Broken limbs, broken lives: Ethnography of a hospital ward in Bangladesh
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Chapter II

THE ETHNOGRAPHY

The human instrument

Shore (1999) pointed out that it is hard to talk about the significance of ethnographic fieldwork in any meaningful sense without being personal. Moreover, ‘The ethnographer is a human instrument... relying on its senses, thoughts, and feelings, the human instrument is a most sensitive and perceptive data gathering tool’, as Fetterman noted (1989:41). In light of these observations, it would be useful to present some personal aspects of my life related to this study before describing the fieldwork, as I am the sole human instrument and the data gathering tool of this ethnography.

After graduating from medical college I took a less-travelled road for a medical doctor in Bangladesh. I left clinical practice and joined a rural health project of Bangladesh Rural Advancement Committee, BRAC a national non-governmental development organization as a public health physician. To my hard-core clinician friends, a public health physician is a lesser kind of doctor. The conventionally successful doctor in Bangladesh specializes in sophisticated subjects like cardiology, ophthalmology and so on, and to work in a big city hospital that provides lucrative income. Generally, to the doctors as well as to their families, a community physician who is working in the rural areas with people’s sanitation, water supply and immunization does not fall within the expectations and ideas about a doctor who has ‘made it’. Though my family was initially disappointed, I was fortunate that they did not oppose my decision to go to a village to work. They were rather worried, how I, their urban-bred son, would cope with the rural life, without a comfortable house with proper toilets, taps and showers, and without telephones, televisions or newspaper.

My life with the rural health project was indeed quite different from the life I was used to. I did completely different work than I did in the hospital. I had to train traditional birth attendants and community health workers, settle disputes with the villagers who were against immunizing their children as they believed injecting vaccine in blood that Allah created is a sin, discuss the relevance of family planning with imams and listen to herbalists who knew the names and uses of hundreds of medicinal plants. These experiences were not comparable with the attempt to hear the murmur of a myocardial infarction patient in the coronary heart disease ward, or to find the right forceps for the surgeon while assisting him at the operation table. I was touched by the multiple facets of the rural people’s simple life. I was moved by the level of poverty, helplessness and oppression. I met malnourished children, hungry and crying while their mothers searched desperately for food for the next meal; I met farmers who were exhausted by the harvest work and felt cheated every time half of his harvest went to the landlord. I also found out that there were lots of things in the village that could take the place of showers, telephones and televisions. The hospitality and warmth of the rural people were more than a match for the creature comforts and impersonal behaviour of the city dwellers. The rivers, sunsets, sunrises, local music, village festivals, cool breeze, clean fresh air, colourful butterflies, chirping birds and majestic scenery offered a different pleasure and recreation. Spending a few years in the village was rewarding for me both personally and professionally. At a personal
level this stay widened my view of life. Professionally, I learned to look at people’s health beyond medicine. What I learned in the village that I did not learn in the medical college was the relationship of health and disease to culture, society, politics and religion.

After spending about five years in the village I went back to the city and earned a Master’s degree in public health. While studying public health, I developed an interest in medical anthropology, a subject that looks deeper into the cultural aspects of health. I decided to further pursue my interest in medical anthropology, and leave the circle of doctors to join the then ‘alien’ anthropologists. By this time, my physician friends considered me to be a confirmed deviant. One even wondered whether I had become interested in insects, as he mistook the word ‘anthropology’ for ‘entomology’.

As there is no institute in my country that offers graduate courses in medical anthropology, I looked for courses abroad. I found the Master’s course in medical anthropology offered by the University of Amsterdam to be well-balanced in applied and theoretical aspects and suitable for me. BRAC, the organisation that I work for in Bangladesh sponsored my study at Amsterdam. I enjoyed the medical anthropology courses.

When I was looking for a topic for my Master’s thesis in medical anthropology, I thought of going back to one of the villages in which I had previously lived to conduct research into some indigenous health care practices or beliefs. This changed once our professor, Sjaak van der Geest, introduced me to an article he had written about an ethnographic research experiment in a Ghanaian hospital involving the admission of his co-researcher as a pseudo-patient (Van der Geest & Sarkodie 1999). The authors showed that it is possible to write an ethnography of a hospital ward in the same vein as anthropologists describe life in a village. The innovative nature of the article fascinated me; I was attracted to the idea of conducting an anthropological study on biomedical practices rather than on a lay people’s health-related beliefs and practices. The paper also stimulated my interest in conducting a thesis on topics related to biomedicine in general, and the hospital in particular, from which I had distanced myself for quite some years while in rural Bangladesh. I thought that after being trained in medical anthropology, I would be capable of looking at biomedicine, which was once my ‘home’, from a different perspective. I therefore decided to conduct an ethnographic study in a medical setting in an effort to bridge between my present academic interests with those of my past: ‘biomedicine’ and ‘anthropology’.

