

2 Participant Observation in Demographic Research: Fieldwork Experiences in a Ghanaian Community

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The anthropologist engaging in demographic research risks getting lost in a forest of paradoxes. Nowhere are intimate personal feelings—let us call them love and passion—and national public interests so closely intertwined as in the birth of children. Movements in the dark come to light nine months later and become hard data for policy-makers. The birth of a child is both a matter for cold statistics and a subject of human emotion. The same can be said about the end of a person's life. Death, the most devastating human experience one can think of, will eventually be transformed into demographic data. The shock of paradox reaches the level of absurdist theatre when the state attempts to break into the intimacy of lovers and publicly holds technical devices in front of them to persuade them to prevent the birth of another child.

The anthropologist working in demography faces these two extremes. In an attempt to 'make both ends meet', he or she sets out to prove that these paradoxes are only paradoxes and not irreconcilable contradictions. Love poetry as well as statistics will have to be dealt with, involving the use of both keyholes and keyboards.

The point of departure for this article is that the anthropological approach and the demographic survey complement one another, both in their methodology and in the type of information they produce. Even where the two seem to yield contradictory data, they should still be regarded as complementary, elucidating and nuancing one another. I subscribe to Kaufmann's conclusion that it is beneficial to combine the strengths of the two approaches:

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Demographic data could be interpreted with the anthropological data, and the demographic data could be used to combat the specificity of the anthropological, extrapolating inferences from them with greater justification. (Kaufmann 1991: 55–6)

If this chapter extols the virtues of participant observation, it does not do so at the expense of quantitative methods. My objective is to demonstrate the need for qualitative research if we want to assess the meaning of quantitative data. The importance of quantification for an assessment of qualitative data is equally recognized but falls outside the scope of the present discussion. This chapter will show which methodological and mental tools the anthropologist in demographic research needs to do the job. They are: the ethnographic interview, participation, observation, and introspection.

I am an anthropologist who lived for almost two years in a West African village, in Ghana. I spent most of my time looking around and listening to people's stories, including their love stories, which mostly ended badly, as good stories should, as well as having my own personal love story. Being there, I became more and more surprised about the certainty with which demographers, economists, and politicians spoke about the 'population problem'. They refer to 'fathers', 'mothers', and 'children', but it is not always clear what they mean by these words. The confusion rises particularly when terms like 'marriage', 'fertility', and 'birth control' are used. My main purpose in this chapter will be to take away the false exactitude of such key terms in demographic parlance and to call for greater awareness of their cultural specificity. The conventional anthropological research approach will be useful to add meaning to statistical data. This should be done by asking, looking, and understanding what these words mean to people.

We should not expect too much clarity, however. People are inclined to conceal and deny what they cherish most. Some things are none of our business, so we have to guess at them. Other things we may not understand because we are never able to put ourselves completely in other people's places and to feel what they feel. Cultural analysis, writes Geertz (1973:23), 'is intrinsically incomplete' and 'essentially contestable'.

Asking Questions, Listening, Conversing

It would be a mistake to take the term for the anthropologist's favourite research technique, 'participant observation', literally and to think that it excludes interviewing. Listening is so much at the basis of every learning process that it is unnecessary to name it explicitly. Seeing a person's life and taking part in it, which are the two main ingredients of participant observation, makes sense only when they are accompanied by speaking and listening.

Spradley (1979) begins his book on the ethnographic interview with an example from the field. Elizabeth Marshall, an American anthropologist, meets a !Kung woman, Tsetchwe:

then after a moment's pause, Tsetchwe began to teach me a few words, the names of a few objects around us, grass, rock, bean shell, so that we could have a conversation later.

Spradley then comments:

'Tsetchwe began to teach me . . .' In order to discover the hidden principles of another way of life, the researcher must become a *student*. Tsetchwe, and those like her in every society, become *teachers*. Instead of studying the 'climate', the 'flora', and the 'fauna' which make up the Bushmen's environment, Elizabeth Marshall tried to discover how the Bushmen define and evaluate drought and rainstorm, *gemsbok* and giraffe, *torabe* root and *tsama* melon. She did not attempt to describe Bushmen social life in terms of what we know as 'marriage' or 'family'; instead she sought to discover how Bushmen identified relatives and the cultural meaning of their kinship relations.

