Ethnographic Experiences with Female Plantation and Apparel Workers of Sri Lanka: A Methodological Reflection

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Abstract

Over the years, feminist ethnographers have engaged in a debate critiquing the practice of ethnography in the light of feminist research principles (Enslin, 1994; Patai, 1991; Stacey, 1988; Visweswaran, 1997). However, such literature has left space for further debate on whether ethnographic practices indeed are paradoxical to feminist values in research. Furthermore, while a few writers claim familiarity with conditions outside of the "west" (e.g., Enslin, 1994; Visweswaran, 1997), the majority of these debates and discussions fall outside the boundaries of the third world. As such there exists a gap between "ethnography as a way of feminist research" as prescribed by western authors and as experienced by third world feminist researchers. This paper where I reflect upon my ethnographic experiences with female plantation and apparel workers of Sri Lanka is an attempt at bringing this gap. Here I ask the question "what is the extent to which existing methodological doctrines of feminist ethnography embody the ethical political consideration as applies to third world locations"? Embedded throughout my reflective account are instances where principles of feminist ethnography had failed to fully reflect ethical political considerations specific to the third world, highlighting a need for "new knowledge on feminist methodology" that gives space for the voice of third world feminist researchers to be heard.

Keywords

Feminist Methodology, Feminist Ethnography, Third World/Postcolonial Scholarship, Research Ethics, Reflectivity

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Over the years, feminist ethnographers have engaged in a debate critiquing the practice of ethnography in the light of feminist research principles (Enslin, 1994; Patai, 1991; Stacey, 1988; Visweswaran, 1997). However, such literature has left space for further debate on whether ethnographic practices indeed are paradoxical to feminist values in research. Furthermore, while a few writers claim familiarity with conditions outside of the "west" (e.g., Enslin, 1994; Visweswaran, 1997), the majority of these debates and discussions fall outside the boundaries of the third world. As such there exists a gap between "ethnography as a way of feminist research" as prescribed by western authors and as experienced by third world feminist researchers. This paper where I reflect upon my ethnographic experiences with female plantation and apparel workers of Sri Lanka is an attempt at bringing this gap. Here I ask the question "what is the extent to which existing methodological doctrines of feminist ethnography embody the ethical political consideration as applies to third world locations"? Embedded throughout my reflective account are instances where principles of feminist ethnography had failed to fully reflect ethical political considerations specific to the third world, highlighting a need for "new knowledge on feminist methodology" that gives space for the voice of third world feminist researchers to be heard. Keywords: Feminist Methodology, Feminist Ethnography, Third World/Postcolonial Scholarship, Research Ethics, Reflectivity

Many writers (e.g., Atkinson, 2000; Clifford & Marcus, 1986; Coffey, 1999; Denzin, 1997; Hammersly, 1992; Mason, 2002; Skeggs, 2001; Tedlocke, 2000; Visweswaren, 1997; Van Maanen, 1995) have contributed extensively towards the debate centred on ethnography as a way of research. Embedded within this broader debate, are concerns raised by feminist ethnographers where they question the practice of ethnography in the light of feminist research principles (Enslin, 1994; Patai, 1991; Stacey, 1988; Visweswaran, 1997). Such critiques however, have left space for further deliberations on whether ethnographic practices indeed are paradoxical to feminist values in research. Furthermore, while a few writers claim familiarity with conditions outside of the "west" (e.g., Enslin, 1994; Visweswaran, 1997), the majority of these debates and discussions fall outside the boundaries of the third world. Accordingly, they fail to fully reflect the context specific issues and concerns as experienced by researchers engaged in feminist ethnography in third world/postcolonial locations. In the following narration where I "reflect" upon my experiences of ethnographic fieldwork with "tea pluckers" and "sewing girls" of Sri Lanka, I attempt to identify the extent to which existing methodological doctrines of ethnography embody the ethical political conditions as applies to third world/postcolonial locations such as mine. Instances where exclusions of such considerations required me to rethink and rework established methodological principles are also "reflected" through my narration.

On the Question of "Feminist Ethnography"?