It was, however, not possible for me to conduct research in a hospital during my Master’s program because of limited time and other practical constraints. Instead, I did an ethnographic study in a relatively smaller rural health care centre in Bangladesh established by the organisation I worked for. This study for my master’s thesis, published as a research monograph (Zaman 2001) equipped me to handle the theoretical and practical aspects of conducting ethnographic research in a medical setting. However, as the study was done in a small health centre, I was still longing to conduct an appropriate ethnography in a larger hospital. The chance finally came when my proposal for doctoral research in a Bangladeshi Government hospital was awarded with Netherlands Foundation for Advancement in Tropical Research (WOTRO) funds. I could then embark on a study of the orthopaedic ward in a government teaching hospital.
Describing people in their natural setting

After working for about five years as a public health physician in different rural health centres, I joined the research division of the organization I work for. Most of my colleagues were engaged with epidemiological research; they talked about questionnaires, coding, statistical significance, P-values and so on. As I was fresh from the field, my mind was full of stories, images and words. I was looking for the type of research in which people are not only studied in terms of numbers. The qualitative research papers with descriptions of people’s thoughts, lives and relationships attracted me.

What exactly is qualitative research? Silverman (1993:12) lists some characteristics:

1. The use of everyday contexts rather than experimental conditions
2. A range of sources of data collection (mainly observation and informal conversation)
3. A preference for unstructured data collection (no prior hypotheses, no prior definitions)
4. A concern for ‘micro’ features of social life (a single setting or group)
5. Concern with the meaning and function of social action
6. The assumption that quantification plays a subordinate role

It must be stressed that qualitative and quantitative researches are two ways of looking at the same reality, and each complements the other. The goal of quantitative research is to isolate and define categories as precisely as possible and determine again with great precision the relationship between them. The general purpose is to discover how many and what kinds of people share a certain characteristic. On the other hand, qualitative research normally looks for patterns of interrelationship between many categories. Its aim, as McCracken writes:

Is to gain access to the cultural categories and assumptions according to which one culture construes the world. How many and what kind of people hold these categories and assumptions is not, in fact, the compelling issue. It is the categories and the assumptions. Not those who hold them, that matters. In other words, qualitative research does not survey the terrain, it mines (McCracken 1988:17).

There are some limitations inherent in both quantitative and qualitative techniques. Each is good for studying certain kinds of problems that call for certain kinds of data. I had already done a number of qualitative studies on various health-related issues for the community-based programmes of the organisation I work for using different qualitative tools. I knew that the issues that I was interested in required qualitative research in order to further investigate them. The research for my Master’s in medical anthropology was my first attempt to do an ethnography, one of the principal qualitative research methods in the social sciences. Although ethnography is defined in different ways, the essence of ethnography is considered to be a description of the routine, daily lives of a group of people, with the aim to understand the predictable

Brewer (2000) wrote that ethnography is not one particular method of data collection, but a style of research that is distinguished by its objectives and approach. The definition of ethnography according to him is as follows.

‘Ethnography is the study of people in naturally occurring setting or ‘fields’ by means of methods which capture their social meanings and ordinary activities, involving the researcher participating directly in the setting, if not also the activities, in order to collect data in a systematic manner’ (Brewer 2000:10).

Two things are crucial for studying people in their natural setting: one is to get access to people’s own territory, and the other is to take a role in the community.

*Gaining access*

In my case, the ‘field’ was a hospital. Others who have done research in hospitals frequently mentioned that gaining entrance into a clinical institution can be a problem. Mathews mentioned two reasons for the potential difficulties:

Firstly, medical practitioners are reputed to resist analysis by outsiders. This resistance results partly because physicians historically have resisted any external review or regulation of their professional prerogatives (Friedson 1970) and also because social science analysts often have engaged in what McKinlay (1977:495) calls, “doctor bashing” or deprecating physicians. Secondly, the typical hospital is composed of several semi-autonomous administrative units that can control access to informants either formally as through a research review committee, or covertly through denial of access to particular areas, activities or potential informants (Mathews 1987:295).

Van der Geest (1989) discussed the cases of social science research that took place in a biomedical setting and resulted in conflict when the data were published, such as a book published in the Netherlands about a cancer hospital that was banned and destroyed by court order. Similar conflict occurred with the publication of research results from a hospital in Denmark (Hensen 1991).

The problems I faced to gain access to a government hospital were of a different sort. As an employee of a non-government private development organisation, it was difficult to get access to a government institution due to the uneasy government-NGO relations that prevail in the Bangladesh. Bangladesh depends on international donors, and in many occasions the international donor agencies prefer to provide funds to NGOs rather than government ministries because of the relative success of NGOs in managing various development projects. The press also regularly criticises the relatively higher rate of failure of a multitude of government projects due to bureaucracy and corruption, in comparison to the rate of failure of NGO projects. On the other hand, NGOs need permission from the government to implement any foreign-funded project. As a result, there is a hidden tension between the government and NGOs relationship (Khan 2001:218). Some of my colleagues at BRAC who wanted to study government institutions were either denied access or were given limited
permission. One study on field level government health staff done by my colleague (Chowdhury 1990) raised much debate, because it showed poor motivation and performances of field level government health staff.

