’ since it was not based on “thorough field research in a community”. In essence, my predecessors had reduced the discipline’s qualitative methods to focus group discussions, ‘rapid appraisals’, and a sometimes unrealistically large number of in-depth interviews. Some anthropologists have tailored ethnographic methods into a formalized process called ‘rapid assessment procedures’ (RAP), the so-called ‘rapid ethnographic survey’ (Pool et al. 2006), or ‘rapid ethnography’ (Beebe 2001). To a certain extent, the phrase ‘rapid ethnographic survey’ is an oxymoron, since, by definition, quality ethnographic research requires an investment of time. Moreover, the maxim “bigger is better” also holds sway in many public health approaches, as an artefact of the reliance on standardisation. Knowledge of specific research practices is not enough to carry out behavioural research: one must also have a sufficient grounding in the theoretical knowledge of those methods as well (see also Bolton 1995).
5.2. Ethical regulations on anthropological research

In medical research, each proposal is bound by the World Medical Association’s Declaration of Helsinki (1964), which stressed the importance of Institutional Review Boards (IRB) in the US and later influenced the ethical standards in Europe (Hoeyer et al. 2005). The goal of these boards is to protect the rights of individuals who are participating as research subjects, and to ensure they are not exploited (Molyneux et al. 2004; Hoeyer et al. 2005; Murphy and Dingwall 2007). Being part of a medical team, I was also bound by the same regulations. The protocol format of the CDC’s IRB, however, was clearly created for baseline studies or clinical trials, and was evidently designed for research in the US. The IRB protocol asks the applicant to list the following information about the proposed research: race/ethnicity distribution of subjects; whether specimens are to be collected and stored; whether HIV testing would be performed; and whether genetic testing was planned now or in the future. Despite numerous critiques of using racial classifications within public health statistics (Weissman 1990), CDC protocols continue to consider race and ethnicity as important public health variables. The majority of the questions asked in the protocol template were not relevant for qualitative research, and some even contradicted standard anthropological notions, such as the existence of race (Hahn 1992). Since none of the participating institutes had developed templates for qualitative work, my ethnographic research proposal had to meet the CDC guidelines, including the submission of informed consent forms.

The discussion whether ethnographers need to follow regulatory regimes, which are based on assumptions derived from biomedical experimentation, is ongoing. Various authors (Molyneux et al. 2004; Molyneux et al. 2005; Hoeyer et al. 2005; Murphy and Dingwall 2007) however, have highlighted the problems with imposing such regulations on ethnographic studies. My research was of a descriptive nature, without specimens or tests, and thus following the same ethical guidelines of medical research was not possible (Murphy and Dingwall 2007). In medical research, obtaining informed consent is often seen as synonymous with conducting ethical research (Molyneux et al. 2005: 2547–2548). While there is much merit to this position, the flipside does not hold: we cannot simply equate the absence of informed consent in social science research with a lack of ethics or concern for risks. Anthropologists place great importance on reciprocity and building good rapport with our respondents, and consent is thus based on trust between researcher and researched. Consent is a relational process rather than a contractual agreement. In order to protect our respondents, we choose to utilize
confidentiality and anonymization (see also Hoeyer et al. 2005; Murphy and Dingwall 2007).

While building rapport with the youngsters of Winam, I repeatedly explained to them the topic of my study, as they developed their own insights and questions about what I was doing. I believe that informed consent forms do not make a lot of sense in the beginning of the study, because doubts and complaints usually arise over the course of the study, not at the outset. As Murphy and Dingwall (2007: 2229) state: “negotiating consent in ethnographic research is continuous and constantly vulnerable to withdrawal”. ‘Informed consent’ as it is conceived within the IRB protocol, then, is not very relevant in ethnographic research: it is practically impossible for anthropologists to get consent from everyone they talk to, and the object of study often changes during the course of fieldwork. Anthropologists have many casual encounters with people who pass through their research site, and it is not always clear who will end up being their research participants. Because of the flexible and reflective nature of the work, anthropologists continue to expand and shift elements of their research, including who their informants are, in order to remain open to new ideas as they emerge in the field. Anthropologists therefore cannot give a full account at the outset of what the research or its risks will involve because they simply do not know yet (Hahn 1995; Hoeyer et al. 2005; Murphy and Dingwall 2007). It is therefore up to the anthropologists’ own judgment and ethical responsibility how they should best respond (see also Dilger 2005). As Murphy and Dingwall (2007: 2230) rightly emphasise: “increasing bureaucratisation risks undermining the moral and ethical responsibility of the very researcher it seeks to control”.

In response to this conflict between biomedicine and anthropology about the use of informed consent, Hoeyer et al. (2005: 1747) suggest formulating an alternative approach that “combines the medical inclination towards respect for the individual with the social scientific awareness of political implications and informants’ conflicting interests”. They propose that the disciplines conduct “an enhanced dialogue [that] could serve to invigorate the ethical debate in both traditions” (Hoeyer et al. 2005: 1741). In the field, however, I found little comprehension among the Belgian ITM team, who had never considered informed consent from this critical perspective. Instead, they insisted that I follow the requisite informed consent procedures, and worried that my research protocol might otherwise be rejected. Since they were my co-investigators in the research proposal, had assisted me in the funding of my research, and were higher in rank at the ITM, I had to follow their requests. We had no real debate about the widely