Over the years' scholars with a feminist orientation (e.g., Cook & Fonow, 1990; Mies, 1983; Smith, 1988; Stanley & Wise, 1983a) have endeavoured to uncover a set of principles they believe as significant in identifying a research or a research method as "feminist." In Cook and Fonow's (1990, p. 72) view, first of these is the necessity of continuously and reflectively attending to the significance of gender and gender asymmetry as a basic feature of all social life, including the conduct of research. Under this principle, a feminist methodology should look at women through a "female prism" in research devoted to description, analysis, explanation and interpretation of the female world. This principle is also touched upon by Smith (1988) where she argues for locating the researcher as a gendered being as a way of attending to gender in the practice of research. In Smith's (1988) view, by acknowledging the common experiences between herself and the women subjects of her research the feminist researcher is able to bring women's realities into sharper focus. Focus on consciousness - raising is seen by Cook and Fonow (1990) as the second principle for feminist research. Stanley and Wise (1983a) explain the concept as "feminist scholars inhabit the world with a "double vision of reality" which is part of their feminist consciousness. Through this double vision women's understandings of our lives are transformed so that we see, understand and feel them in a new and a different way at the same time that we see them in the old way enabling us to understand the seemingly endless contradictions present within life" (p. 54). Challenging the norm of objectivity that assumes the subject and object of research can be separated and that personal and/or grounded experiences are unscientific; concern for ethical implications and recognition of exploitation of women as objects of knowledge; and emphasis on the empowerment of women and transformation of patriarchal and social institutions through research, are identified by Cook and Fonow (1990) as the third, fourth and fifth principles respectively, the application of which identifies a research practice as "feminist" (pp. 72-73).

Finally, as stated by Skeggs (2001), "feminists have tactically crafted an ethical and political stance out of feminism more generally and applied these to the research process, it is the way in which feminist political/ethical proscriptions are applied, that makes the research identifiably feminist" (p. 429). Indeed, many feminist scholars (e.g., Bell, 1993a, 1993b; Duelli Klein, 1983; Reinharz, 1983; Stanley & Wise, 1983a, 1983b) advocate the view that ethnography is particularly appropriate to feminist research and argue that like feminism, ethnography emphasizes the experiential. They see its approach to knowledge as contextual and interpersonal and attentive to the concrete realm of everyday reality and human agency.

However, there is also concern among feminist scholars themselves about the probable limits of ethnography as a feminist method. For instance, Judith Stacey (1988) raises the possibility of a contradiction between feminist principles and ethnographic method when she states, "I find myself wondering whether the appearance for greater respect for and equality with research subjects in the ethnographic approach marks a deeper, more dangerous form of exploitation" (p. 22). She discusses two major areas of contradiction, the first involving the ethnographic research process and the second involving its product. Stacey (1988) argues that:

> …because ethnographic research depends upon human relationships, engagement, and attachment, it places research subjects at grave risk of manipulation and betrayal by the ethnographer… The inequality and potential treacherousness of the relationship is inescapable. So too does the exploitative aspect of ethnographic process seem unavoidable. The lives, loves, and tragedies that fieldwork informants share with a researcher are ultimately data, grist for the ethnographic mill. (p. 23)

The second major area of contradiction between feminist principles and ethnographic method is the dissonance between fieldwork practice and ethnographic product. According to Stacey (1988), "It is the researcher who narrates, who authors the ethnography. Here too, therefore, elements of inequality, exploitation, and even betrayal are endemic to ethnography" (pp. 23-24). Stacey (1988), answering the question as first raised by her, "Can there be a feminist ethnography?" concludes, "...while there cannot be a fully feminist ethnography, there can be ethnographies that are partially feminist accounts of culture enhanced by the application of feminist perspectives" (p. 26). She further advocates that feminist researchers should be vigorously self - aware and humble about the partiality of their ethnographic vision and its capacity to represent self and other.