However, I knew if I could use my physician identity I would be able to overcome the antagonism of the medical institute that other social scientists or NGO personnel usually face. I therefore decided to do research in Chittagong Medical College, the teaching hospital where I was trained as a physician, as I would be able to ignore my affiliation with the NGO and utilise my identity as an ex-student of the institute to gain access.

As per the tradition of single-sited ethnographies, I decided to do my research in one particular ward of the hospital. I knew that if I could get permission to do the research through informal negotiation from the professor and the head of the ward, then it would be easy to avoid the complicated administrative procedures of seeking permission from the ministry or hospital administrator. But it was still difficult to make the initial contact. I had left the medical college and the city immediately after my graduation, more than twelve years back. At the time I wanted to begin this research, I was living in Dhaka, more than 300 kilometres away from the city where this hospital was. Moreover, the professors whom I knew as a student had all been either transferred to other universities or had retired. Nevertheless, I still had a few friends in the city whom I occasionally visited. There was one senior physician friend in particular with whom I had regular meetings, because we shared a common interest: literature. When I was a student, he lent me books of Dostoevsky, Kafka and Garcia-Marquez. I decided to share my research ideas with him. He was not aware of hospital ethnographies, but he knew of a number of famous literary works based on hospital life. We talked about Chekov's Ward Number Six, Thomas Mann’s Magic Mountain and Alexander Solzynitsin’s Cancer Ward. He was enthusiastic about observational research in a hospital. He told me that the professor of the orthopaedic ward was a good friend of his, and that he might also be interested in the research. The professor, he said, had passions other than medicine.

This friend introduced me to the professor, who was happy to hear that one of the alumni of the medical college was pursuing a Ph.D. at a prestigious European university. I gave him a general view of my research. He showed interest and said: ‘The social life of hospital is always an interesting thing. We are busy with technical things; someone should concentrate on these issues. You are one of us. You are welcome to do your research here. The journalists regularly write stupid things about hospitals, they know nothing about the medical world. I hope you will not just write how bad we are.’

I was relieved to hear his positive response, however I knew that there was a danger for the research in his belief that I was ‘one of them’. He might not have realised that he himself and his colleagues would be among my subjects of observation. His first statement has already been considered as data. The professor told me that I should not worry about contacting the administrative director of the hospital; his approval was enough for any academic activity concerning his ward. He assured me that if any problem arose, he would take care of it.

Thus through my identity as a physician and through a proper intermediary I solved the problem of getting access into the ward, a major problem researchers face when conducting observational research in a hospital. Though selecting the orthopaedic ward was a coincidence, I later realised that it was very appropriate for my
research, as this was a very eventful ward with lots of happenings and interactions within and outside the ward.

Taking a role

After gaining access to the ward, the next step was to take on a role in the ward. In some cases it may be beneficial to disguise the researcher's identity to get more reliable information. One of my research supervisors in the Netherlands, Sjaak van der Geest, suggested that I think about becoming a fake patient in the hospital, like his co-researcher did in the aforementioned research experiment in a Ghanaian hospital. There are other cases of researchers becoming pseudo-patients in a hospital; the examples of Caudill (1958) and Rosenhan (1973) becoming patients in a psychiatric hospital are well known. In another study a researcher of the team became a fake patient in a rehabilitation hospital (French et al. 1972). There are also examples where the researcher even changes their physique for the sake of the research. A researcher who studied the training of soldiers in the USA underwent minor plastic surgery and lost more than 15 kilograms to look younger so that he could infiltrate the training (Sullivan et al. 1952). This kind of hidden participation is practiced in situations where participation in the ordinary sense was not possible, because the people concerned would not allow outsiders. Examples of this type of research include, but are not limited to, research on drug users, criminals, religious believers, political activists and prostitutes.

Although initially the idea of becoming pseudo-patient seemed exiting to me but finally I did not follow Van der Geest’s suggestion. Acting as a patient in a psychiatric hospital or even in a general ward may be easier than acting as one in an orthopaedic ward, where one needs a broken leg or arm to be admitted. This does not mean that I learned less than I otherwise could have, for in the conclusion of their research experiment in a Ghanaian hospital, Van der Geest and Sarkodie (1999) discussed how they had not learned anything by being a fake patient that they could not have learned if they would have revealed their identity. They claimed that the main lesson to be drawn from their experiment is that the researcher, either as a patient or as a scientist, should find a way to be in the ward continuously day and night.

Additionally, there are ethical issues concerning concealed identity during research. There is a wide range of views among social scientists regarding the ethical principles in conducting social research. Some outright reject any form of deceit in research; some have more liberal views that the end justifies the means (Bulmer 1982). I would quote Caudhill (1958) in this connection, who did research in the same hospital twice, once as a fake patient and once as a social scientist:

In the first I acted as a patient, and my identity was concealed. The second and the fuller study, reported on here, was begun a year and a half later, and in it I presented myself openly as researcher anthropologist. From the published accounts of both studies the reader may judge which was the more successful. In the report of the first study, mention has already been made of the ethical problems posed by such a study, along with the suggestion of the feasibility of alternative procedures. I have no wish to rationalize my actions in this earlier study. I did it, and I learned much from it. I do not recommend others doing it because I feel the price is too high. The ethical questions here are rather tricky,
some of the purposes of almost any research project on human behavior are concealed from the subject, client or patient, and this is too involved a topic for a preface. Certainly one factor, however, is how comfortable -- morally and emotionally -- a person is about the matter of concealment. Once in the situation, I felt decidedly uncomfortable (Caudhill 1958: xiv).