And he concludes:

the naive realist assumes that love, rain, marriage, worship, trees, death, food, and hundreds of other things have essentially the same meaning to all human beings. Although there are few of us who would admit to such ethnocentrism, the assumption may unconsciously influence our research. Ethnography starts with a conscious attitude of almost complete ignorance. (Spradley 1979:4)

In my case, the ignorance was overwhelming. I spent almost six months in the village doing nothing other than learning the language. Some of my teachers were small children. They taught me the words for the things most physically present: table and chair, nose and eye, yam and rice. 'The stranger is a child', was one of the proverbs I learnt first. Another one was equally appropriate: 'The stranger does not break the law.' I was allowed to make mistakes, but I learnt they *were* mistakes.

The Demographic Approach

Naturally, one is inclined to think that the census and the survey questionnaire are the most suitable techniques for demographic research. There are however at least two reasons to treat that assumption with caution. In the first place, close-ended questions assume that the meaning of the question is clear, and that interviewer and respondent agree on that meaning, which may not always be the case. Second, questions may touch upon delicate and potentially embarrassing issues which people do not want to discuss.

Even simple questions such as 'Are you married?' or 'How many children do you have?' caused confusion in my own fieldwork. What did I mean by 'married'? There are different ways of having a partner. Formal customary marriage, which involved some flimsy rituals, was one of them. Christians could have a church wedding, which was a big thing, but it hardly occurred in the village although it was full of Christians. Rather, church weddings

took place primarily among the urban élite. The same applied more or less to 'marriage by ordinance', the official state marriage.

On the 'illegal' side there were also various different shades of sexual union. *Mpena awadee* was a socially recognized but not customarily sanctioned relationship. Some people, after a couple of divorces, preferred *mpena awadee* because, as they said, they were 'tired of marriage'. Young people often engaged in a secret lover relationship, although that 'secret' was sometimes known to a large group of people. A married person—or, to be more precise, someone (usually a man) with a publicly known partner—could also have a secret love affair. And finally, there were a number of people who, for various reasons, opted for more casual relationships.

For example, a man of about 35, who rented a room in the compound where I was staying, told me that he had decided not to marry but to stick to lovers. 'Women', he explained, 'ask too much, so it is not advisable for a poor man to marry.' He had one child and said he gave the child one or two cedis each month (then about one-half of one British pound). He claimed he gave his girlfriends a cloth three times a year. 'That is cheaper than being married to them.'

I wonder what this man would answer to a poll-taker's question 'Are you married?' Depending on his mood, his impression of the poll-taker, etc., he could just as well answer yes or no. To him, either answer would be wrong anyway. For him, the right answer could never be one word; he would need a story to explain his position.

My main point, however, is not the ambiguity of the term 'marriage', but its emptiness. Even if it were clear what legal status and form of union the term refers to, we still would not know what 'marriage' means to the person interviewed. In the village where I did my fieldwork, some 'married' people hardly ever saw one another because they lived far apart. Even if a man and his wife were living in the same village, they usually did not cohabit. As a rule, each lived with his or her own lineage.¹ In the evening the wife would prepare a meal for her husband. She, or a child would bring the food to the man's lineage house where he would eat it together with his brothers and cousins and not with his wife.

In the late evening the wife may visit her husband and spend the night with him. (And while she is away her daughters may spend the night with their boyfriends and return home a little while before their mother returns.) There are also couples who do cohabit. A rich man is able to draw his wife away from her family because he can offer her financial and social security. Others live together because one of the couple, usually the woman, is from another village, which is too far away to practise a duo-local marriage.

It is not only the residence pattern, however, that varies from one marriage to another. Similar divergences exist with regard to care of children, division of tasks, and financial arrangements, to mention only three.

In summary, what do we really know if someone truthfully and correctly answers that he or she is married? What does this variable 'married' clarify with regard to the complex issue of fertility behaviour? In my research the answer was: Nothing. Asking the question 'Are you married?' was almost useless because the term itself had no meaning. Neither 'yes' nor 'no' carried the information I needed to understand what people were doing.

The irony of the survey question is that it conceals this lack of understanding. The 'closed' question (*nomen est omen*) does not lend itself to the complexity of the respondent's own ideas and experiences and thus escapes the correction of its wrong presumptions. The closed question is a 'safe' question, in that it allows the questioner to stick to his naïve realism (remember Spradley) without being aware of it, let alone being bothered by it.

