ENCOUNTERING THE FIELD: A MIXED METHOD STUDY IN A REFUGEE SETTLEMENT

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ABSTRACT

Since 2005, I was pursuing my PhD. Research project with an in-depth study of refugee life—everyday experiences of people as refugees, their struggle for relocation, and the dynamics of social relationships as reflected through oral narratives describing the course of refugee hood, from dislocation to relocation. My research intended to bring forward into consideration the historical struggle of the East Pakistani Hindu Bengali refugees to establish their right to home in an alien land with a focus on the role of their women. This article provides an account of methodological aspect, underscoring specific situational and methodological problems that arose during the Field work. The article describes the a brief account of quantitative–qualitative methodology adopted and depicts the particular challenges I faced in encountering the field, in qualitative interviewing, and in using oral history interviews to elicit reliable testimony about everyday performances occurring more than six decades before the interview. It also highlights how the presence of an interviewer may interfere with and has to be negotiated in the field, with special reference to sociologist in a native setting, and gender reflexivity.

Key Words: Setting/locale, oral history interview, ethnographic imagination, memory, everyday, gender, sociologist in native setting

Project Overview

intended to explore the issue of ‘Refugee hood, Gender, and the Dynamics of Relocation’, with specific focus on one of the unauthorized refugee settlements, namely Deshapriya Nagar Colony of North 24 Parganas in West Bengal, India. My objective was to understand the following: what refugee hood really meant for those who had experienced it in their everyday life; how the refugees had traversed the long
route starting from dislocation to relocation, and whether they really feel relocated; whether and to what extent the long run of struggle—the united struggle of refugees as a community—had been an agent of transformation to their family life, role relations, socio-economic position, social and societal interaction and status, and above all, in perceiving and performing gender by them as a whole. I needed the micro-histories of partition and refugee hood directly from those who had lived through it and resorted to penetrate refugee memories and collect and analyze individual oral accounts, and personal documents and relics, which, in turn, revealed a different account of partition and refugee hood from that recorded in the official history and national narratives relating to the issue.

Methodology: Qualitative Interviewing in Combination with Quantitative Techniques - A brief outline

The partition of the Indian subcontinent in 1947 and its consequent refugee hood are the particular subjects that have intrigued me since my childhood. Although I belong to a native West Bengali family by birth, I spent many of my childhood days with friends, peers, and relatives belonging to the refugee families and living in the unauthorized refugee settlements.

Now, I find that some of those memories and experiences have left a deep impression in my mind. Perhaps it is this nostalgic part of my mind that launched my interest for this academic endeavour on refugee issues. My personal experiences and passions combined with scientific curiosity of a sociologist, and reading of scholarly works of some eminent writers and stalwarts in refugee studies and gender studies, helped me to find the gap between the official accounts and traditional meta-narratives about partition and refugee rehabilitation and the real veracity of living refugee life. Therefore, the principal research questions of this project were developed, centering on the perception of refugee hood, relocation, and the role of gender in the dynamic path of refugees’ struggle from dislocation to relocation; the answers to these questions were sought by focusing on a particular refugee settlement.

I had to select possible research sites (several refugee settlements or colonies in North 24 Parganas) and visited, met, and interviewed several people in those colonies; visited colony-committee offices; and talked with several responsible persons to assess and compare the relevance of those squatters' colonies as the setting for my research purpose and above all to compare the possibilities of carrying out my research there. Finally, I chose Deshapriya Nagar Refugee Colony as the locale for the present research study considering several reasons:

1. There I could find and communicate with several first-generation refugees.
2. The colony bore all the features and experiences that could be associated with a refugee settlement of West Bengal, for example, forcible occupation of land, struggle against eviction effort, everyday struggle of the refugee settlers for upholding livelihood in a strange new land, the active involvement of the colony dwellers in refugee movement, and the existence of refugee market with the Left-dominated politico-social environment and suchlike.
3. Access to this colony people would be easier for me than other colonies, as I had many friends and relatives here, and I knew several other people who were in many ways related to the colony people. Furthermore, a locale familiar to the sociologist enhances her observation and understanding of the ongoing social processes in the community under study and strengthens her power of comparison, which
“seems inevitable in the process of understanding of another society or even a different section or period of one’s own. All new social experience is referred to a pre-existing base of known and understood framework of known and understood framework of social institutions, values and ideas” (Srinivas 1966: 158). Further, “studying one’s own society” favours the researcher with additional power of understanding from the point that as an ‘insider’ s/he shares “as a member of that culture- the larger culture- some of the values, beliefs, and attitudes of people’ s/he studies, and ‘this gives him (/her) – or to give him (/her)-greater insight into their behaviour” (Srinivas 2004:413-20). “It may be worthwhile reiterating here that in sociological-anthropological understanding, as distinct from some other forms of understanding, the entire psyche of the anthropologist is involved and not only his cognitive faculties. Such involvement is more generally characteristic of insiders rather than outsiders.” (Ibid. 419)