Stacey's thesis is countered by feminist writers (e.g., Enslin, 1994; Patai, 1991; Wheatley, 1994) who seem to feel that the moral dilemmas evoked by Stacey are not necessarily feminist, but more epistemological and ethical. It is from such a perspective that Patai (1991) replies to Stacey's criticism of feminist ethnography saying, "in an unethical world, we cannot do truly ethical research" (p. 150). Similarly, Enslin (1994) argues "in a non-feminist world, we cannot do truly feminist research" (p. 545). In Enslin's (1994) view,

> in a world shaped by gross inequalities of gender, race, caste, class and geography, research done on the lesser privileged, by and for the ultimate benefit of the privileged, is simply not ethical. We gloss over this inequality by claiming to do research with our subjects. We pay lip service to collaboration and dialogue, and mask the very real differences among us and the ways that our research continues to buttress them. (p. 545)

Diana Wolf (1996) draws attention to a yet another aspect of ethnographic inquiry, namely feminist dilemmas in fieldwork. In her view "the most central dilemma for contemporary feminists in fieldwork, from which other contradictions are derived, is power and the unequal hierarchies or levels of control that are often maintained, perpetuated, created and re-created during and after field research" (p. 2). Power is discernible in three interrelated dimensions as:

> ... (1) power differences stemming from different positionalities of the researcher and the researched (race, class, nationality, life chances, urban-rural backgrounds); (2) power exerted during the research process, such as defining the research relationship, unequal exchange, and exploitation; and (3) power exerted during the post fieldwork period-writing and presenting. (Wolf, 1996: 2)

Wolf (1996) further argues that

> the first dimension of power difference cannot be altered if one is studying marginalized or poor people. While "first world women" may experience multiple levels of difference when working in "third world countries," postcolonial feminists working in their own countries experience their class and educational privilege, at the very least. (p. 3)

The second and third dimensions of power differences result from feminist researchers tending to maintain control over the research agenda, the research process, and their results. "By maintaining this control and distance, most feminist scholars end up benefiting the researcher more than those studied and furthering the gap between them." Such behaviour, argues Wolf

(1996), undercuts the goals of feminist research and reproducers aspects of mainstream research. (p. 3)

Kate McCoy (1988), in a paper on ethnographic drug research asks, "Am I just doing spy work?" Raising a point that is broadly applicable to all of the social sciences, McCoy argues that in spite of good intentions, "all research is to some degree surveillance" (cited in Lather, 2001, p. 482). This argument interrupts the romance of empowerment that drives much current ethnography, obscuring the surveilling effects of the best researcher intentions. As feminist ethnographers (e.g., Enslin, 1994; Patai, 1991; Stacey, 1988; Visweswaran, 1997) have pointed out, given the danger of research to the researched, ethnographic traditions of romantic aspirations about giving voice to the voiceless are much troubled in the face of manipulation, violence and betrayal inherent in ethnographic representation. Visweswaran (1997) raises suspicions of "the dangerous ground between intimacy and betrayal" that characterizes feminist work intended to "testify" and "give voice" (p. 614). In her ethnography of Indian women in the freedom movement against England, Visweswaran relates stories of the gaps and fissures, the blind spots of her romance of empowerment.

In summary, then, while there seems to be some consent among feminist scholars about the appropriateness of ethnography as a feminist research practice, there also seems to be concern about possible contradictions between ethnography and feminist research principles. Possibility of betrayal and exploitation of the informants by the ethnographer, producing ethnographies that pay lip service to emancipation of participants while continuing to buttress their oppression, power differences that are set up and maintained throughout the research process and finally representational violence are thought of as contravening feminist values in employing ethnography. In the final section of this paper I will revisit and closely examine each of these concerns, in the light of my own ethnographic experiences - as a third world feminist researcher working with female workers within a third world/postcolonial setting.

The debate around the "question of feminist ethnography" as reviewed above leaves space for further deliberations on the extent to which such arguments are reflective of the conditions specific to third world/postcolonial locations. In fact, a vast majority of feminist scholars engaged in this debate are from the Western world and their writings are invariably specific to the contexts from which they are writing. As such, there exists a gap in knowing the extent to which such argument as raised by feminist scholars based in Western countries are reflective of the conditions as experienced by feminist researchers engaging in ethnographic filed work in the third world - under significantly different ethical political conditions. It is this gap in feminist methodological literature that I attempt to bridge in my paper by reflecting on my own filed work experiences in a third world/postcolonial location.