To minimize any bias to my study that the use of informed consent might cause, while still adhering to the ethical standards of my profession (see the American Anthropological Association statement on this (AAA 2012), I felt free to change the standard version of the informed consent form used by CDC. The AAA (2012: 7) defines informed consent as an “ongoing dialogue and negotiation with research participants” rather than a one-off contractual agreement. I indicated in my research proposal that I would only obtain signed informed consent forms prior to tape-recorded, in-depth interviews with the key youngsters I worked with, most of which I conducted at the end of the fieldwork. This was in line with the AAA (2012: 7) as they state that “participation in activities and events in fully public spaces is not subject to prior consent”. According to the ethical guidelines of the AAA (2012: 7), “informed consent does not necessarily imply or require a particular written or signed form”. On the form that I used, I did not mention any monetary reimbursement for travel costs or provision of soft drinks or soap, although the CDC consent form template calls for such. Depending on the time and duration of the interview, offering a soft drink, tea, cassava, or bread was just part of being hospitable, and had nothing to do with a quid pro quo compensation for the interview nor could it be construed with ‘buying off’ my study participants. Travel reimbursement was also not needed because I mostly travelled to the youngsters who lived far away, and those who lived nearby usually came to my house on foot. In addition to the informed consent forms, I made use of oral consent on various occasions throughout my fieldwork, not at only one moment in time. The informed consent in my research was a “process (that) is necessarily dynamic, continuous and reflexive” (AAA 2012: 7).

It took about a year before the CDC’s IRB approved my research proposal, and without the approval of the CDC, my proposal could not be admitted to the other collaborating institutions. It is not clear what precisely slowed down the process, whether it was just subject to delays typical for large bureaucracies. It might also have been some of the anthropological jargon that delayed the approval since the reviewers certainly struggled with it. Their comments demonstrated that these IRB reviewers had a narrow and sometimes negative view of ‘participant observation’, the main and most important method of anthropology. Their narrow point of view on ‘participant observation’ was not only the case on paper, but also in the field.
5.3. Prejudices against and misunderstandings about ‘participant observation’

One of the IRB reviewers’ comments on my proposal was a request to specify precisely “when, where and why [the investigators] plan to collect each set of data (e.g. life histories, interview, focus groups, etc.) and how they will recruit participants for each type of data collection activity” (email communication, April 2005). The reviewers were also dissatisfied with my interview topic guide since it was too flexible and imprecise and therefore not regarded as a ‘valid instrument’. According to the CDC, a research proposal needs to be permanent, not subject to alteration mid-research. This is impossible in anthropological work because the research changes according to the context; necessary changes may only become apparent during the course of the fieldwork itself (see also Hahn 1995; Hoeyer et al. 2005; Murphy and Dingwall 2007). Once a research proposal is approved by the IRB, changes can only be made through an amendment letter that, again, also needs to be approved by the IRBs of the funding organisations before the revised proposal can be implemented.

I tried to answer the IRB comments in a comprehensive way, taking time to explain what anthropological fieldwork entails and the flexible nature of the study. At the same time, I stated my concern that “it is not always appropriate or helpful to judge qualitative work by quantitative standards” (email communication, April 2005), as I realised that one medical doctor and one epidemiologist had reviewed my work, and I knew that they might not possess the necessary understanding of ethnographic work and more specifically participant observation. My revised proposal was then met with a second round of comments, given by a behavioural scientist and an anthropologist (with a Master’s degree in linguistics). It is surprising that in a big institution like the CDC, qualitative work is not automatically reviewed by an interdisciplinary team that includes people with expertise in the kind of research being proposed.  

For my medical colleagues, certainly at the beginning of my fieldwork, the notion of ‘deep hanging out’, familiar to all anthropologists as part of participant observation, was not clear. For me, doing ethnographic work on the lives of the young people of Winam meant living with the people in Winam. When I was strolling around Dhonam or eating at my place with a number of youngsters, my colleagues might have wondered whether I had nothing else to do besides having pleasant afternoon visits. They assumed I would live in Kisumu, report on a daily basis to the CDC office there, and use CDC cars whenever I needed to go to the village. But I was certain—if one of my main methodologies was participant observation—that I needed to go and stay in the field and
participate in the daily activities of young people, and the closer to the centre of village life, the better. I chose to stay near the market of Dhonam, close to the fishing beach.

There was a serious mismatch between my medical colleagues’ expectations towards me and my intentions. This was caused by confusion about my role in the field as an anthropologist, while being, at the same time, an ITM team member. These misunderstandings caused a lot of friction and frustration, both for me and for my colleagues, because despite my good intentions as an ethnographer I often had the feeling that I could never produce the kind of results desired by my colleagues. I also often felt controlled, as if they wanted to make sure that I was effectively doing something in the field. My work was also often interrupted by requests from the Kisumu office, which did not take into account that I had my own schedule to follow. Although I asked on various occasions for a general staff meeting or an individual meeting in the field, during which I could have increased their understanding of my work and thus improved the situation, it seemed that it was not their priority.