For a detailed understanding of the problem and for collecting life narratives, unique stories, experiences, and private documents, I had to build a relationship with the setting, with the respondents, so they could open their hearts and spell out their feelings to me without hesitation. This needed a style or practice very much similar to that of ethnographic fieldwork. I spent much time in the colony with people who experienced refugee hood. In case it was not possible to reach the first-generation refugees, because of their death or other unavoidable circumstances, I tried to fill the gap through recourse to oral histories, memoirs, and other documents that could be obtained from their next and later generations. I tried to explore all the personal documents, such as diaries, letters, and photographs that could be found from the respondents. I also tried to access archival documents present in the block offices, committee offices, and elsewhere for a better and concrete understanding of the present research issue.

Research Design

Refugee issues are not a new topic of research in today's social science arena, and I had a good many references about the topic. Therefore, I decided to go for a descriptive study, using at first the method of sample survey, judgmental or purposive sampling technique, and in-depth interviewing as the method of data collection. These interviews helped me to select several cases of interest pertaining to the research question. Later, I followed up these selected cases more extensively with the aid of qualitative interviewing that produced unique life narratives of those refugee respondents. Although it was a descriptive study, it did not proceed with hypotheses testing; rather, the research questions (objectives) of this study guided the research method. As the chosen settlement of the study consisted of more than two thousand families/households, I opted for a sample survey, which, if designed correctly, not only would favour the cost and time of research but would add to the quality of study and depth of data to be collected. I classified the population primarily on the basis of generation and gender. The first generation of refugees comprised those who had direct experience of crossing the border at an age not younger than five years. The second generation of refugees was the offspring of the first-generation refugees and those who had been younger than five years old when their families had crossed the border. The third generation consisted of the offspring of the second-generation refugees. I classified each generation of refugees by their gender. As my primary interest in this study was on the direct experiences of refugee
people, particularly on the first-generation refugees getting access to whom would obviously be difficult because of their old age, loss of memory, unavailability due to death or other circumstances, refusal or shame to talk to an outsider, and other unavoidable reasons, I decided to go for a purposive sampling technique to create a sample of optimum usability with the size being two hundred in total.

I made a list of open-ended questions relating to the respondents' experiences ranging from dislocation in their homeland to the whole process of relocation and their attitude and expectation about the settlement. I contacted and met each respondent personally; explained to each my interest and objective; appealed for an in-person interview; tried to clear respondents' confusions, if any, about the interview (e.g. if there would be any risk, any political intervention, whether or not their anonymity would be secured); obtained their consent; and arranged a convenient date and time for the interview. While interviewing each refugee respondent, I asked the questions listed in the interview schedule, but I took here the flexibility of changing the serial order of the questions, to promote the flow of communication. In this sense, my tool of data collection was interview with semi-structured schedule. After the end of every interview, I continued with a small natural communication session with the interviewee. This promoted further rapport, and by this time, I obtained the interviewee's consent to sit together for another in-depth interview session to yield further information.

After I had finished with the interview of two hundred respondents, I selected about thirty five interesting cases for further interrogation and arranged for in-depth qualitative interviews with these selected respondents. I prepared one interview guide (a list of topics to be asked) rather than an interview schedule (a list of specific questions). Before meeting any respondent for the second phase of interview, I meticulously studied the report of the first interview with him or her and noted critical points such as interesting issues for further probing and discrepancies in information (if any) in order to extract from that person necessary information about his or her life during the interview session.

The next steps in the research such as recording the data, analyzing and interpreting them, and, finally, writing a report of the research were indeed important and absorbing tasks. But for now, I would like to focus on more interesting part of this research study: my experience in encountering the field.