Can social scientists really be so naïve? Am I not making a caricature of the survey approach? I think not: in addition, I believe that the naïveté and false trust in statistical data are understandable from a cultural point of view. There are thoughts and practices that are self-evident only to those who are part of a specific culture. Things become unquestionable in their 'natural environment' (i.e. their own culture). What to outsiders may seem a weird belief is respectable and rational knowledge (science) for the 'natives' of that culture. That is as true for beliefs about witchcraft among the Azande as for faith in quantitative data among certain Social Science tribes; thinking in terms of statistical data and not being bothered by what is outside those data is part of their culture, as is continuously evidenced by their publications.

Let us now, finally, look briefly at the second reason why the questionnaire approach may cause problems in demographic research. Some topics are too sensitive or embarrassing to be dealt with in a survey. A coincidence during my fieldwork demonstrated this in an almost disconcerting way (Bleek 1987).

I did my research in one extended family or lineage (*abusua*) and was able to have interviews and informal conversations with 42 of its adult members, nearly its total number. The conversations were about marriage and sexual relationships, having and not having children, and birth control, including the practice of induced abortion, to mention only the most important topics. My research supervisor judged the number of 42 too small for a research project with demographic implications. He advised me to carry out a survey among a larger sample of the village population. One of the steps I took to follow his advice was to interview mothers with young children during their visit to the local child welfare clinic. I had ascertained that these constituted a good representation of all mothers between 20 and 45 in the village. Six nurses from a nearby hospital carried out the interviews after receiving a brief period of training. All young, around the age of 20, they wore their

uniforms during the interview to give the proceedings a medical air. I assumed that questions about sex and birth control would meet with the least resistance if put in an aseptic clinical environment. I kept out of sight as much as possible.

Examining the results, I soon realized that the quality of the research had not been improved by this survey. During my conversations with the women of the lineage, 63% of them ($N =$ only 19) had told me that they had practised some type of birth control at some time, 21% of them said they had used three (or more) different types of birth control, and 53% confided to me that they had once (or more frequently) had an abortion. The corresponding percentages from the survey sample were 14%, 1%, and 4%. Clearly, that difference had to be explained by the research method and not by some exceptional characteristics of the particular lineage that I was studying. I became convinced that the respondents of the survey had tried to make a respectable impression on the nurses, something they had not been able to do with me, since I already knew 'too much' about them. I was sure, but could I prove it?

I was helped by a stroke of luck. I discovered that six women of the lineage I was studying had taken part in the survey. They had been interviewed without knowing that their responses would eventually come to my attention. When I compared their answers with what I knew about them, I made a both shocking and fascinating discovery. To put it plainly, they had lied lavishly; to put it in a more sympathetic light, they had construed their own 'truth', presenting themselves in terms that they expected would make the nurses respect them. Some of their answers were so far removed from the facts as I knew them that I was confounded. Some of the contradictory answers given by one individual, whom I have selected specifically to illustrate this point, are as follows:

<i>Survey interview</i>	<i>Anthropological research</i>
24 years old	31 years old
Divorced once	Divorced twice
Given birth to four children	Given birth to six children (two of which had died)
Has been pregnant four times	Has been pregnant at least nine times
Lives with husband in Accra	Lives with husband in village
Has never used any form of birth control	Has experience with many methods of birth control
Has never had an induced abortion	Has had at least three abortions

The conclusion is self-evident, old news: the questionnaire approach is unsuitable for eliciting information about intimate, potentially embarrassing thoughts and practices. If informants want to remain polite—and many do to an incredible degree—they have no other choice than to lie. These six did so profusely.

In conclusion, many issues in demographic research are too complicated, too ambiguous, and too intimate to be handled in a survey manner. Informants are likely to respond to closed or half-closed questions with true answers which do not make sense or with lies which do make sense, but a sense that is beyond the grasp of the poll-taker.

The Contribution of Anthropology

So far, this chapter has exposed some of the weaknesses of demographic research. How can it be made 'stronger'? How can the anthropological approach ameliorate the quality of demographic fieldwork? Some suggestions have already been implied in the above critique of conventional quantitative methods.

Conversation

Demographic research should begin with a qualitative reconnaissance in that specific setting of the meaning of words and practices to be used in the research. Such a reconnaissance becomes imperative when the researcher is not a member of the community or society under study and is unfamiliar with its culture. The most appropriate method of obtaining information is ordinary conversation, which is both informal and spontaneous. This method can slowly be developed into a more systematic and structured exchange of information. This is not the place to delve deeply into the methodological variations and details of qualitative research. Numerous introductions on qualitative methods exist, e.g. Spradley (1979); Burgess (1984); Hammersley and Atkinson (1995).