Further explaining the context in which this study was conducted - it draws on my fieldwork experiences in Sri Lanka, my home country where I work as a University Lecturer. The purpose of such fieldwork was to generate data for my PhD thesis, where I attempted to explore the multiple interactions of women's productive and reproductive labour as happens with the work regimes of a postcolonial plantation estate and an apparel factory. The issue I address in this paper is related to but is not the main research problem of my thesis. It was inspired by the many contradictions and dilemmas I faced in the field which led me to ask the question: what is the extent to which existing principles of feminist ethnography reflect the context specific ethical political issues experienced by feminist researchers working in third world/postcolonial settings such as mine? This is the question I attempt to address in this paper, which unfolds as an ethnographic narrative where I reflect on my experiences in the field.

This methodological reflection which forms the results section of my paper is preceded by the following section on methods where I justify using ethnography as the main methodological approach for my study, explain how the two ethnographic settings were

selected, strategies used for generating data and finally the way in which this data set was analysed.

Methods

Why Ethnography? *Justifying the selection*

Mason (2002, p. 85) describes several significant reasons as to why a researcher might want to use ethnography as a method of data generation. Firstly, the researcher might have an ontological perspective, which sees interactions, actions and behaviours and the way people interpret these and act on them as central. The researcher may be interested in a range of dimensions of the social world (not just written responses to a questionnaire, or verbal responses to an interview or written texts) including daily routines, conversations, language and rhetoric used, styles of behaviour (including non - verbal behaviour) and the active construction of documents and texts in certain settings. Therefore, observation, if not participant observation, facilitates the researcher to overcome the constraints and limits of questionnaires, interviews and so on while experiencing the real nature of "Being" in her/his research setting. The use of ethnographic observations and interviewing for data generation within the contexts of the "estate" and the "factory" - the research settings of my study - is based primarily on this ontological perspective. Within both these settings I saw behaviours, interactions and the way participants interpret and act on them as central. My interests lay not in obtaining answers to questionnaires, or even responses to structured interviews but rather to observing the daily routines, behaviour patterns, interactions and conversations of the participants' working and living within these settings.

Secondly, a researcher might decide to use the ethnographic method, if she or he has a epistemological position which suggests that knowledge or evidence of the social world can be generated by observing or participating in or experiencing "real life" settings, interactive situations and so on because not all knowledge is articulable, recountable or constructible in an interview. This position is based on the premise that these kinds of settings, situations and interactions reveal data in multidimensional ways. The researcher can be a "knower" in these circumstances because of shared experiences, participation or by developing empathy with the researched. They know what the experiences of the social setting feel like, and in that sense, they are epistemologically privileged (Mason, 2002, p. 85). Thus, the ethnographic method emphasises the fact that reality is socially constructed and indeed it facilitates the researcher to explore and understand multiple realties in her/his research setting.

The use of an ethnographic approach to generate data for my study draws on Mason's argument as stated above. Accordingly, use of ethnographic observations and interviewing is firmly grounded in the epistemological position that knowledge can be generated by experiencing "real-life" settings and interactive situations. It is also believed that such experiencing allows the generation of multidimensional data on social interactions in specific contexts as it occurs rather than relying only on people's retrospective accounts, and on their ability to verbalize and reconstruct a version of interactions or settings (Mason, 2002).

For instance, data obtained through observing and interacting with the participants of the research settings are thought to be richer, rounded and more specific than what could be "known" by merely asking them to recount their experiences. Further, such encounters give an opportunity for the researcher to share experiences and develop empathy with the participants and also to explore and understand multiple realities in the settings. However, such intimacy, engagement and attachment with the participants while enabling an "epistemological privilege" on one hand also opens up the possibility of betrayal and exploitation of the participants' on the other (e.g., Enslin, 1994; Stacey, 1988), thereby situating the ethnographer in a state of

dilemma. This is an inherent limitation of ethnographic fieldwork that needs to be acknowledged and dealt with empathy and insight by the researcher.

Mason (2002) highlights another reason for selecting ethnographic observation when she states

> choosing to use observational methods usually coincides with the view that social explanations and arguments require depth, complexity, roundness and multidimensionality in data rather than surface analysis of broad patterns, or direct comparison of interviewee responses to a standardized set of questions. (p. 86)

In using this method, the researcher may claim that the data were situationally occurring, rather than clearly artificially manufactured. Accordingly, it is argued that experiencing the daily routines and interacting with the participants who live and work within these settings enables generation of rich, complex, in-depth and multifaceted data that are more appropriate for answering the research questions, than data that might be "artificially" made up by comparing the responses to a standardized questionnaire distributed among participants.