Whenever I travelled to town, where the CDC office was located, I made sure to spend time visiting residents and other establishments, so as not to be solely associated with the CDC and the Yeshica staff. My Belgian medical colleagues from ITM did not understand why I needed to hang around in town, and expected me to immediately report to the CDC office and spend a full workday there. There were multiple reasons for me not to spend all day in the office. As I have noted, I needed to distance myself from the CDC, so that the youngsters would feel free to talk about their sexual relationships, Yeshica, and their perceptions of the CDC. Grasping town life would allow me to situate my local observations of the village in a wider perspective. Being in town gave me the opportunity to meet up with some of the youngsters who had migrated to town, and to meet with social scientists, health workers, and HIV-prevention workers. Of course, I still spent quite some time in the CDC office, and there were plenty occasions to observe the ‘organisational culture’ of the CDC office and to enjoy conversations with the CDC’s research staff. In addition, my research assistant Petronella and I participated in many activities organized by Yeshica, both in Winam and in Kisumu. The only difference was that we were not seen as part of the Yeshica or the CDC team since we only occasionally attended the weekly staff and research meetings. As I had hoped, we were appreciated as independent researchers who were interested in understanding young people’s point of view concerning health-promoting organisations in the area.
I definitely disappointed the ITM team in various ways because there was a lack of understanding and confidence in my kind of work. On numerous occasions I tried to explain what anthropological fieldwork entails and why I was making certain decisions, but often those decisions were attributed to my personality, rather than disciplinary rigour. In their view, I was a ‘die-hard anthropologist’ who did not want to adapt to their way of working, refused to go to the office on a daily basis, and wouldn’t join them in their Land Cruisers. When I became ill with malaria, I chose to be tested at the local hospital of Dhonam instead of going into town; doing so, I personally experienced the lack of resources in the hospital (despite the area being overwhelmed with medical research projects, not even a simple, rapid diagnostic test for malaria was available). These efforts to become immersed in the field and to improve my data were met with scepticism and distrust by my colleagues.

One could suggest that my medical colleagues were worried about me, and that may have been true as well, but there was more at stake. They had expected that I would organise in-depth interviews and focus group discussions with youngsters at the start of my fieldwork, and they assumed I would need their help to reach youngsters. When I carried out my work independently and did not need any help, they had less control over the collected data. Some of the Yeshica staff members were also worried about the kind of information I was gathering, concerned that my data would reflect negatively on them. Once they realized that the data I collected was not immediately reported to the Kisumu office, nor to Belgium, they had more confidence in me.

5.4. Colleagues and subjects
The place of Yeshica staff and ITM/CDC/KEMRI staff in my research brought often confusion. My supervisor at the ITM once asked, to my surprise, “Am I also being observed by you?” I clarified that, indeed, in order to understand the point of view of the youngsters who were my main research subjects, I needed to look at many different actors in the field in order to place the youngsters’ point of view in context (see also Hastrup 1995). If I wanted to understand what it meant for the young people to participate in Yeshica, I also needed to learn about the intentions and expectations of Yeshica staff and the ITM team, and how their intentions translated into practice. For example, the youngsters’ opinion of the project would be affected if a certain staff member took advantage of his position by having a ‘one-night stand’ with a youngster participating in Yeshica.
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My Belgian medical colleagues did not grasp that they were an important part of the larger context of the project, and therefore might be research subjects at certain points in my study (see also Murphy and Dingwall 2007). Whereas in the field, Yeshica staff members were aware that in addition to the youngsters that I followed, they were also my research subjects. In order to make my work understandable, I organised meetings with Yeshica staff, but I had only two formal conversations with the program coordinator, who was usually very busy with the implementation of YIP as well as other medical studies. The communication with our supervisor in Belgium happened more smoothly. Each time I went to Belgium, we had private meetings during which I clearly explained the work I had done so far and the challenges I was facing. My Belgian colleagues from ITM were given the opportunity to read and comment on my draft chapters, which gave me the chance to repeat my research objectives and explain how I came to certain conclusions. I gave several slide presentations for the ITM in which I presented summaries of my preliminary findings. However, most of my medical colleagues continued to question my methodology, and pointedly asked whether I had found some magic bullet for HIV prevention. Hoeyer et al. (2005) write that anthropologists should anticipate that most epidemiologists and medical staff do not understand the ‘thick description’ (Geertz 1983) of ethnographic work, and urge us to continually negotiate the terms of agreements made between epidemiologists and us. My experience as part of a medical team underscores the necessity of repeatedly explaining the nature of anthropological research, so that all potential research subjects—including team members—understand their role in it.

5.5. Research or intervention?

Having realised that I needed to maintain distance from the CDC in order to build rapport with the youth, I was often in an uneasy position of choosing between doing research or helping to implement Yeshica. For example, once during a formal staff meeting, the Belgian program coordinator asked my advice about the ‘livelihood intervention’ that could help young fisherman gain access to microfinance, and how the participating organisation (Kenya Rural Enterprise Program, or ‘K-Rep’) could better reach the fishermen in Winam. I shared with them that Yeshica’s office hours, 9am until 5pm, did not fit a fisherman’s schedule, as they work during the night, sleep in the morning, and hang around at the market or fishing beaches in the late afternoon. I suggested that these young men have no time or interest for savings clubs that promised them a loan in the far future. The program coordinator had asserted that the fishermen
made “easy money”, a characterization I contradicted, explaining the hard circumstances of the work and the small harvests they pulled in (see Chapter 4). The meeting ended with the program coordinator urging me to collaborate with K·Rep and Yeshica staff so that the fishermen I already knew could get introduced to Yeshica. While I didn’t want to disappoint her, I felt very uneasy about this prospect: I did not see it as my role in the field to promote the ‘livelihood intervention’. It would only bring confusion about my position in the field among the youngsters with whom I worked.

While my role was to conduct fieldwork, I received many, many requests related to project management—so many, in fact, that I began to wonder what to do with my knowledge about the problems of the project. I found it hard not to intervene in the YIP-related injustices I often saw happening in the field. In fact, I tried to draw attention to some serious issues of fraud at one point. In addition, I found it my ethical responsibility to intervene (see also Murphy and Dingwall 2007) when I realised that one member of Yeshica staff withheld travel reimbursements from a number of youngsters (see Chapter 7). I reported only some cases of impropriety—the majority I had to leave untouched as they were too delicate—to my supervisor in Belgium, and I hoped she would act on this information as she deemed it necessary.