I have already referred to Spradley's plea for the anthropologist to see himself as a learner and the informant as a teacher. For an attentive researcher it is not difficult to 'play' that role in conversations and interviews. The comparison with a normal conversation in everyday life is the best guideline for effective qualitative research. One person listens to another. A question is asked and the questioner tries to understand what the respondent is saying. In the answer, the respondent is adding other pieces of information which may contribute to the context of the topic under study. New questions arise, and one soon realizes something can be learnt only by fully understanding its context.

Thus, a natural conversation consists of a flow of information which proceeds rather haphazardly. People think in an associative way, jumping from one topic to another; but the final result of that seemingly undirected communication is a gradual understanding of the initial topic. After the person who asked the questions has returned home, he or she may stumble upon an aspect which they forgot to discuss. The next day, on meeting the

other person, the questioner will pick up the thread and a new conversation evolves, which will lead to new insights and a more complete comprehension. A definitive, perfect answer will of course never be achieved, either in real life or in anthropological research (cf. Geertz's remarks earlier on).

After a satisfactory level of general understanding has been reached with regard to the key issues of the research at hand, one will then be able to ask questions that will produce meaningful information. The question 'Are you married?' may have to be rephrased or combined with additional questions. Some questions must perhaps be cancelled, since they do not produce the type of information that can be quantified without losing all meaning. Qualitative and quantitative methods are shown in this way to be complementary, since qualitative research selects and steers the quantitative questions and is indispensable for interpreting the answers.

One of the disadvantages of the 'natural conversation' method is that it takes time. Another drawback, although to some extent also an attraction, is that it requires considerable psychological, and even emotional, investment. The latter is indeed an essential part of the anthropological approach. Without feeling there is no understanding. The personal engagement, an issue that will be discussed more fully later on, enables the researcher to assess what marriage and children, health and illness, youth and old age, and so on mean to the people themselves. The fieldworker is both the research tool and the unit of analysis, and involvement is a condition for intersubjectivity.

Observation

Malinowski, who is still revered by present-day anthropologists for his exemplary research among the Trobrianders, stressed the importance of direct observation time and again. The decisive difference between the fieldworker and the armchair anthropologist of his days was that the latter knew only from hearsay, while Malinowski, the fieldworker, *saw* things taking place 'under my very eyes, at my own doorstep' (1922: 8). Malinowski derived his great ethnographic authority from this personal presence at the spot. Geertz (1988: 73–101) has characterized him as an 'I-witness'.

There are two main reasons why Malinowski attaches so much importance to direct observation. In the first place, people usually do not speak about the most ordinary 'facts' of their lives. They are so familiar with them that they do not think of them. They are not worth mentioning and at the same time are hard to describe in words. Malinowski (1922: 18) calls them the 'imponderabilia of actual life'. For the anthropologist, however, they are not at all 'ordinary'. It is only by observation that he can learn about them.

Here belong such things as the routine of a man's working day, the details of his care of the body, of the manner of taking food and preparing it; the tone of conversational

and social life around the village fires, the existence of strong friendships or hostilities, and of passing sympathies and dislikes between people; the subtle yet unmistakable manner in which personal vanities and ambitions are reflected in the behaviour of the individual and in the emotional reactions of those who surround him. (Malinowski 1922: 18–19)

A second reason is extensively discussed in Malinowski's book on sexual customs among the Trobrianders. Some information is not mentioned during conversation or interview because the people are not willing to reveal it. There can be a large discrepancy between statements and direct observations:

The statements contain the ideal of tribal morality: observation shows us how far real behaviour conforms to it. The statements show us the polished surface of custom which is invariably presented to the inquisitive stranger; direct knowledge of native life reveals the underlying strata of human conduct... (Malinowski 1929: 425–6)

He is however quick to excuse the informants. They are not deceiving the researcher. The latter is to blame for his naivité:

it must be made clear that no blame can be laid on native informants, but rather on the ethnographer's whole-hearted reliance in the question-and-answer method. In laying down the moral rule, in displaying its stringency and perfection, the native is not trying really to deceive the stranger. He simply does what any self-respecting and conventional member of a well-ordered society would do: he ignores the seamy and ugly sides of human life, he overlooks his own shortcomings and even those of his neighbours, he shuts his eyes to what he does not want to see. No gentleman wants to acknowledge the existence of what is 'not done', what is universally considered bad, and what is improper. The conventional mind ignores such things, above all when speaking to a stranger—since dirty linen should not be washed in public. (Malinowski 1929:426)

But it is unlikely that Malinowski really got to *see* that dirty linen. His most productive method of breaking through the wall of decorum was a mixture of seeing and 'hearsay', let us call it gossip.