Finally, in selecting ethnographic method researchers are likely to conceptualize themselves as active and reflective in the research process. Thus, users of ethnography are said to write themselves into their field notes and into their analysis. In this sense researchers should be careful not to under - estimate the challenges of analyzing their own role in the research process, neither should they over - estimate their capacity to empathize with or "know" the other simply because they have been in a shared setting.

This is a challenge or yet another dilemma faced by researchers in employing ethnography from a feminist perspective. On the one hand, we should strive to understand the implications of our own involvement in the research process. On the other hand, we should also be mindful of the limitations of being able to "know" the "other" simply because of sharing the same setting for a short span of time. One way of dealing with the first issue is, as far as possible, to acknowledge the implications of our own presence within the "process" in writing up the ethnography. The second, which relates to "ethnographic representation," should be dealt with in the light of ethical political considerations specific to feminist research. Simply, the fact that ethnography even at its most robust allows only a "partial representation" should not deter feminist researchers from striving to understand the lived realities of women's lives through this methodology. The solution is not to abandon ethnography as a method of feminist research but to commit to refining and fine-tuning it to suit the specific circumstances of the settings under study, enabling the generation of the closest possible insights about women's lives within these settings. Again, this is an issue that warrants further explanation in the light of actual ethnographic experiences; a task I will undertake in the forthcoming section on methodological reflections.

Ethnography: *Settings and strategies*

Writers on ethnography express diverse views on the issue of "what is ethnographic fieldwork?" Reinharz (1992) views "contemporary ethnographic fieldwork as multi-faceted including participation, observation, interviewing and archival analysis." Coffey (1991) presenting a more reflective account says, ethnographic fieldwork "is personal, emotional and identity work." She further argues that "all fieldwork can be conceptualized in terms of the body…We cannot divorce our scholarly endeavours from the bodily reality of being in the field" (p. 1). Together with the social, emotional and bodily dynamics as mentioned above, the intellectual issues involved in generating data through ethnographic fieldwork present a major

challenge to the ethnographer. In the face of such challenge, a framework developed by Mason (2002) where she identifies several clusters of questions, each woven around an important aspect of fieldwork, becomes an effective methodological tool to be used in designing and engaging in ethnographic fieldwork. The clusters of questions woven around finding the research setting(s), generating knowledge and significance of the setting(s), directing the ethnographic gaze, getting in and getting by, identity, relationships, informed consent and turning observations into data/writing field notes are drawn upon throughout this section (Mason, 2002). These themes are used firstly to explain how fieldwork was actually carried out as stated below and secondly to reflect upon ethnographic experiences.

Finding the research setting(s). In finding the research setting(s) within which to carry out fieldwork the ethnographer should consider where the phenomenon in which s/he is interested is located in time, space and place. The issue of how immersion in a particular setting shape what the ethnographer "sees" or "does not see" should also be duly considered. As stated by Atkinson et al. (2001, p. 5) "Ethnographic fieldwork and the disciplinary commitments that inform it, construct the objects of research as well as providing ways of exploring them." Simply, how, where and in what ways we look will shape what we see. Thus, the choice of setting(s) is not simply a practical matter, but a highly intellectual one, which express core elements of the researcher's ontology and epistemology (Mason, 2002).

Ceylonita estate, the first site within which fieldwork of this research was carried out was a state-owned plantation located in the district of *Nuwara Eliya*. Identified as the heart of the plantation industry, *Nuwara Eliya* is home to the highest numbers of estate workers in the country. Thus, an estate located in its midst was thought to be reflective of the features specific to the plantation industry as well as to be adequately representative of the characteristics unique to its workforce. Moreover, plantations in this part of the Island identified as the "up country" have resident workforces consisting of only workers of Indian origin[1]. Having first been brought to the Island by its colonial rulers their origins are closely linked with the country's colonial heritage. As this study sought to explore the enduring effects of colonial power relations on the plantation labour force, the location of *Ceylonita* was ideally suited for its purpose. Further, *Ceylonita* was a large-scale plantation consisting of some 200 hectares of land and 505 families, most of whom make a living as waged labourers in the estate. Out of a total resident population of 2062, 1044 were female, who became the focus of this study. Finally, I was able to obtain permission to enter the estate, to live within its boundaries and to interact with members of its work force without any limits or restrictions. All these factors contributed towards making *Ceylonita* an appropriate setting within which to carry out ethnographic fieldwork.