Knowing about problems and being unsure about whether to intervene is an ethical dilemma that arises when interventions and research are intertwined without a clear delineation of roles and responsibilities. Throughout my fieldwork, I struggled with the extent to which my empirical data might, in the midst of the research, affect Yeshica. Whenever certain important discussions took place within Yeshica, I wondered: Should I speak up or keep quiet? In many cases, the youngsters explicitly asked me to intermediate between them and Yeshica staff, as their voices were often not heard, and they depended on the money that one Yeshica staff member withheld. In some cases, I did speak up, representing the point of view of the youngster, but the program coordinator often questioned the validity of youngsters’ self-reports (see also DiGiacomo 1999) and had more confidence in what local staff told her.

I was unsure whether my observations of the problems at Yeshica would be heard, and if they were even welcome. Schoepf (1995: 40–42) notes that whenever anthropologists are consulted on matters of HIV prevention, their advice might be ignored, seen as contrary to medical ‘wisdom’ (see also Streefland 1990; DiGiacomo 1999; Schoepf 2001). Similarly, in her work ‘Letting Them Die’: Why HIV/AIDS Prevention Programmes Fail, Campbell (2003: 171)—a social psychologist analysing an HIV/AIDS prevention project in South Africa—reports that “the social scientists’
attempts to elicit any kind of feedback, comment or discussion of their process evaluation findings from stakeholders or donors have been unsuccessful”. In a personal conversation with Campbell, she confirmed to me that even though she had spoken up during the implementation of the ‘Summertown project’ in South Africa, her voice was often not taken seriously and her views had not been taken into account (personal conversation, November 13 2006). After the data collection was complete, and I was busy with analysis, I spoke up on various occasions to my Belgian medical colleagues from ITM. Besides listening to my point of view, not much action was taken. I wrote two reports, one on the microfinance project and another on the relationship between parents and children, and in this way contributed to the evaluation of two of Yeshica’s interventions (the ‘livelihood intervention’ and the ‘Families Matter! Program’) (see Blommaert and Oluoch 2007; Blommaert et al. 2008).

The microfinance report was highly critical: it clarified some of the participants’ complaints about the livelihood intervention and offered recommendations for how to improve it. External consultants also evaluated the ‘livelihood intervention’ and came to the same conclusions (see Parott and Heyer 2007). In the end, nothing was done with the reports, and the microfinance project was simply stopped. At the time, I recalled a remark made by Hans Sonneveld, the former director of the Amsterdam School for Social Research (now named Amsterdam Institute for Social Science Research): “At the time you will be writing up your data, the project will no longer exist”. Indeed, during the writing of this dissertation, Yeshica ceased to exist.

Most of Yeshica participants left disappointed but, at least, had managed to get some benefit out of the project (see Chapter 7). While it did not change their sexual behaviour or prospects for the future, as was the project’s intentions, it had at least helped them to survive the day, and even the day after. And even though Yeshica no longer exists, the youngsters’ experience of the project—articulated in this dissertation—will hopefully contribute to improving many future interventions, even beyond HIV/AIDS prevention projects. Indeed, some of the insights put forth in this dissertation are relevant for any kind of intervention planned in the context of development work. The message emanating from my data is pretty clear and simple, for both researchers and development workers: work as equal partners across quantitative and qualitative disciplines, and encourage open discussions so that direct feedback from the field can be incorporated in project management.
5.6. Conclusion: An alternative contribution under the hegemony of epidemiology?

Working across disciplines, especially when combining quantitative and qualitative approaches, requires a lot of energy and time, usually from both sides. But in the rather common situation where most of the power and resources clearly lie on the quantitative side, the onus of explaining methods, and trying to gain credibility, lies with the qualitative researcher. Although some of the challenges I faced were related to specific personalities or circumstances, the underlying dynamics are common. DiGiacomo’s (1999) efforts were met with resistance, and even hostility. Bibeau described the struggle as “trying to participate [in the medical team] in a manner that did not also require alienating myself from my own profession” (cited in DiGiacomo 1999: 438). Permitting anthropological methods to be misappropriated will undoubtedly lead to a diminishing of the reputation and meaning of qualitative anthropological work. Bolton (1995) suggests a solution might lie in the development of a professional and ethical code to define clearly what constitutes an anthropological approach. At the very least, such a code could warn anthropologists about the pitfalls of participating in joint research ventures that fail to give anthropology its due.

While frustrating and time consuming, the challenges I faced trying to justify my methodology did not have a direct impact on my research findings. After enduring many difficulties, my situation improved significantly during the last six months of fieldwork, when I was allowed to work totally independently, without having to give explanations about my work. Although the team did not achieve a fruitful interdisciplinary exchange, at least some of my epidemiological colleagues did come to realise the added value of my anthropological work.

6. On researching sexual relationships

Moyer (2003: 8) convincingly argues: “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”. My goal was to understand how young people communicate among themselves about sex, sexual relationships, and HIV/AIDS: how they choose their sexual partners; and how these issues permeate their daily life. Observing and participating in their daily activities could give me insight into these questions, and because premarital sexual relationships are often kept secret, it was important to win young people’s confidence—and to carefully discern how best to introduce and approach the topic (see also Tadele 2005; Spronk 2006; van Eerdewijk
2007). Like Moyer (2003), I did not mention ‘HIV/AIDS’, ‘reproductive health’, or ‘sexuality’ while explaining my research topic in the local community. Not mentioning these terms helped diminish any association of me with the CDC, and, more importantly, prevented people from giving me practised, socially conventional answers. In order to find out about AIDS and young people’s sexual behaviour, I intentionally did not bring up those subjects until I had established rapport with the youngsters.