The thought of occupying a bed in an orthopaedic ward as a fake patient made me morally and emotionally uncomfortable, especially when severely injured patients are denied admission because of a shortage of beds. I therefore didn't see any reason to disguise my identity in the ward. I planned to appear in the ward as a researcher. One day, the professor took me on his round and introduced me to his junior colleagues and other staff members and told them all that they should co-operate me regarding the research I was planning to do in the ward. That greatly facilitated my access to various spaces, papers, records and staff of the ward.

**Data collection techniques**

Participant observation, the fundamental method of ethnography was my main technique for data collection. Singha (1993:124) described four possible roles of a participant observer, (1) complete participant, (2) participant as observer, (3) observer as participant and (4) complete observer. It is obvious that I was neither a complete participant nor a complete observer. The role of participant as observer demands that the researcher be a participant in one of the social groups in the research setting, and appears as such to persons outside that group. But I did not participate as a member of any group, so this role did not apply for me. According to the categories therefore my role was the only remaining one: observer as participant.

However, many authors even doubt the very term 'participant observation'. Van der Geest and Sarkodie wrote:

> [P]articipant observation is not an easy thing to do, or, to be more precise, it is impossible. Participant observation is a dream, an ideal, and a *contradictio in terminis* (Van der Geest & Sarkodie 1998:1373).

Rabinow noted:

> [N]o matter how far ‘participation’ may push the anthropologist in the direction on non-otherness, the context is still ultimately dictated by “observation” and externality. (Rabinow 1977: 79).

In my case, I was basically an observer and my participation was participation in the sense that I was present in the scene of the ward as a social being, watching, observing and talking to the people of the ward.

Collecting data in ethnography through heavily interpersonal methods is a labour-intensive process. As Hamersley and Atkinson stated: ‘Observation in the social world is rarely as straightforward as reading a thermometer’ (1983:17). Ethnography is not as clear-cut as other hypothesis testing research is. Anything around that affects the researcher’s senses has the potential to be considered as a piece of data. Unlike
structured observation or interviews, in which the researcher extracts data from the respondent, the ethnographer selects data from the naturally occurring flow of life.

The idea that we collect data is a bit misleading. Data are not 'out there' waiting for collection, like so many rubbish bags on the pavement. For a start they have to be noticed by the researcher, and treated as data for the purpose of his or her research (Dey 1993:55).

For the convenience of data collection, I divided the information into three categories: place, people and events. During my first month, I gathered information about the background of the hospital and the ward, its routine and organisation and observed different medical procedures and social interactions in and around the hospital and the ward. After the first month I began to give special attention to specific people and events. I followed different staff members and observed their activities and interactions throughout the day. Then I followed activities and interactions of specific patients and their relatives. After that, I concentrated on observing various events, like the professor's rounds, patients' deaths, teaching sessions, the doctors' tea sessions, procedures in the operation theatre and so on.

I did these observations in different phases. Each phase lasted for one to two weeks, followed by a break, during which I returned to my home and work place. In the initial phase I spent the greater part of the three shifts (morning, evening and night) in the ward. But in the later phases, I mainly spent a particular shift in the ward by rotation. The total time spent conducting fieldwork was about five months in between November 1999 to September 2001. During that time I also visited the Netherlands to attend courses and to discuss my progress with my supervisor.

I used three notebooks. One was a jotting book, one a field notebook and one a memo book. The jotting book was always in my hand, and I took short notes openly during my observation. Sometimes I elaborated on the observations made in my field notes while in the ward. Several times a day, I elaborated on the observations made during the preceding hour or two by retiring to one of the staff rooms. Sometimes I did this elaboration at night after returning home. In the memo book, I wrote my reflections on each day of observation. In that book, I kept notes about the significant observations, new ideas, or missing observation and things to be done next. I always tried to make sure that I finished writing the detailed observation and memo each day.

I gathered most of my information through informal conversations and discussions. I talked with all the staff members and selected patients and their relatives. There were short term and long term patients. So, while the staff members that I interacted with were fixed, the patients changed. In addition to observation and informal conversations, I did case studies of fifteen particular patients and their relatives. I had in-depth interviews with the following staff members: the professor, the assistant professor, the clinical assistant, the casualty medical officer, two intern doctors, the nursing supervisor, two junior staff nurse, two ward boys, two cleaners, and two gatekeepers. I did not generally use any recording device, except during a few of the in-depth interviews. A final source of information came from consulting different registers and files in the ward and in the administrative office.

Focusing my attention on the three kinds of topics as explained above (places, people, and events) proved an efficient way to organise the complex world of my
research subject and data collection tools. The following three tables (Tables 2.1-2.3) summarise my data collection plan.