SriKnit Garments, an apparel factory situated within the *Katunayake* Free Trade Zone was selected as the second site for this study. *SriKnit* was one of three factories owned by a large multinational corporation which had been in operation in the "zone" almost since its inception in 1979. Accordingly, it had a long-standing reputation as one of the oldest and most well established industrial units in the zone. I was able to successfully negotiate access to *SriKnit* which had a work force of 1048, 90 percent of whom were female workers. Similar to that of *Ceylonita* estate, here again it was these female workers who became the focus of the research. In the face of extremely restrictive access policies adhered to by all the companies operating within the zone, the ability to negotiate access was one of the main considerations in

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[1] Due to historical reasons, agricultural workers of traditional Sinhalese villages surrounding the "up-country" tea estates had been reluctant to work as labourers in colonial tea plantations. Thus, estate work forces of these estates were made up of immigrant workers. This situation had not changed and even today the workforces of these estates are predominantly *Tamil*. This is different from what is practiced in "low country" estates (estates in the lower elevation areas) which employ both Sinhalese and Tamil workers. However, these estates are smaller in size and quality of tea produced is different from that of "up-country" tea.

selecting *SriKnit* as the second ethnographic site. However, its long history as a reputed manufacturing entity, large scale of operations and the work force that could be considered as representative of apparel factory workers in general were also central to the selection decision.

Since a considerable part of observing and interviewing of female workers was done in the vicinity of their living environments (i.e., in boarding houses located outside of the factory premises), the actual ethnographic site in this case extended beyond the boundaries of the factory, drawing additional areas into its ethnographic fold. Sometimes it extended even as far as the traditional villages which were home to the female workers. Such inclusions link up with the following discussion on "generating knowledge and significance of the setting(s)" where it is argued that not all data related to the research questions could be generated within the setting(s).

Generating knowledge and significance of the settings. Here, the issues likely to be faced by an ethnographer include: "What does my research setting represent?" "What type of data can it yield?" "What else do I need to know?" Many researchers who use ethnographic methods do not view settings as naturally occurring data. Rather it may be argued that once an observer enters a setting, s/he becomes a part of that setting transforming the dynamics within them. Therefore, some of the significant developments in the thinking about reflectivity in research and the constructive nature of knowledge have resulted from the reflections of ethnographers (Mason, 2002). This understanding becomes a crucial one since it influences the ways in which the ethnographer observes as well as record such observations, how they are woven into an analysis and an argument and how the ethnographer is implicated within the process. In the case of this study my chosen role as ethnographer was to consciously look for and reflect upon data that would, upon analysis and argument help build up an understanding of the research questions.

In addition to going, being in and leaving the setting I was also engaged in some critical self-questioning about exactly how the setting produces data and if all the required data comes from the setting. The significance of such self- questioning is illustrated by Mason (2002, p. 88) when she says, "the way in which a researcher conceptualizes what a setting is and in particular what its data generating capabilities might be, has important ramifications for the nature of the knowledge they can argue to have produced." Such conceptualization or self - questioning in relation to this study was achieved by considering how far the setting, as a physical and social place or a space, encapsulated everything that I was interested in finding out. Thus, it was considered whether there were interactions, which occur "outside" the setting, which may nevertheless influence what takes place inside it? For instance, in the case of a *SriKnit* garments the female workers who were the focus of observation within this setting were continually subjected to a conflicting set of interactions that occurred both inside and outside the setting i.e., their temporary boarding houses in the "zone" as well as their homes in the remote villages. As such due consideration was given to the fact that the settings did not exist in a vacuum, but were part of a complex and dynamic socio-political process.