In the early months of my research, I spent time learning how to articulate my questions within local ways of knowledge. The UK’s Department for International Development’s ‘livelihood approach’ (Carney 1998; Carney et al. 1998; Chambers and Gordon 1992) served as a rough guideline for my research. Instead of asking questions directly about AIDS and sexual behaviour, my research assistant Petronella and I started with questions about how youngsters tried to earn their living. It was not my intention to measure young people’s poverty level, nor was it my intention to collect detailed data about each person’s livelihood. I just wanted to obtain a rough idea of what poverty and unemployment meant to them and what opportunities and constraints they had in making a living. It was not my intention to apply the ‘livelihood approach’ in all its details, as this was not the purpose of my research. Conversations also often started with people asking me where I was from, about my family, and what we did to make a living at home, which was an entrée for me to inquire about their families and their livelihood. We learned about the different livelihood possibilities in Winam and why young people made certain choices. Since engaging in sexual relationships is part of growing up and is a topic of interest in youngsters’ daily life, the youngsters themselves soon introduced the topic in our discussions. I quickly noticed that youngsters actually talked quite easily about sexual relationships and HIV/AIDS once I had approached the topic with some caution and had won their confidence. It was especially common to hear them gossiping about their peers, and the stories they shared with me about their friends’ sexual experiences made it easier to turn the questions to them. In this way, I could grasp young people’s life world, including their daily problems and aspirations, which would subsequently help me to understand their perceptions and expectations of Yeshica.

I realised at the beginning of my fieldwork that it was much easier to create rapport with young men than it was with young women, who usually just observed me from a distance. Because many HIV/AIDS studies have found that young women’s sexual behaviour is often underreported while young men’s is overestimated—largely because young men often boast about and young women often minimize their sexual
activity (see for example, Tadele 2005 and Spronk 2006)—I found it of genuine importance to find a young female research assistant in order to more easily associate with the young women of Winam. With Petronella's assistance, I managed to forge close relationships with most of the young women with whom we worked. Many shared their problems with us, and some even approached us for information on contraception and abortion. Approaching young men, on the other hand, was easier since most had a better knowledge of English and were not hesitant to get to know Petronella and me. Petronella had to be clear that she was already engaged, so no one would interpret her curiosity about their lives as sexual interest. The young men's interest in Petronella presented some problems: some of them tried to present a good picture of themselves to Petronella, denying they had already had many girlfriends. They wanted to hide from her the fact that they had had a sexually transmitted infection, but did not hesitate to ask my advice (and sometimes financial aid) about it. However, we were conscious of this and managed to challenge their answers, showing them that there was no need to hide information from us, and that they had nothing to lose by giving us the right information. Neither of us had any intention of starting a sexual relationship with anyone since we both were in committed relationships. But since Petronella's partner was not around, she was sometimes a target for some young men: the 'tactics' they used to try to seduce her gave us first-hand data about the 'seduction game' among young people (see Chapter 6).

Beginning with closed-ended questions such as “Do you have a boyfriend?” or “Have you already engaged in sex?” did not make sense since it was clear that all of the youngsters I worked with between the ages of 16 and 25 had already had sexual experiences, except for one, and I doubted whether her account was truthful. Instead, I approached the matter assuming that the young women already had a boyfriend, which made the conversation flow more naturally, and if they had no boyfriend, they felt comfortable saying so. We found that detailed information about youngsters’ past sexual relationships could best be obtained by listening carefully to the villagers’ gossip. The youngsters themselves did sometimes mention anecdotes about their past sexual relationships but some preferred not to talk about it because they wanted to forget it—which is understandable—as some relationships had left them feeling bitterness and hurt.

6.1. Data collection

I used multiple methods of data collection, including participant observation, informal
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conversations, in-depth interviews (including family genealogies), life histories, focus group discussions, a diary kept by one young man, and document analysis: I then triangulated the findings obtained from the different methods.

6.1.1. Participant observation

I wanted to grasp how young people give meaning to their sexual relations and navigate between different sexual partners. In addition, I tried to gain as complete as possible a picture of the diverse activities and tactics that people employed to improve their standard of living, in order to understand why young people made certain choices regarding their sexual relationships. My previous fieldwork in Bolivia had taught me that conversations alone would not always give me the full picture: a mother might tell me that her family was supported by her husband’s salary, neglecting to mention that they also own sheep, tend fruit trees, and cultivate staple crops. People do not always realise that all kinds of information may be relevant for ethnographic research.

Participant observation has also its limitations, and is especially difficult in the study of people’s sexual relationships. Spronk (2006: 34) concisely concludes: “participant observation has to stop, so to speak at the bedroom door”. Sexual relationships are surrounded by secrecy and intimacy and the investigation of such involves many ethical dilemmas (Herdt 1992; Bolton 1992; Abramson 1992). Thus a lot of the information obtained on sexual relationships is mainly based on what people have told me and, to some extent, what Petronella and I could observe. We also obtained a great deal of information by listening to youngsters’ gossip. Through understanding their everyday use of language (and noting what was left unsaid), I gained knowledge of the symbolism that gave meaning to the things that happened behind closed doors.