**Table 2.1: The places**

<table>
<thead>
<tr>
<th>Places</th>
<th>Issues</th>
<th>Data collection tools</th>
</tr>
</thead>
<tbody>
<tr>
<td>Hospital</td>
<td>History of the hospital, existing facilities, staff strength, staffing patterns, physical environment</td>
<td>Observation, secondary sources, interviews</td>
</tr>
<tr>
<td>Ward</td>
<td>Floor plan, existing facilities, available staff, staffing patterns, architecture, physical environment</td>
<td>Observation, secondary sources, interviews</td>
</tr>
<tr>
<td>Staff rooms</td>
<td>Number of rooms, location, room arrangements, facilities inside the room, physical condition, the activities and interactions that take place in the rooms</td>
<td>Observation, conversation</td>
</tr>
<tr>
<td>Operation theatre</td>
<td>Facilities, physical condition, activities and interactions</td>
<td>Observation, conversation</td>
</tr>
</tbody>
</table>

**Table 2.2: The people**

<table>
<thead>
<tr>
<th>People</th>
<th>Issues</th>
<th>Data collection tools</th>
</tr>
</thead>
<tbody>
<tr>
<td>Patients</td>
<td>Social background; type of cases; daily activities; communication with the hospital staff, other patients and relatives; dealing with the treatment; communication outside the ward; views about the ward, the staff and the treatment</td>
<td>Observation, conversation, case studies</td>
</tr>
<tr>
<td>Relatives of the patients</td>
<td>Relationship with the patient, activities, interactions with the hospital staff</td>
<td>Observation, conversation, case studies</td>
</tr>
<tr>
<td>Supporting staff (ward boys, cleaners, ayas, gate keepers)</td>
<td>Activities of different supporting staff; interaction with staff members, patients and relatives of patients; views about their profession, their work and the people around</td>
<td>Observation, conversation, in-depth interviews</td>
</tr>
<tr>
<td>Doctors and nurses</td>
<td>Activities of different doctors and nurses; interaction with staff members, patients and relatives of the patients; views about their profession, their work and the people around</td>
<td>Observation, conversation, in-depth interviews</td>
</tr>
<tr>
<td>Administrative staff</td>
<td>Their role and activities; views about their profession, the hospital, the staff and patients</td>
<td>Observation, conversation, secondary sources</td>
</tr>
</tbody>
</table>

**Table 2.3: The events**

<table>
<thead>
<tr>
<th>Events</th>
<th>Issues</th>
<th>Data collection tools</th>
</tr>
</thead>
<tbody>
<tr>
<td>Professor's round</td>
<td>Procedure, interactions</td>
<td>Observation</td>
</tr>
<tr>
<td>Operation</td>
<td>Procedure, interactions</td>
<td>Observation, conversation</td>
</tr>
<tr>
<td>Admission and discharge</td>
<td>Procedure, interactions</td>
<td>Observation,</td>
</tr>
</tbody>
</table>

25
Native among the natives

*Anthropology at home*

It has been a common trend in traditional anthropology for anthropologists to describe their adventures, troubles and joys of ‘going native’ (Diamond 1974, Evans-Pritchard 1976, Leonard 1990, Mead 1978, Watson 1999). When anthropologists study a culture other then their own, they write about critical experiences regarding the process through which they made themselves familiar with the previously unknown culture. However, the paradigm is gradually shifting; anthropologists are becoming more and more interested in studying their own culture, and conducting anthropology at home. Van Dongen and Fainzag (1998) mentioned a number of reasons behind this ‘homecoming’, particularly for Western anthropologists. They argued that the primary reason is the end of the colonial era, and with it, suspicion by many African and Asian states of neo-colonial intellectual imperialism. Secondly, decreased funding and increased student numbers makes it more difficult to afford study abroad. Finally, an enhanced critical awareness which has exposed the biases in anthropology as a discipline which studied ‘others’ as ‘cultural’ beings, but overlooked its own cultural foundations and cultural dimensions at home. As a result anthropologists are increasingly becoming ‘native among the natives’. Patton (2002:84) also discussed about the emergence of *autoethnography*-studying one’s own culture and oneself as part of that culture. Although it is set in a non-Western context, my ethnography falls into this trend of anthropology ‘at home’; my ‘nativity’ was two-fold. I am a Bangladeshi doing my fieldwork in Bangladeshi society and I am a trained medical doctor studying a hospital, the domain of doctors.

Let me now mention some of my experiences of doing fieldwork in such a two-fold ‘home’. I have already outlined some aspects of my personal background; here I will mention how that affected my fieldwork. After all, the post-modernist and feminist critique of ethnography makes it more logical to depict the ‘self’ in ethnographic writings (Fonow & Cook 1991, Shore 1999). Shore noted:

> Anthropologists are not like ‘detached’ scientists studying the behaviour of rats from outside glass cages; we are positioned subjects within those fields and should therefore be ‘objects’ of anthropological enquiry as well (Shore 1999:45).