Such considerations made it apparent that all the data required to address the research questions were not available from within the chosen "settings" and the settings couldn't be understood solely from the inside. This realization influenced me to stretch the limits of the selected sites by directing the "my ethnographic gaze" beyond their physical boundaries.

Directing the ethnographic gaze. Once the setting(s) were selected and their significance in generating knowledge carefully identified, another cluster of questions that needed to be addressed was in relation to directing the ethnographic gaze. In directing her/his gaze the ethnographer should consider the issues of: how data should be generated. "Where do the data come from?" "What do they look like?" What to look for and observe in the settings. Once within their selected settings ethnographers are generally interested in talk, behaviour, interactions, layouts and special elements, appearances, physicality/embodiment, procedures

and so on (Mason, 2002). However, if s/he is to overcome being confused and unfocused the ethnographer needs to work out the issues of "selectivity" and "perspective." This necessitates the ethnographer thinking through the process of linking up the research questions with what to look for and what to ask about in the field. Even though generating ethnographic data is a fluid and an ongoing process a tentative plan of what to look for next, who to speak to next, and so on helps to avoid a lack of focus.

As previously explained *Ceylonita* estate at *Nuwara Eliya* was the first site within which field work was carried out. Once the initial decision of which estate to study was taken, it was necessary to identify units for analysis within the estate itself. Firstly, an overall understanding about the estates working patterns as an agricultural/industrial entity was sought. As households were seen as bringing together both the productive and reproductive aspects of women's labour, households or families in residence within the estate were considered as cases to be studied in relation to the research questions. A strategy of purposeful sampling was adopted in selecting the cases. Information rich cases were selected by talking with the estate midwife who had close knowledge of each of the families in residence. In selecting the sample of households to be studied, the life cycle of the female workers was also taken into consideration. Women and girls at different stages of their life cycles that is, young girls, married women with young as well as grown up children, women who had no children of their own, mature women who were no longer of working age were all drawn into the sample. A major part of the field work consisted of observing female workers during their work day, and close and continuous intermingling and interactions with them during times after work. Even though centred around the households or cases included in the sample, fieldwork in the estate drew in a much wider and richer grouping of informants resulting from spontaneous opportunities that sprang up in the field. It also included engaging in informal discussions with other members (e.g., estate workers other than female tea pluckers) of the estate community.

In observing female tea pluckers at work I was required to take up the role of passive observer due to two reasons. Firstly, plucking tea leaves was a skilled task which I was unable to perform and couldn't hope to master within a short period of time. Secondly, any attempt to interact with the workers while at work was not viewed favourably by the *Kanganys* (male supervisors of female tea pluckers) nor by the workers themselves since they worked to achieve a set target each day in terms of number of kilograms of tea leaves plucked.

However, there were many opportunities to interact and talk with the women workers while they walked down to their allotted slots in the field from their "line rooms" in the mornings and returned in the afternoons, on their way to and from the crèches with their children, on their way to the factory to weigh and record their daily harvest and so on. There was also time to talk with and listen to them during times spend in their homes after work. As such I intermingled with them while they engaged in their daily domestic work of cleaning the house and garden, cooking, tending to their children, fetching firewood and water. The focus of all these activities was to get an insight into what the female workers thought and felt about their life and work in the estate, to hear their voice about the dual roles imposed upon their bodies as waged productive labour in the field and as unpaid reproductive labour in the household.

The way in which "ethnographic gaze was directed within the estate setting as described above, as well as later explanations of the same within the factory setting was inspired and guided by writings of feminist ethnographers" (e.g., Abu-Lughod, 1990; Bell, 1993; Mankekar, 1993; Mies, 1982; Ong, 1988; Parameswaren, 2001; Salzinger, 2004; Schrijvers, 1993) where they describe the methods used and constrained faced with similar situations. Significant among these being the work of Maria Mies (1982), *The Lace makers of Nasapur: Indian Housewives in the world Market*, where Mies (1982) explores "the roots of the relationship between capitalist development and patriarchal structures, and the effects of market - economic