Participating in the daily activities of young people allowed me to establish rapport, and to eventually reach through the ‘snowball method’ a broad range of youngsters, who had diverse backgrounds and came from different villages within Winam. Together with Petronella, I went to all the places that young people frequented. In addition to the many funerals and disco matangas around Dhonam, we visited almost all the markets in Winam and several nearby fishing beaches; weeded and harvested maize and millet; helped prepare meals; washed clothes in the lake; and fetched water. On occasion, we accompanied youngsters to check-ups at the public hospital and visits to the herbalist; I went for my own malaria test at the local hospital. Petronella and I also participated in many forms of entertainment: played draft (checkers), karata (cards), and pool; watched football matches, and played football and basketball—sometimes in
the company of my boyfriend Sven; and watched local youths playing in a band with self-made instruments. We ‘chilled’ (hung out) at the Coca-Cola container (with warm Coke!); spent time at the hair salons; went to various discos; watched movies; and went to the bar in Dhonam. We visited many young people, both at the rooms they rented and at their parents’ homesteads. We observed and participated in religious events, including Catholic, Anglican, Evangelist, and traditional Nomia church services, and prayed with people in private homes. And of course, we participated in many Yeshica activities: the microfinance project meetings, Community Advisory Board (CAB) meetings, staff meetings, the Yeshica Youth Festival, vocational trainings, voluntary counselling and testing (VCT) outreach efforts, school visits, and more. Lastly, there were innumerable walks through and between the villages in Winam, stopping at every house where we were invited in.

It is difficult to communicate the extent of the research in figures, but to give an idea of the magnitude: we saw the core group of about 15 youngsters (of the total of 44) who lived close to our residence almost daily, not always for long conversations, but we at least exchanged a short greeting and we were able to ask how they were doing. We visited some of them at least once a week for a longer conversation of one hour to two hours. A good number of young people regularly visited our compound, so that every now and then the house was filled with visitors—even if Petronella and I were away and only Sven was home. Those who lived farther from the village we would visit every fortnight, and spend an entire day helping them with their daily chores and participating in whatever their activities might be. Every once in a while I brought some small gift as sign of our friendship, to show that I cared about them. My gifts were not always directly meant for the youngsters, as I often just gave them to their mothers on whose household resources they were living. People—usually the older women of Winam—also commonly asked me to bring them something whenever I returned from my trips to Kisumu. As it was a way of showing respect and affection as ‘a child of Winam’—as I could call myself at times since I was given the Luo nickname ‘Adhiambo’ (born in the afternoon)—to those whom I cared for, I often brought sugar, which was highly appreciated since it was relatively expensive in the village, or gave them soap or some of the many second-hand clothes that I had brought with me from Belgium.

6.1.2. Informal conversations and in-depth interviews

As part of observing and participating in the daily life of young people, I shared in and followed along with numerous informal conversations. I did not come up with a list of
topics that I wanted to discuss (although I had them in mind) at the first visit, but rather just visited people and let all kinds of topics emerge. While time was spent on topics that were at first sight unrelated to my research, they helped me gain a detailed picture of daily life, which taught me a great deal. Most conversations started with a group of family members, as everyone wanted to welcome me and my research assistant to their place, but soon after such greetings, we were able to talk one-on-one with the youngster we had planned to visit. When we did not find the youngster at home, we asked the parents or other caretakers about his/her whereabouts. In these moments, we also took the opportunity to ask them about what life was like when they were young, and their opinions on certain topics related to my research. Although the staff of the HIV/AIDS programs and other development programs in the area, religious leaders, and traditional herbalists were not an immediate part of the research group, I also had a number of conversations with them since their opinions and perceptions of everyday life and the HIV/AIDS programs were important in gaining a broader perspective.

In addition to informal conversations, we carried out in-depth interviews to deepen our understanding of certain issues. These took place only towards the end of my fieldwork, between April and October 2006, because I first wanted to have a good relationship with the young people we worked with before having a formal, tape-recorded, semi-structured interview, complete with an informed consent form. I did not carry out formal interviews with all the youngsters I worked with; I interviewed those who I expected to have answers to my questions, or to capture a particular life story. In total, I conducted 18 in-depth interviews with youngsters, seven in-depth interviews with grandparents (family genealogies), and eight in-depth interviews with health staff, two researchers, and a priest. I prepared all interviews beforehand with my research assistant: I selected the person to be interviewed and the topics to be discussed, and discussed the topics with Petronella, who added topics as needed. Petronella conducted the interviews if they needed to be done in Dholuo, but I was present during the majority of the interviews, and, since my Dholuo was relatively good, I was able to follow the conversation. I conducted the interviews when the youngster could express him- or herself sufficiently in English. After each interview, I gave feedback to Petronella about what went well and what could have been improved, giving her on-the-job training and assuring the quality of our work.
6.1.3. Life histories

As a way to examine the interplay of social structure and action by individuals and groups, I made use of life histories. Through this method, I got an overview of the changes and developments of a person’s life and an insight into how large-scale processes of sociocultural change act in local contexts (Schoepf 1992: 260–261). Life histories, however, were not collected all at once but in bits, sometimes on different days. In White’s (2000: 39) collection of social histories related to the various African bloodsucking stories, she confirms that “people do not give testimony that fits neatly into chronological or cosmological accounts”. In the beginning of the conversations with the youngsters, Petronella and I usually tried first to get a general picture about the livelihood options available to them. As trust increased, we could broach the topic of sexual relations. Subsequently we discussed their memories about their upbringing and their parents’ livelihood: some were raised by grandparents or siblings, because their parents had died. Others had never had a relationship with, or had even known their father, especially children born out of wedlock. Some of the young women were already mothers and refused to discuss paternity, shyly laughing our questions away. It was only through gossip and rumours, and tying together little data points like where they went to school or when the child was born, that we eventually came to guess where and when they likely had met the biological father.

During these conversations, the youngsters had the freedom to shape the conversation and describe things in their own terms, such as the meaning they gave to sexual relationships or how they wanted to improve their living conditions. This methodology helped me to discover people’s less obvious motives and emotions, and to put those into the context of their upbringing and family history. Like a large puzzle, we pieced together each life story from many different sources and moments of information. In total, we completed seven life histories of young men and nine of young women. Though fewer in their number than interviews, these 16 life histories are an incredibly rich source of data and a powerful way to understand how and why a person might see things a certain way.