*Being a doctor*
To find a researcher in a similar situation, I looked for other physician-anthropologists. I found some, such as Farmer, Frankel, Hahn, Hilfiker, Konner, Lewis and Manderson. All, however, are Western physicians studying various medical aspects of either their own country or of those abroad. I was disappointed to not find any work by a non-Western physician doing ethnography in a non-Western hospital. I had hoped I was not alone in this endeavour.

Physician-anthropologists Lewis and Frankel argued that anthropologists who are not medically trained fail to adequately consider the practical dimensions of healing and illness. They pointed out that anthropological accounts of health and illness have been overly concerned with belief rather than practice and too focused on theory, ideas, semantic content and symbolic interpretation, rather than on action and behaviour (in Bolton 1995:1656). Bolton further argued that physician-anthropologists are better positioned to avoid various biases common to anthropological investigations of biomedical practice. He pleaded to the anthropologically trained doctors to write more about medical practice:

The tiger’s experience of tigritude is different from those who hunt tigers and write about tigritude; the more tigers who write about being tigers the better we will understand both tigers and tigritude (Bolton 1995:1660).

Even though I am technically a ‘tiger’ (doctor), I cannot fully enjoy the advantages of being a ‘tiger’, as I had been away from real ‘tigritude’ for many years. As I mentioned earlier, although I graduated from medical college, I did not practice clinical medicine in the conventional way. The last time I had spent a considerable time in a hospital had been more than twelve years back, when I was in medical school. At that time, I had spent only a couple of weeks in the orthopaedic ward while I was doing my internship. Afterwards, I rarely visited hospitals, and if I did, it was only briefly to see a sick relative or a friend. Fortunately or unfortunately, I have never been hospitalised myself.

After finishing medical school, I became interested in public health and medical anthropology. This meant that professionally, I was either engaged in providing primary health care to the villagers or conducting research and writing reports. My professional interactions were mostly with health policy makers and public health managers rather with medical doctors engaged in clinical practice. Yet even then, I was not far away from tigers; in those reports I sometimes hunted the tigers. Through all of these experiences, I had also developed a critical outlook on various dehumanising aspects of medical practice in Bangladesh.

As a result, the clinical environment of hospitals had become a distant world for me. Therefore, even though I am a trained physician, I do not consider myself a real member of the in-group of hospital culture; I was more a tiger that had remained away from the jungle for several years who has returned to observe tigritude. Although my schooling in medicine might cause me to put a greater focus on the practical aspects of medical practice, I may not be completely free from the biases common to anthropological investigations of biomedical practice that Bolton wrote about.

Thus, my position was paradoxical. I was both an insider and an outsider of the hospital world. I embodied both the advantages and disadvantages of this position. There were some practical advantages, for example, as I mentioned earlier that my identity as a physician and ex-student of the medical college greatly increased my
chances of the difficult job of acquiring permission for the research. This also enhanced my credibility as an observer of a clinical situation and helped dispel perceptions that physicians and nurses have about other social scientists’ or journalists’ intent to expose, criticise or disrupt the clinical system. Moreover, because of my training in medicine I could easily understand the meaning of actions in a hospital. I am sure that someone wholly unfamiliar with the medical world would spend months trying to understand the basic elements of a hospital setting. But again, while my knowledge of medical practice greatly reduced the time required for me to complete my fieldwork, it might have clouded my eyes at points and caused me to take certain things for granted.

Doctors were generally confused about the nature of my research. They were curious about my endless note taking and conversations with people. One junior doctor asked me: ‘I was wondering -- what did you talk with that patient about for more than an hour?’ Initially, the doctors thought that I might be interested in various remarkable orthopaedic cases, and drew my attention to them whenever they encountered such cases. However, they soon realised that I was not much interested in the clinical aspects of the patients. I explained to them that I was trying to know how the patients experience their stay at the hospital or how the staff members feel regarding their work. Some doctors had cynical reactions towards such research. One said: ‘What is the use of knowing such things? How it is going to help us?’ Some did acknowledge the value of such research, but they too wanted to know what the applied implication of such research was. They were curious about the facts and figures of the hospital, rather the stories of the patients.

When writing about Clinically Applied Anthropology (CAA), Phillips (1985) emphasised that its goal was to translate the understandings of anthropology for clinicians so that their service could be more humanistic, holistic and culturally sensitive. He commented that clinicians are generally unreceptive to the theoretical constructs of the social sciences; they want to know what to do and how changes in their behaviour will improve outcomes for their patients. He thinks CAA’s contribution to medical care settings has been limited by the epistemological incompatibility of medical anthropological theory with the dominant medical ideology. In another paper on the similar topic Mering wrote:

To the clinician, the shadows of reality -- e.g. X-rays -- constitute essential evidence for a necessary cause of knowledge events. To the anthropologist, the reality of shadows -- e.g. meanings -- is admissible evidence of multiple contingencies in human behaviour (...) To the physician, facts must be demonstrated, to the anthropologist, meaning must be interpreted (Mering 1985:71).

I was not in the ward to change doctors’ behaviour or views; nevertheless awareness of the difference in the mindset of doctors helped me to maintain workable communication. Sometimes I received information partly because the doctors considered me as ‘one of them’, someone with whom they did not mind sharing the ‘secrets of the house’.