**Encountering and Negotiating the Field**

In this research, my encounter with the field centred round two basic dynamics in my presence as a researcher: first, I was a native sociologist working in a quite familiar setting, and second, I was a woman and therefore, the existing gender structure played its role in every encounter in the field. Sociological and anthropological researches at home, occurs within a non-exotic socio-cultural context of primary socialization and requires a professionally induced schizophrenia between the ‘native self’ and ‘professional self’ (Mascarenhas-Keyes 2004: 422-35). I found typical problems of professional access to information and role definition in a context, where ‘my people’, on the one hand, did not perceive me, a researcher, as ‘special, exotic or powerful’ (Cassell, 1977:413) one; on the other, my curiosity on rather
mundane everyday life activities, was perceived as ‘odd queries’ or ‘foolish endeavour’ from a person, who by her birth and nourishment in a neighbouring locality should knows everything ‘normally’. Hence estrangement from my hitherto socialized self, and priori ascribed social position to relate to the whole spectrum of refugee experience and refugee life, was not a very simple task to perform. The colony settlers were Bengali Hindu refugees, with diverse background of caste and class. In an urban setting of today’s West Bengal (as is my research locale) caste is never a distinctly practised social phenomenon (except in some religious rituals), and mingling of caste and class is so prevalent that categorizing people with the traditional features of these two variables and considering them as different social groupings on that basis, remain very much superficial if not impossible. I was identified by the colony dwellers in terms of a complement of immutable characteristics: a Bengali Hindu from a neighbourhood locality, a native West Bengali by birth, an integral part of a bangal family by marriage, an upper-caste person without any distinct caste-identification (as perceived ‘general’ or ‘normal’ in the broader urban social spectrum of West Bengal) or caste-consciousness; a woman, young in age, married, educated, employed (in academics), and from a middle class background; each of which had its own virtues and venom in relation to my purpose as a researcher.

As a Bengali it was quite easy for me to communicate in the native language (mainstream/ urban Bengali in particular) of the colony. I found my Hindu identity to be particularly favourable to enter the households in the colony without any stigma of pollution, which many Bengali Hindus still attached to other religious communities. Further, when the first generation respondents shared their experiences of dislocation in their native land—their feeling, fear, attitude, sentiment, and trauma relating to their religious identification in the riot ridden East Pakistan, that pushed them to this side of Bengal, and their consequent feeling of hatred, anger, emotional ambivalence towards the other community (Muslims in particular),—they expressed their feelings openly and without hesitation to me. From the words they often used about the ‘other community’ I sensed that it was not my expertise as a researcher, but my identity as a Hindu by birth that favoured this kind of ease and openness in communication. Any other religious identity of the researcher, especially if he or she belonged to the Islamic or Christian community, would have made it difficult for him or her to gain access to communication centring on communally sensitive issues, which were highly valued from the sociological-anthropological perspective of my research. Whether it was ethical or not to use or exploit the personal identity (religious/ racial/ caste/ creed etc.) of a researcher against or in favour of him or her in the field might raise an issue of theoretical and ethical debate in research methodology. As a researcher I must admit that in such situations, I tried to be as

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1 Bangal is a term used to refer to the people of the Eastern part of undivided Bengal (after partition of India named as East Pakistan), now in Bangladesh (as opposed to the native West Bengalis who were called edeshi). The term is used to describe Bengalis from the east, who are marked by a distinct accent. Some of the people from East Bengal, mainly Hindus, migrated to West Bengal as the consequence of the Partition of India in 1947. These refugees were sometimes referred to as bangals by the native population of West Bengal. From now on the term will be used in this article without further note.
neutral as possible and listened attentively to what the respondents were communicating without any stimulus from my end that could intensify their communal feeling.