6.1.4. Focus group discussions

Focus group discussions (FGDs) were useful for exploring how the accounts of livelihoods or sexual relations are constructed through social interaction. Although I would argue that focus groups are currently a bit overused in evaluations of development work, they are a good tool to analyse the degree of consensus on a given
topic and which opinions are publicly acceptable. Like the in-depth interviews, FGDs were held in the latter part of my fieldwork after a relationship of trust had been established; they were held in our rented room, and participants were offered tea and bread. One FGD on the topic of sexual relationships was held with the five young men who made up Yeshica’s Post-Test Club (PTC, the group of youth who had been tested for HIV/AIDS and were public about the results); they enjoyed it so much that we organised two additional FGDs with them. Another FGD was organised with four fishermen to learn more about their livelihood. Only two of the four fishermen belonged to the group of my research participants that I followed closely. We made multiple attempts to organise FGDs with a group of young women who participated in Yeshica and a group of young women from Dhonam to talk about sexual relationships, but in the end we were only able to organise one with five young women from Dhonam who already knew each other very well. The young women in the group talked mainly about others instead of themselves. Each FGD lasted three to four hours; the PTC group really enjoyed themselves, noting that they lacked opportunities for such frank conversation at Yeshica, while the fishermen and young women were eager to wrap up and get home after three hours.

6.1.5. One young man’s diary

I did not intend to ask anyone to keep a diary as part of my research. But one young man, Onyango, was a particularly rich source of local knowledge. If he was visiting us, which was often, when other visitors dropped by, he usually talked to us after they departed about their reputation and current situations in which they might be involved. Onyango also came by to update us whenever a disco matanga was taking place. As he was eager to improve his English, I suggested he could write for me about what he encountered every day when walking around the village. His account was a very interesting source of information about how young people seduced each other, how they discussed sexual relations, where they had sex during the day, and so on. For ethical reasons, Petronella and I did not ask him to present us to the persons he was talking about in his diary, as these persons were not aware that Onyango was writing about them. Neither had we informed them about our research. Our main intention of this diary collection was rather to grasp where youngsters usually meet each other and the topics they usually discuss amongst each other.
6.1.6. Document analysis

I also collected a number of secondary-source documents before, during, and after fieldwork in order to understand the larger context of life in Winam. Newspaper articles from a variety of journals, translated songs from Dholuo bands, and documents from Yeshica and the YIP were of relevance as they provided important background information.

6.2. Data analysis

Except for writing down a few key words in a small notebook, Petronella and I rarely took notes during participant observation activities or interviews. Afterwards, around lunch and dinner, Petronella and I wrote up the data—sometimes until late in the evening—as accurately as possible in a hand-written field journal that Petronella then word-processed using a computer. A significant number of interviews were conducted in Dholuo, which meant that I had to rely to some degree on Petronella’s reporting. Every evening I took time to go through the notes to point out lingering questions, contradictions, and uncertainties. Petronella was often able to shed light on what was unclear, and if not, we took note and revisited these questions during our next visit.

Tape-recorded in-depth interviews and FGD data were transcribed and word-processed (Microsoft Word 2003). Given the large amount of data that I was able to collect in the field over 20 months of fieldwork (approximately about 3 to 6 pages of fieldnotes per day or about 40 to 60 pages per month; each in-depth interview ran about 30 to 40 pages and each FGD about 50 to 60 pages), transcription took an enormous amount of time. I was lucky that Petronella was willing to transcribe during evenings and weekends while I was busy with the analysis of the data. However, quite a big amount of fieldnotes, in-depth interviews and FGD could not be transcribed and word-processed before the time (October 2006) that Petronella left for another job as my fieldwork period was finished. For the remaining part, Petronella trained two transcribers who finally were able to finish the work three months later.

After everything was transcribed, I coded the data using Atlas-ti, a Computer Assisted Qualitative Data Analysis Programme. Atlas-ti is used to systematically identify concepts across multiple interviews and fieldnotes, allowing us to find similarities and disparities in how young people perceived their life world. Sometimes I found very similar uses of a concept, but in other cases, I could discern that there were important nuances that needed to be taken into account. In this way, Atlas-ti or similar programs are superior to quantitative programs that only provide statistics about the
frequency of usage of certain concepts. Atlas.ti also allows the quick collection of all information related to a specific youngster. By coding data not only by theme but also by name, I could easily bring together everything that I had learned about a person, either from him or herself or from others. After identifying the key concepts, Atlas.ti allowed me to build small ecosystems or networks of related concepts. This process allowed me, for instance, to show that a purely transactional account of sexual relations in fact did not exist in the perceptions of young people. Love and emotional attachment were concepts that inhabited the same ecosystem as sexual relations.

With all of this data, the challenge was to gain sufficient distance from my material to be able to formulate a critical analysis. My aim and approach was to work inductively by letting my data inform theory, i.e., to let my data speak for itself, which turned out to be a rich, intense, and, in the end, satisfying process. When writing about ‘the Other’, I take Fabian (1983)’s critique about anthropology’s allochronism into account, which prompts us to reflect critically on the political dimensions of temporal rhetoric. As he writes, the use of the ‘ethnographic present’ creates a timeless sense of action that turns “the ethnographic subjects into exotic creatures” (Fabian 1983: xxv). It is important to be conscious when using time in this dissertation. I decided to use the present tense when I discuss something that has been happening for a long time, or when it has a continual sense to it, while I use the past tense in recounting specific conversations and events. My use of present tense is not supposed to imply that things will never change, but is simply a stylistic tool to make the text more fluid for the reader.