Indeed, most of what anthropologists write about they do not actually see, since their ethnographic work is based mainly on what people have told them. Ironically, moreover, the topics that interest anthropologists most are things that people often do not want to talk about. The more things are hidden, the more they rouse the anthropologist's curiosity. What can be seen every day, and is known to everybody, is rarely the focus of anthropological interest. Few anthropologists write about their own society, and if they do they usually choose what is at the margin and virtually unknown to them. One could almost say that an anthropological research topic by definition is unobservable to the general public. This was certainly true for my research topic, which concerned sexual relationships and birth control.

What then do anthropologists mean by participant *observation*? What they do observe above all concerns the context in which the events that

interest them take place. And, as we just have seen, only by knowing the context do we begin to understand something about the events. The role of the eye in acquiring knowledge is so much taken for granted that we may forget its crucial importance. Seeing the houses in which people live, the land on which they work, their schools and churches, and their shops and their markets—all make a difference. I have seen how mothers bath and cuddle their babies, how children play, how older children take care of younger ones, how men and women move and do not move together, how people socialize and how they fight, and how they mourn their dead, Malinowski's *imponderabilia*. All these observations have helped me to describe and interpret what people do and think with regard to having and not having children.

Furthermore, observations produce questions. That effect too is so obvious that we hardly seem to be aware of it. The eyes pose questions continuously: Who is that person? Who lives in that big house? Whose children are playing together over there? Why does that woman beat her child? What food is this? Who uses this toilet and who cleans it? And so on.

Occasionally, an observation seems to contradict what we already know, or think we know. It may then lead to new questions about the same topic and possibly also to new insights. The previous information may have been untrue or too simple, revealing only 'one side of the coin'. Further observation enables us to correct our information or to reach a deeper level of understanding. In my journal I wrote:

My old landlord has two wives. In the evening they sit together, each in front of her own door, and have a lively conversation. The old man sits with them and takes part in the conversation. I see it every day: a homely scene, a harmonious polygamous marriage.

One night a loud noise wakes me up. Two women are shouting at one another. My crooked door leaves a big split. Through it I can see one of the two wives. The old man tries in vain to calm them down.

It is difficult for me to understand what they are saying. It goes too fast and there are many unfamiliar words in it. I hold the microphone of the cassette recorder in the split of the door and record their 'conversation'. The next morning my assistant translates it word by word: 'You with your crooked ass', 'You are black and dirty', 'You better take your bath in the afternoon rather than in the evening', 'You with your cracked heel', and so on.

The quarrel had causes and consequences which kept me busy the following days. I had to readjust my understanding of the old man's marriage and to change my somewhat romantic ideas about polygyny.

My diary is full of such observations which have enabled me gradually to get a clearer picture of the 'demography' of this village. One could say that such observations helped me to ask better questions, to check and interpret the answers, and to see new connections in my understanding of people's way of life.²

Participation

There is a Chinese proverb which translates roughly as meaning: 'I hear, I forget; I see, I remember; I do, I understand.' It succinctly captures the basic idea of participant observation, in that only by taking part in people's lives can we understand them. But what I previously said about observation also applies to participation. What interests us most are usually the things that are the least accessible and most difficult for us to participate in. In my research these included: marriage, death, witchcraft accusations, sexual relationships, and birth control. Getting involved in a sexual relationship for the sake of research would be a problematic kind of participation, not to speak of its ethical ambiguity. Any sort of participation for the sake of such research reeks of insincerity, and is inevitably both half-hearted and uncommitted. Such participation also produces feelings that are essentially different from those experienced by the people being studied, for whom it is a question not just of participation but of life itself. For the farmer working on the land, his work was a daily necessity, whereas for me the same work was an anthropological experience. The difference between real and artificial experience has been strikingly described by Orwell in his reminiscences of his life as a tramp in Paris and London:

my money oozed away—to eight francs, to four francs, to one franc, to twenty-five centimes; and twenty-five centimes is useless, for it will buy nothing except a newspaper. We went several days on dry bread, and then I was two and a half days with nothing to eat whatever. This was an ugly experience. There are people who do fasting cures of three weeks or more, and they say that fasting is quite pleasant after the fourth day: I do not know, never having gone beyond the third day. Probably it seems different when one is doing it voluntarily and is not underfed at the start. (Orwell 1933)

Having said all this, I still would like to defend participation as the most felicitous research method for anthropologists. It is true that participation is often limited to only a few aspects of life and that it is not 'real', but it is certainly better than nothing. To draw again a parallel with observation, the researcher participates in the context of the study, and by doing so gets closer to it. Sometimes, by chance, it may even prove possible to get in direct touch with it, as actually happened to me.