My birth into a native West Bengali family identified me as an edeshi\(^2\) to the colony people, who could thereby accept, without much hardship, my ignorance about the pros and cons of refugee life, experiences and hardship, or how it really felt to be a refugee. From that point of view, I could assume an ‘other’ identity for myself that might have been quite fruitful for my purpose. On the other hand, my in-laws’ family being a bangal one, provided me with an identification that I did not hold any aversion towards them, as the edeshis were perceived to hold generally for the bangal settlers of the squatters’ colonies. But my identity as a person born and brought up in a neighbourhood locality coupled with my natal and after-marriage family orientations initially created a kind of confusion among my respondents about how to judge me, or my objective/ intention. As a locally born and brought up person, I was expected to be well accustomed with the history of the establishment of the colony that existed in local discourse; an eyewitness of the change the colony had gone through since the last several decades; and well versed with the cultural milieu of the colony. Therefore, initially I had to face various critical comments, often in form of questions, full of sceptic, contemptuous feelings from my respondents- ‘what is there for you to search again in the colony?’ or ‘are we some kind of strange species- what do you hope to get strange here?’ or ‘people had to take plight if they were uprooted. This is just simple. What academics will you do with that?’ etc. Literatures relating to research methodology are concerned with the crucial difficulty confronting the researcher, who wants to study a segment of his own society, of the imperatives needed to achieve some distance from it in order to see it as a strange world (Srinivas, 2004:413-20, Leach: 1982). However, I found the problem quite reverse while doing fieldwork in the colony. Following the methodological prescriptions of sociology and social anthropology initially I tried hard to remain stranger in the field and this invited sceptic and diverse comment and emotional aversions from the colony people, as the later also tried to make sense of my behaviour (Mascarenhas-Keyes, Stella, 1987: 429). Further, it evoked the historic suspicion against social anthropology in the third world countries (Srinivas, 2004:417) that includes a strong feeling that, to be studied by the social anthropologists was enough to be labelled at least as ‘the community of difference’, where ‘difference’ connotes a kind of negative feeling as ‘not part of the mainstream culture’, if not ‘the primitives’.

There was another kind of suspicion that whether the information collected by me would bear any relation to the movement of the United Central Refugee Council against the central government, of which the Deshapiya Nagar Refugee Rehabilitation Association was an integral part, for the right of \textit{patta} to the forcibly occupied land. I was made acutely conscious of Mascarenhas-Keyes’s comment that ‘intimate

\(^2\) Edeshi is a Bengali word with the literal meaning ‘of this land or country’. The native West Bengali people, who used to reside in the western part of undivided Bengal, which remained within the Indian nation state after the partition of 1947, became popularly called as edeshi, as opposed to the Hindu Bengali migrants from the eastern part of Bengal which became East Pakistan, the east part of the Islam dominated new nation Pakistan. From now on the term will be used in this article without further note.
knowledge has an ominous and indefinable potency’ (1987: 427) to alleviate insecurity and anxiety even in an apparently stable and unchallenged settlement like Deshapriya Nagar Colony. It took a lot of pain from my end to build up the confidence of the colony people regarding my role as a researcher, a sociologist, with a purely academic interest of the understanding of their everyday life struggle. This initial confession on my part favoured my ethical position as the colony people would not really expect any benefit from me or my study. Though they (especially the men, the male leaders of the colony) kept asking me how they were going to get benefit from my research. Assuming that they would not be benefitted from my research they kept on helping me in my research; but they seemed to be patronizing in their deliberation and dealings with me. This power play between the researcher and the subjects continued to play a crucial role in the field, which I will discuss later in this chapter, but at this moment I intend to express this as my success in building up confidence among the colony people.

The existence of a huge body of official-historical-political accounts and literatures on refugees as the victims of partition and their mass acceptance, created for me the problem of ‘everything is known- what then here is left as unique as to be researched’- the question that I had often faced from my respondents during the initial stages of field work. I sensed the pervasive ‘relations of ruling’ (Smith, 1987:156) facilitated by the body of texts that successfully attributed to the everyday life of the subjects “the quality of generality and anonymity so that they may be seen as applicable in various everyday life circumstances” and in doing so seized them of the realization, expression and accentuation of their unique agencies. This also intervened the interview sessions, especially with aged first generation refugee men, who often shifted from personal experience to the generalized story of coerced migration, refugees’ plight, refugee movement etc. In such cases, I used to be a persistent listener waiting for a proper chance to redirect the interaction towards my end. Usually I did this by appealing repeatedly to the faculty of ‘bifurcated consciousness’ (ibid: 90) of my respondents, often by the aid of bringing examples from the cultural texts (anti-venom effect!) like Neeta, Sankar, and their parents, the refugee characters of Ritwik Ghatak’s popular and award winning film Meghe Dhaka Tara (“The Cloud Clapped Star”). I explained how each character had unique experiences, reactions and strategic struggles of their own, despite belonging to and sharing the very same socio-economic and cultural environment with the others. Sometimes I resorted to the stories of my own bangal relatives reflecting their uniqueness (micro versus macro), so that the respondents could develop a line between the official generalized accounts available in the stock of knowledge to describe refugees and refugee experiences, and their own personal, lived, and reflected-on experiences, which were of my research interest.