7. Conclusion

How can we as researchers contribute to a more effective fight against the HIV/AIDS epidemic? What lessons can be learned from working in multi- or interdisciplinary teams comprised of both quantitative and qualitative researchers? And what methods will help deliver new insights into the sensitive topic of sexual relations and livelihoods among young people in a Kenyan village?

This chapter reviewed the history of HIV/AIDS research, the challenge anthropologists have had in convincing funders of the value of the discipline’s methods, and the more recent theoretical focus on the structural violence of poverty and other inequalities. Since 1990, ethnographic research has helped to shape the creation of more successful, community-based interventions that aim to address the wider socioeconomic
and political factors responsible for the spread of HIV, not just individual behaviour. My personal view is that as researchers we should have an activist approach and work towards improving the situation of the people and the programs that we are studying. To be able to do this, we as anthropologists have to continue to improve the quality of our work and not compromise our methodological rigour. While we strive to provide good anthropological insights, epidemiologists must meet us halfway and embrace collaborative project design, implementation, and evaluation. Only in this way will no longer talk about “we” and “them” but instead work together, on the same level, in an interdisciplinary team, with the common goal of winning the fight against HIV/AIDS.

But the journey does not end here. Even if our methodology is an integrated part of a research project’s design, the real-life implementation of interdisciplinary work is far from self-evident, as I learned during the course of my research. I argue that we should not allow our interdisciplinary partners to ‘cherry-pick’ one or two anthropological methods: anthropology is not a toolbox, but a rather sophisticated approach that triangulates data obtained through several methods, most importantly participant observation. Sticking to my methodological ‘guns’ meant that it took extra effort to convince IRB reviewers that my work was indeed scientific. While there was certainly a lot of good will regarding the inclusion of qualitative work in my experience with the ITM team, I learned that there still remain prejudices against participant observation, and beliefs that it lacks empirical rigour. From my experience there are two sides to this. On the one hand, anthropologists need to do a better job in communicating how we safeguard the quality of our work and how we make it rigorous. We should not assume, as I probably did, that when we are hired, other team members understand what our work entails. On the other hand, our colleagues need to understand that interdisciplinary teams require additional effort in terms of internal communication, so that all members understand each others’ methodology, including why the team members might be research subjects themselves.

Having discussed the historical and institutional context of my work, the last part of this chapter detailed how I actually conducted the research. The combination of participant observation, informal conversations, in-depth interviews, life histories, focus group discussions, and a diary, was very labour intensive, both in the field as well as afterwards, when I had to dig through mountains of data. The sheer volume of data meant that analysis took a long time, especially because I wanted my data to speak for itself rather than press it into a preexisting framework. In the following chapters, we will see what the data has to tell us about how youngsters create livelihood networks in
an impoverished area such as Winam. I will show how my methodological choices opened the way to an in-depth understanding of the youngsters’ ambitions, concerns, and aspirations in an impoverished, highly unpredictable context. Thanks to the good rapport Petronella and I (and my boyfriend Sven) established with them, I am able to shed light on their pragmatic actions, both in their sexual relationships and in their daily livelihood pursuits.

1 Sven and I officially married in 2013 after a relationship of more than 15 years.

2 I carried out fieldwork in the Tropics of Cochabamba (Bolivia) in 2002; my goal was to understand the impact of the ‘war on drugs’ on the livelihood strategies of coca farmers (Blommaert 2003).

3 In the next chapter I discuss this issue further, including an incident in 2003 when a newspaper reported that people in Rarieda were used as ‘guinea pigs’.

4 I have two academic supervisors: Prof. dr. Anita Hardon from the University of Amsterdam (UvA) and Prof. dr. Mirjam De Bruijn from the African Study Centre (ASC). My third supervisor is Prof. dr. Anne Buvé from the Institute of Tropical Medicine (ITM).

5 The phrase is borrowed from an article by Trostle (2005).

6 The ‘Health Belief Model’, the ‘Social Cognitive Learning Theory’, and the ‘Theory of Reasoned Action’ are different models based on social cognitive theory, and start from the assumption of reasoned action. These theories argue that behaviours can be shaped by rewarding or punishing people for their behaviour. These models are often used in public health approaches that aim to change health behaviour.

7 Spronk’s (2006) dissertation was published in 2012 (see Spronk 2012).

8 When those people “who provide significant linkages across densely intra-linked, clustered subnetworks no longer serve as transmission links (because they die, start using condoms […]), overall prevalence is likely to decline rapidly” (Thornton 2008: 79).

9 For race/ethnicity the IRB used the following categories: white, Hispanic, Black or African American, American Indian or Alaskan native, Asian or Pacific Islander.

10 According to Trostle and Sommerfeld (1996: 255), “At least 19 anthropologists are now employed at CDC in Atlanta”, yet I wonder who falls under the category of ‘anthropologists’. My experience has taught me that for some, including the CDC staff working in Kisumu, any social scientist or sociologist or psychologist or even a public health worker falls under this category.

11 There were a number of issues that were going on in the field that were in contradiction to the project’s general aim, such as Abich Rawere (Forum for the Youth) which was organised on Saturday afternoons to show documentaries on HIV prevention, turned into a local cinema with Nigerian and war movies (see Chapter 7).

12 The first formal meeting with the YIP program coordinator took place at the end of my stay in March 2004, the evening before I flew back home. The second meeting was when Petronella and I
organised a formal in-depth interview with her at the end of the research in September 2006. The program coordinator however, had to cancel the meeting and the meeting was rescheduled in October 2006. I had already left the field, so Petronella actually carried out the interview.