I fell in love with a girl from the village. It was both an awkward and an exciting experience. Her father was an important person related to the family I stayed with and we feared trouble. We met in secret, and I gradually realized that I was in the same situation as many others in the village. She came to my room in the night, after her mother had left the house. She knocked on my window and I softly opened the main gate of the compound to let her in. Before dawn she left me and joined her sisters in her mother's room. The sisters knew where she had been but kept the secret.

My love affair was never planned as 'participant observation', which is why it could become one. It taught me many things. We talked about ourselves, what we liked and disliked, about our parents, our brothers and sisters and other relatives, and about the past and the future. We also discussed how to prevent a pregnancy, and such matters as which contraceptives were the most effective. Actually getting the contraceptives was instructive. Buying oral contraceptives or condoms from a local store was risky if we wanted to keep our relationship secret. It was safer to buy them elsewhere. In solving these and other problems, I became more and more a participant in the everyday complications of a secret love affair. My relationship with her taught me more about sexual relationships and birth control in that community than the interviews I held.³

But the affair did more. Being involved in a secret relationship gave me a sense of belonging, and seemed to make me more one of them. I am convinced that most—if not all—of the people in my house knew what was going on, but they never referred to it directly. I would not be surprised if some of them watched us, peeping through the splits of their doors. I suspect that they 'secretly' enjoyed witnessing my affair. Three young people in the house were fully informed about our relationship and sometimes functioned as *postillons d'amour*. Even her father once made a remark which was probably a signal to me that he knew what his daughter was doing, but he never took any action.

My impression is that being in this somewhat awkward and vulnerable situation made me more accepted in the community. I compare this to an incident reported by Berreman (1962:10) during his fieldwork in a Himalayan village. When it became known that he, like many other inhabitants of the village, served clandestine alcoholic drinks in his house, his relationship with the people improved considerably. Geertz (1973: 412–17) in his essay on the Balinese cockfight makes a similar observation. During a police raid against the forbidden practice of cockfighting, Geertz and his wife had to run away together with the other spectators. The people appreciated their 'solidarity':

getting caught, or almost caught, in a vice raid is perhaps not a very generalizable recipe for achieving that mysterious necessity of anthropological field work, rapport, but for me it worked very well. It led to a sudden and unusually complete acceptance into a society extremely difficult for outsiders to penetrate. (Geertz 1973: 416)

In the same way, my love affair, which was less secret than I had hoped, helped to get me accepted as a member of the community, especially among the young. It made me seem a 'normal' human being.

I am not suggesting that anthropologists should have love affairs in order to do good research. As a matter of fact, such an affair may well jeopardize the entire research project. A less tolerant community may take offence at the visitor's behaviour and request him/her to leave. Deep emotional

involvement with one person may also block good relationships with others. In some more conflictuous societies, a fieldworker is well advised to keep strict neutrality. I was fortunate, the people in 'my' village were relatively relaxed and liberal in sexual matters.

Good anthropological fieldwork is a question not so much of efficient planning as of flexibility and improvisation. Advances in the research are often strokes of luck, gifts of serendipity. Examples of such 'lucky strikes' can be found in many accounts of anthropological—but also in hard science—research.

What happened to me could be compared to Anja Krumeich's experience on the Caribbean island of Dominica. Her research was on mothers' ideas and practices during pregnancy and their care for young children. At first the mothers were friendly and helpful but somewhat reserved. They felt the anthropologist's questions were a kind of examination and they did their best to give the right answers. Then it was discovered that Anja was pregnant and that a Dominican man was responsible for it. From that moment onwards, the all-knowing anthropologist turned into a helpless young woman who, far from home, had been made pregnant by 'one of those men' and who needed their help and advice. Uninvited, they started to tell her what she should do to protect the pregnancy and have a safe delivery. And when the baby was born in the local hospital, they instructed her how to keep it healthy and how to bring it up in the proper way. Thus, the information she first tried to acquire with so little success was suddenly given to her in abundance (Krumeich 1994).