On the other hand, my identification with doctors also generated problems. For example, the nurses and particularly the lower staff members tried to maintain some distance from me. Initially, they thought I was sent by an external organisation to
evaluate the ward, to see if everything went as it should. I could sense their alertness in my presence. Only the nurses became curious enough about my note taking to ask me about my research; the other lower level staff kept a distance, keeping with typical patterns of interpersonal interaction in Bangladeshi hierarchical society. I therefore had to make an extra effort to build a rapport with them. Whenever they had a free moment in their busy day, I would converse with them. I frequently sat in the nurses' room or went to the veranda to talk with the gatekeeper, cleaner or ward boy. While talking with them I had to be cautious in my language and gestures, so as to not appear threatening to them. I was successful to a certain extent in bridging that distance as the gatekeeper, cleaner and ward boys told me their personal stories in the later phases of my fieldwork. They never did consider me as one of them, as the doctors did. As a result I might have not received some information that is their 'secret of the house'.

My identity was ambiguous to the patients. They thought I was someone from the hospital but were not sure about my role. Many patients told me that they first thought I was a doctor, but after they did not see me engage in any medical activity, they became confused. Because I was always taking notes and talking with them, many of them thought that I was a journalist. This helped in a way, for such a role encouraged them to express all their distress and complaints about the hospital to me. I found the patients liked to talk to me and were surprised when they met someone in the hospital who gave them so much time. A number of patients told me that talking to me was a relief of their boredom.

My physician identity also created some problems in gaining information from the doctors. As the doctors considered me to be one of them, I could not ask them 'innocent questions' about certain procedures or acts. They thought I knew the answer and that either I was joking or trying to be critical. Once, when I asked the register why most of the patients' history sheets were incomplete, he replied: 'Why ask me? You worked here long before me.' I did not remember details of the procedures of the ward that I had followed for a couple of weeks twelve years back. However, it was not easy to inquire about those. Similarly, it was not easy to ask the doctors about their feelings or opinions about various aspects of the ward.

I therefore had to find other ways to gather information. I realised that the simple act of asking questions might disturb the way things work at the ward. I decided to proceed without asking anything, but simply following their spontaneous discussions and gossips, their actions and verbal and non-verbal communication. These provided me with answers to lots of questions. Collecting data is like catching a butterfly; if you run after it, it flees, but if you sit quietly, the butterfly sits right on your head. I gradually learned how it is possible to gain a lot of information just by 'hanging out'. As Bernard wrote: 'Hanging out builds trust, and trust results in ordinary conversation and ordinary behaviour in your presence' (1994: 152).

This is not to say that I never asked any questions. I had to ask questions in many occasions and had to formulate the questions in a way that did not raise any suspicions. Sometimes I simply raised an issue during a gathering of doctors or other staff members in order to generate discussions, which I then used as my data. I also conducted formal interviews. However, I must agree that I always felt a latent anxiety in this process of 'crafting relations' (Coffey 1999) and 'manufacturing distance' (McCracken 1988).

I came to appreciate the dynamic nature of clinical work. I realised the stress and pressure on the staff members who must work with such limited resources and
manpower. Sometimes, for example when I saw the broad smile of the patient who had just had his half-amputated finger fixed, I realised how difficult it is for a public health physician who works in preventive health to bring such a immediate satisfaction to his clients, and for a moment, I missed clinical medicine.

Sometimes just hanging out and taking notes appeared inadequate to me in the midst of life and death events that were taking place in the ward. In a few cases, I crossed the boundary of being just an observer and drew the doctor’s attention to a certain unattended case, explained an X-ray to the patient who did not dare to ask the duty doctor about it, consoled the relatives outside the operation theatre and explained to them what was happening inside or gave some very poor patients money. I also presented a drill machine to the operation theatre, as the one from the hospital ward was out of order and it was taking weeks to complete the formalities to purchase a new one. It touched me deeply when patients, their eyes filled with tears, told me how devastated they were because of the hospitalisation. I realised how much I had overlooked when I was an anxious student and a busy doctor in this hospital decades ago. I agree with Agar who called the ethnographer a ‘professional stranger’. He wrote: ‘Ethnography, whatever else it is, is an experientially rich social science’ (1980:6).

Being a Bangladeshi

Van Ginkel remarked that the problem of the anthropologist doing research at home is how to get out of his culture while his colleagues working in a foreign culture struggle to get in (1994:12). I was again simultaneously in and out of the culture that I was studying. While doing the ethnography, my familiarity with Bangladeshi culture definitely generated a ‘lack of cultural shock’ (Jaffe 1993). But the research field was also not completely devoid of surprise for me. After working for more then a decade as a public health physician for an NGO, busy with writing reports on sanitation, immunisation and family planning, the overcrowded, chaotic, government hospital ward crammed with severely injured blood-soaked emergency patients was shocking for me. Because of the small-scale and target-oriented project activities, staff accountability is strictly maintained in the NGOs. This made the poor accountability of the staff members of the hospital striking for me, as was the extremely unfriendly behaviour of the doctors and staff members towards the patients and their relatives.