As a married person, I was recognized in the colony as a general normative person (woman) who adhered to Hindu Bengali customs and values, and this in spite of my education, employment in higher academics, high-caste orientation, and middle-class position, provided me with a disposition of a respected woman which turned out to favour me in order to get access into the colony households, and establish rapport with the women and men. But gender (both of mine, and the subjects of my research study) started playing an important role in the field with various reflexive dynamics; and often filtered into the
interactions particularly through power negotiations and, for the most, actualized in the clashes between the researcher and the participants, and also in more usual interaction (Koivunen 2010: 682-708).

During the early stage of my fieldwork, when I was trying to gain access into the colony households, most of the time I had faced initial encounters with the male members of the households, whether a first generation refugee and the head of the family, or a second or third generation male member, to whom I had to explain the purpose of my visit. These men either allowed me access to a female respondent of the family, or offered himself, or another male member as my respondent, saying that the women (especially the first generation refugee women, who were of prime interest to my study) could say nothing as they know “not so much”. Women's knowledge was perceived as an inferior one, irrelevant to the academic interest, as if rational, important, informative knowledge persisted only in the domain of men. This patriarchal construction of ‘knowledge’ was so deep rooted that most of the first generation women, whom I interviewed, started with, “what I can say? I don’t know much. You better talk to my husband....” or “if my husband was alive he could tell you much more”. Crossing the barrier of the first generation men in order to get access to their wives or other contemporary female members of the household was more difficult than the second or the third generation men. I could easily bypass the latter on account of the methodological demand of firsthand experience, which I pressed upon them. But clashes with the first generation refugee men, setting a wall between me and my wished female respondents, proved to be interesting cases of negotiating power and gender in the field. First, showing interest in a woman, while refusing a willing man for the interview, might hurt the masculinity of the later. The refused man might then perform gender by denying my access to the prospective woman respondent, which would be a least wanted situation. Further, rejecting a male respondent might double the complexity for me as the prevailing notion of femininity could perform its role in the form of denial from the desired woman respondent. She might see my rejection of the man as the breaching of norms, which would produce humiliation to her family as a whole (the male head of the family is commonly the representative of the family for the Bengali Hindus); and returning that rejection in the face of the adamant researcher would be her responsibility to reproduce the honour of the family. Initially, I resolved to interview the first generation male refugees as they offered themselves instead of their wives. It proved to be quite easy for me to access the female respondents after I built up confidence in their male guardians. Though as a women I was received by the colony people as a ‘less threatening person’ in comparison to an unknown man desiring information about their everyday life, and access to their household and family members for the purpose of interview, I found that the first generation refugee women waited for an approval or ‘safety signal’ from the men of their family before talking with me.

As a woman researcher “studying up” men or interviewing men meant focusing on those, who are more powerful in the first place (DeVault 1999: 217; Loe 2004: 209). Generally, I found the first generation refugee men of respected middle-class households as willing to share their experiences with me. They usually started with a liberal “ask what you want to know” kind of words with a distinct pride of patronage towards me that I sensed, satisfied them with a sense of control over the situation. I found them more interested in describing the property that their ancestral family possessed in East Pakistan; the status the
family enjoyed there; the power they had over the village people etc., but about the experiences of riot, refugee hood, and struggle for settlement, somehow they showed prolixity in drawing historical information, rather than sketching their personal experiences. About their struggle to establish the colony, fight against eviction bill and eviction attempts, refugee movements etc., they either elaborated their personal connections with different famous communist leaders; or resorted to history; while showing little interest in talking about their own mundane, day-to-day experiences. In such cases, I adopted the role of a patient listener allowing my respondents to speak as they wished. Thus, when they became at ease with their position, I started occasional interventions with consequential questions, but with a disposition of a curious and interested student or learner so that the man’s masculinity in relation to his power over the situation was not threatened and I also could control the interview session guided by my research objectives. However, when all my modest efforts to keep a man on my research track failed, I explained him that despite of my interest for his ‘stories’, it was the methodological obligation of my research that we should talk about some other issues. This recourse to methodology and by it to science, which is “men’s domain”, at once gave me control over the situation, turning the man, until now obsessed with his own story regardless of my queries, into an attentive interviewee offering due respect to the researcher (not a woman and therefore novice) and feeling the worth of the study.