It was not only verbal information that was offered to her, however. Getting pregnant, having a baby, looking after a child, and fighting with its father enabled her to experience the emotions that are part of motherhood in Dominica. She argued with others about her son's health, she was beaten up by the child's father, and was comforted by the mothers who had become her friends. Participation was no longer a methodological device alone; it had become a personal reality and was part of her own way of life.

Such participation, however, relates uneasily with a time-honoured principle of social research: non-intervention. Non-intervention makes sense in the laboratory concept of research. People are perceived as actors whose behaviour is observed in much the same way as one studies the reactions of mice in a particular situation. Intervention would thus spoil their 'normal' behaviour.

Personal involvement with the people one tries to understand necessarily leads to intervention. It is 'natural' that people exchange information, advise, and help each other. Refusing to do so for academic reasons shows that one is a false participant.

My own life in this Ghanaian community was a mixture of intervention and non-intervention. Looking back, I discern both pretence and sincerity, involving sham as well as true participation during my research. Sometimes I

tried to help people, took part in family deliberations, and gave my opinion, whereas at other times I kept silent, waited, and watched. I seldom tried to influence young people's birth-control practices, even though I saw how dangerous and harmful they often were. In retrospect, I regret this and I am sure that I would have learnt more about these practices if I had tried to change them.

Being a full participant, I realized, was difficult (cf. Bleek 1979, 1980). Moreover, even full participants sometimes wait and watch. One cannot intervene all the time. And, of course, there are boundaries beyond which even local people are not supposed to intervene. What happens in another family, for example, may not be your concern.

Introspection

Anthropological research is bound to have an autobiographical side too. The researcher is likely to look for a part of himself or herself in the community studied and to achieve greater self-knowledge by understanding 'them'. Moreover, each fieldwork experience and each interpretation of data is filtered through the mind and heart of the researcher.

The unavoidable subjectivity of anthropological and much other research was suspect and disapproved of in the not-too-distant past. The researcher was urged to avoid it as much as possible. Nowadays, we tend to see it as an asset rather than an obstacle to good research. The implicit comparison between 'my' and 'their' experience is a prerequisite for understanding 'them'. If we do not recognize anything from ourselves in them, our data will tend to remain stale and meaningless. It would be like reading a novel about people and events which do not touch us in any way; if there is nothing we can share with the characters of the story, and we do not relate to their desires or anxieties, we will take little interest in them and understand them less. We will never finish the book.

Instead of suppressing personal views and feelings, the researcher should examine them carefully and try to use them in conversation, observation, and participation. Through exposure to an informant, a deeper level of mutual understanding and appreciation may be reached. When Desjarlais (1991: 394) asked an old man in Nepal what happens if one's heart is filled with grief, the man smiled and gave the best possible answer: 'You ask yourself.' During my research, I once had a conversation with an old woman and one other person. The topic arose of birth-control practices in the past and we asked her about coitus interruptus. She laughed loudly and asked me rhetorically: 'Can you do it?' I said nothing at the time and so missed a valuable opportunity. If I had said 'Yes', the conversation would probably have become much more enlightening.

Renato Rosaldo (1984) asked Ilongot people in the Philippines why they cut off other people's heads. They answered that the rage coming from

sadness about someone's death impelled them to kill. Cutting off someone's head and throwing it away helped them to overcome their grief. They could not explain it any further. Either you understood, or you did not. Rosaldo admits he did not until he went through similar feelings when he lost his wife. Jackson (1989:4) relates this example in his book, which is one long plea for the recognition of subjectivity in fieldwork. Without subjectivity there is no intersubjectivity. I fully subscribe to the following remark by Lutz and White:

the youth of the typical ethnographer is a liability in the cross-cultural investigation of emotion insofar as limited life experience makes her or him unprepared to understand some things about the emotions of those met. (Lutz and White 1986: 415)

Freud, it seems, produced some of his most incisive comments on the human psyche by reflecting on his own experiences, including his dreams. It is significant, therefore, that his remarks about love are shallow and hardly insightful. He describes love almost as an aberration in which a person loses control of his mind: 'against all the evidence of his senses the man in love declares that he and his beloved are one'. The state of love 'represents at least a partial return to the primal state before the discrimination of self from mother'.⁴ Freud's perception of love is one of an outsider: he admits that he himself never had that 'oceanic feeling', thus implicitly indicating that his understanding of love is incomplete (cf. Suttie 1988: 219-20).

People in the village I studied told me about their loves, about the blessings and frustrations of having children, about their fears of getting pregnant while at school, about the spectre of remaining without children, about their financial worries and concerns, and about their sadness when someone died. Looking back at my research, I realize that I was too young and unexperienced fully to grasp the meaning of their stories. I would have understood their worries better if I had lived through the same things myself. I could then have gauged much better the social and emotional significance of their information by comparing it with my own experience and knowledge.