While doing the fieldwork I actually felt myself to be ‘thrice-born’ as described by Srinivas (in Nanda 1987:16). He wrote that we are first ‘born’ into our own particular culture. Our second birth occurs when we move away from this familiar place to a different world. Our third ‘birth’ happens when we return to our native place and find that the familiar has become exotic. After my first birth as a clinician in a government hospital, I moved away to the world of NGO, public health and anthropology to be born for the second time. I felt I had my third birth when I again turned towards my first home, the government hospital.

Despite this contrast in accountability and discipline in the work place, there are deeply rooted cultural traits that are common to both an NGO and a government institution in Bangladesh, such as the hierarchy in interpersonal relations. Therefore, despite some shocks in the hospital, I had the advantage of familiarity of Bangladeshi culture to help me to share in and understand the reality of life in the hospital both faster and better. I knew the language and even the dialect of Chittagong, as I lived and studied in that city. When the people of the hospital told me their stories, I could
understand immediately the meanings of important events, persons and places. Anthropologists who study other cultures go through a fairly long and painful period of adjustment during which they accumulate knowledge, competence and sensitivity. Being an insider greatly reduced the cognitive and emotional efforts necessary to adjust to an otherwise foreign culture.

Because I was native to Bangladesh, my presence in the ward did not indicate anything unusual, unlike in the case of a ‘white’ anthropologist in a ‘coloured’ population. Moreover, during most of the day, the hospital ward is a public place where visitors meet their sick relatives, except during the professor’s round. During the professor’s round, regular part-time trainees from other public and private hospitals join in. So, during the visiting hours I was one among the crowd of relatives, and during the round I appeared as one of the trainees. In addition, the wearing of a white coat by the doctors during duty is not strictly practised; they mostly wear their regular clothes when in the ward. Thus, I did not have to worry much about not participating in the symbolic meaning of white coat, unlike Coser, who found that wearing a white coat assured her entree in an American hospital (1962). I also doubt that my presence affected the behaviour of the staff members much. It is such a busy crowded ward that they often did not have time to pay attention to what I was doing. Initially there was some alertness and curiosity about my work, though after a while it seemed my presence was taken for granted. Vermeulen, a Dutch nurse and sociologist, who studied a Dutch neonatal ward also wrote: ‘Being at home in the field makes it possible to observe without giving the staff members the feeling that an outsider is looking at them’ (1998:7).

When doing fieldwork in one’s own culture, a researcher runs the risk of developing ‘over rapport’ with the research subjects, which might hamper in gaining authentic information. I think I was free from that risk, because unlike the anthropologist in a small-scale society who lives and works twenty four hours a day with an unchanging population, my encounter with the hospital staff was confined to working hours and the patient population was always changing. Moreover, as Van Dongen (1998) argues, being an anthropologist at home does not mean that the identities of the anthropologist and the people in the field are similar. People and cultures are diverse, different and divided from within. As a result, the familiar becomes unfamiliar to the researcher as a matter of course.

But if this diversity of identity of the natives saves the native anthropologist from developing over rapport, it raises another question. Which among this diverse reality does the anthropologist actually see? I am an urban, middle class male Bangladeshi, does that not affect the way I see Bangladesh as well as the hospital? Am I therefore really a native among the natives?

Naive among the natives?

So, maybe I was a naïve among the natives. When I started writing my dissertation I constantly asked myself, how I am going to report my observations? How am I going to present Bangladesh, my country, the medical profession, of which I am somewhat a part? Am I going to write another account that shows how bad things are in Bangladesh or am I going to defend the country and the profession? By being Bangladeshi, did I take many things in the field for granted? Did my medical training cloud my ears, eyes and nose to the ways of the hospital environment? If the hospital is
a mirror, which Bangladesh did my 'elite' eye see in that mirror? Was I a proper 'objective' observer?

In response to these questions, I can only say that throughout my writing I have always tried to be self-critical, candid and reflexive. I sometimes felt restricted in the language that I could use because I was a non-native English speaker (De Swaan 2001), and because of the inadequacy of post-colonial anthropological theories (Asad 1998). But as rightly argued by Bernard (1994:152), who is, after all, objective in this world? No human being can be completely objective. We cannot rid ourselves of our experiences. We can however, become aware of our experiences, our opinions and our values. As a result, bias is a human condition, a danger for both insider and outsider researcher. The ethnographer will therefore always be somewhere on ‘the continuum of empathy and repulsion, home and strangeness, seeing and not seeing’ (Ellen 1984:130). As Evans-Pritchard noted:

In any case one always remains oneself, inwardly a member of one's own society and a sojourner in a strange land. Perhaps it would be better to say that one lives in two different worlds of thought at the same time, in categories and concepts and values which often cannot easily be reconciled. One becomes, at least temporarily, a sort of double marginal man, alienated from both worlds (Evans-Pritchard 1976:243).

Finally I agree with Van Dongen and Fainzang when they write: ‘Just as distance is not a guarantee of objectivity, familiarity is not knowledge’ (1998:247).