What I found strikingly similar to all categories of men, irrespective of their class, status and power, was that they were not comfortable talking about the women of their families. Asking a good deal of questions about women—their feelings, experiences, agencies—resulted in negative reactions ranging from impassive avoidance (“What about them? They had to bear the hardship with us (men)...”) to disparaging comments (“Feminist!” showing distinct prejudice attached to this word) and even to aversive irritations (“What special do you expect about them? Are they something different?..... Perhaps, according to you, a man should stay at home sitting idly among women to know the specialty about them!”), especially when I continued insisting that the men comment on these issues. I admit that often I created such clashes deliberately to grasp the status of refugee women as seen by their male counterparts; and, of course, at the end of each such session I managed to turn the raging state into a cool favourable one, so as to assure continued access.

Compared to the men, the interview sessions with women were more cooperative and open; however, our gendered equality might have contributed to, or even created, my feeling of ease in the case of female respondents. Women initially showed their hesitations to speak as their knowledge was commonly considered kitchen-oriented and, therefore, inferior; but once they started sharing their experiences, they proved to be a mine of data that pertained to my research. A problem that I faced with the women respondents was that they were so much engulfed by their womanly household duties that often I had to sit alone and wait for them, because they were busy in their kitchens or serving other members in the family and only when they returned, could I continue with the remaining part of the interview session that too after repeating all the early efforts of probing to keep them on proper track.
I knew, as a sociologist familiar to the field my interaction with ‘my people’ in post-fieldwork situation would never be a sporadic correspondence with a few natives as was likely for an outsider; it would be a life-long engagement. Hence it was not only the academic concern, but I should consider the native context also while writing up my thesis. In producing a socio-anthropological account including the natives, I, a native researcher, had to share the concerns of the outsider about respecting confidentiality, protecting individuals, and keeping the field open for further research (Barnes 1967: 205-12). Further, I had the obligation to portray my research subjects at least in a way that they would not feel betrayed, if not in the most advantageous light. In addition to that there was the demand of science that must be finely weighed against those of humanity (Stephen and Greer 1981: 129).

**Conclusion: Memory and Nostalgia ventured through Science**

Fieldwork was not an easy experience like vini vidi vici. Often I felt confused about whether the data I was collecting would be of academic importance or would lead to stereotypical research findings. My anxiety led to despair for finding something exotic. I resorted to ethnographic accounts and literatures on methodology that helped rebuild my confidence and regain enthusiasm to find out the exotic from the apparently mundane field notes by evoking the power of sociological imagination. This resort to literature was a continuous and consistent part of my research till the end of writing report, as I had to put myself under constant interrogation whether my own subjectivity had filtered into my report that would bother the objectivity of the scientific account. However, Devereux recommends that ‘we must use the subjectivity inherent in all observations as the royal road to an authentic, rather than fictitious objectivity’ (1967: xvii). Further, the existence of ‘personal equation’ (Malinowski 1967; Beteille 1975; Okely 1975: 171-88; Cesara 1982; Rainbow 1977) and the acceptance of sociologists or anthropologists as interpreters, not mere recorders of cultures (Agar 1982: 779-95; Hammersley and Atkinson 1983; Ellen 1984) in a world of already constituted meaning (Rainbow and Sullivan 1979) are already recognized in various academic publications. The continuous shuttling back and forth from the field to literature and vice versa was a unique lesson for me which my previous small sociological survey works could not provide for.

Finally, I should conclude by saying that this research experience guided by qualitative method, demanded from me a lot of passion, scientific curiosity, and inquisitiveness; an extreme skill of sociality, including dramaturgical performance and impression management; scientific rigor, scepticism and tenacity coupled with sociological imaginations; in which it gradually trained me, nurtured me, exploited me, and moved me with the mystery and joy of gaining new insights into an apparently familiar world; and that also along the path of its progress and according to its stipulations. Through engagement with this research study, I had a chance to float into the nostalgic ocean of my childhood days along with my subjects, who perhaps dived deep into the fathom of memory, occasionally floating into the present, and opening their heart about hopes, despair and demands about the future; while I held tightly to science, the principles of methodology, and with continuous vigilance over myself as a researcher, a sociologist.
NOTES AND REFERENCES


Smith, Dorothy E. (1987). The Everyday World as Problematic. Boston: Northern University Press. In this book Smith, a feminist sociologist, introduced her major theoretical statements and concepts of relations of ruling; generalized, anonymous, impersonal texts; and local actualities of lived experiences.