When Lévi-Strauss suggests that people perceive and order the world by a mental act of binary opposition, I have no urge to check whether this applies to most societies about which anthropologists have written. Rather, when I am evaluating the responses to anthropological research questions, I close my eyes and ask myself: Does it apply to me? Do I use binary oppositions in my life? That is the first and, I think, the most effective way to assess the plausibility of any statement and I assume others do the same. I completely agree with Atwood and Tomkins's statement:

no theorist puts forward definitive statements on the meaning of being human unless he feels those statements constitute a framework within which he can comprehend his own experiences. (cited in Wengle 1987: 368)

Introspection, in summary, is a valuable quality in anthropological research. The researcher should explore his or her own ideas and feelings

while listening to others and reflecting on their stories. The standard question to ask is: What would I do, think, or feel if this happened to me? The underlying assumption is that there is a similarity in the human experience all over the world (cf. Jackson 1989). Of course, that assumption sounds crude and simplistic when expressed in this way and borders on ethnocentrism, but it will take us further in the attempt to understand other cultures than will attitudes that involve distance and objectivity. I firmly maintain that introspection is an inherent part of the anthropological research approach.

Conclusion

My conclusion will appear as a truism to those who have ever practised demographic fieldwork. In the first place, I maintain that survey research alone cannot handle the delicate and complex issues involving people's ideas, desires, and practices with regard to sex and reproduction. More sensitive approaches and more subjective involvement are needed to analyse such issues in any depth.

In the second place, I suggest that the anthropological research approach is indispensable for the interpretation of quantitative data. My purpose in this paper has been to convince readers that there is no safety in numbers alone, and that any apparent safety found in statistics is in fact a mirage. I believe that quantitative and qualitative approaches are complementary. The information provided by the anthropologist is what the demographer needs to know to ask the appropriate, specific questions. Qualitative research helps demographers to count what they want to count. Most of the chapters included in this book illustrate these complementary virtues.

In this chapter I have presented and discussed four basic ingredients of the anthropological approach: conversation, observation, participation, and introspection. Most of the examples I described were derived from my own research on sexual relationships and birth control in a Ghanaian rural community.

By now, there is a long history of anthropologists writing about their fieldwork, but only few of them have included their own experiences with love and sex in their account. This apparent blind spot in methodological reflection was an additional reason for discussing such issues in the present chapter on participant observation.

Notes

1. One of the cultural repercussions of the absence of fathers is beautifully sketched by Bartle (1977: 239), who carried out fieldwork in a nearby town: 'in the compound where I

- lived... the girls would say, let's play *Mame ne mame* (mother and mother) or *Mame ne nana* (mother and grandmother). The adults these children emulated were their mothers and their mothers' sisters... Their socialization took place in a matrilineal rather than bilateral kinship system.' Bartle's dissertation is available in only a few copies, most of which seem to be in Ghana. Bartle gave me his personal manuscript, from which I am quoting. It is possible that the quotation does not exactly agree with the final text of his dissertation.
2. By the way, this incident highlights the ethical aspect of participant observation. Several of my colleagues have criticised the secretive use of a microphone to record the exchange of words between the two women as unethical. I share their concern about the ethics of fieldwork. The direct confrontation between fieldworker and local people frequently results in ethical dilemmas which force the anthropologist to reflect on his or her position in the community. Unambiguous guidelines on how to behave correctly in anthropological research are difficult to formulate. Finding the correct way is very much part of the anthropological exploration itself. In this particular case, I allowed myself to record the shouting. Everybody in the compound heard and understood what the women were saying. I alone was left out, because my command of the language was too limited to grasp the meaning of the words. The cassette recorder was used in this case merely as a hearing aid, and it helped me to hear what everybody else had heard. My action would have been unethical if I had used the recording for another purpose which was against the interests of the people concerned.
 3. My friendship with Kwasi Asante-Darko, a university student, who helped me during the research and shared almost every minute of my life in the community, was another invaluable source of learning. We stayed in one room and did nearly everything together. In our discussions, which sometimes continued late into the night, we talked about everything that had happened during the day. He explained people's reactions to our research, helped me to understand their stories, and showed me the mistakes I had made.
 4. Freud's quotations from *Das Unbehagen in der Kultur* (1933) have been taken from Suttie (1988).

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