(capitalist) developments on poor rural women in the Third World" (p. 5). Carried out in two areas of the *Indian* state of *Andhra Pradesh* - among home workers around the small city of *Nasapur* and among rural workers in the *Nalgoda* district, the context of this study bears close resemblance to the social economic context of the present study. Mies (1991), in one of her later writings on feminist methodology, describes the field work which helped generate data for *The Lace makers of Nasapur* as:

> …we decided to share as far as possible, the living conditions of the rural women workers. We lived in these settlements, … we fetched water from the well as they did, cooked our food in the same way, slept on the clay ground like them….We also accompanied the women as they worked in the fields and took part in some of the work ourselves. In this way we established a relationship with them without which no research would have been possible... Alongside this direct participation in their lives we also carried out many discussions… (p. 71)

Mies (1991) emphasises the appropriateness of such methods to study her own and similar research problems when she says,

> through this participation in their lives, we learned more about the division of labour according to gender, more about working hours, wages, exploitation, patriarchal structures and the women's forms of resistance than we would have had we followed the usual research methods … it would never have been possible for us to gain these insights by using conventional research methods. (pp. 71-72)

Joke Schrijvers's (1993) paper on "conceptualizing motherhood among Sri Lankan women' added further methodological insights to directing the ethnographic gaze." Schrijvers's detailed descriptions of fieldwork such as

> …during our first weeks in the village, when I tried to get acquainted with the women, I had the greatest difficulty of meeting them at home as most of them worked in their *chenas* (fields) miles away from their homes. I chatted with the women in their kitchens in the early mornings when they prepared food to take to the field and I went with them on long walks to the fields from the village in the surrounding wood. (p. 146)

-- were especially helpful in dealing with similar situations as encountered in the field.

SriKnit garment factory at *Katunayake* was the second ethnographic site selected for this research. Within this setting, the units of analysis were considered to be the boarding houses/rooms where the female workers resided. Here again the sampling strategy was one of purposeful sampling and was done with the help of a NGO activist who acted as a key informant. The sample or the cases to be studied were selected so as to include a cross section of workers who were at different stages of their work lives, that is, workers who had just come to the "zone, who had worked as sewing machine operators for a few years, who were about to leave their jobs in the "zone," and those who were no longer working in the "zone" but still living in the boarding houses." In addition, one worker who no longer worked in the "zone" but who came to visit her friends was also included. Further, diverse "cases" were also drawn into the sample by including workers who had young children and who were pregnant.

Similar to that of the estate, filed activities took the form of observing female workers during their work day; close and continuous intermingling and interactions with workers during their non - working times, interviewing other members who make up the social fabric of these sites and gathering supplementary data from the sites.

Since permission to enter the shop floor was obtained under restrictive conditions time spent within this space was solely as a passive observer of the female sewing machine operators at work. Such passive observation of the shop floor focused on assessing the work environment both physical and emotional, observing the facial expressions and body movements of workers while they operated the sewing machines throughout the day, the level and intensity of supervision, verbal and nonverbal behaviour of supervisors, including their tone of voice, choice of words and manner of addressing the workers and, workers' interactions with each other. Observation within the premises also extended to the workers' lunch and tea breaks, when they left the production floor and went to the canteens for short durations. Out of the shop floor, in canteens and wash rooms, I searched for evidence of the expression of feelings of workers (i.e., lyrics written on walls, newsletters or posters written by them), the way they interacted with other workers who were not from the shop floor and their mannerisms and behaviour patterns (i.e., did they appear tired and stressed or relaxed and energetic).

However, due to the impracticality of engaging in lengthy and drawn out conversations with workers while they were within the factory premises, such interactions were mainly carried out in the vicinity of the boarding houses. It was possible to meet and talk with the workers outside of their working hours in the factory, that is, on their way to and from work, at the boarding houses after work while they engaged in cooking, cleaning and washing clothes and even while they were chatting with each other about the day's events. Weekends, especially Sundays when most of the workers did not go to the factories but spent their time in preparing daily meals, cleaning the rooms and washing clothes, shopping for essential items and so on provided a good opportunity to closely interact with them. Such times were made use of to observe their activities, listen to their interactions and conversations with each other and sometimes to ask questions. The focus of all activities within this setting was to gain an insight into how these workers viewed their productive role as waged labour in the factory and their reproductive role as mothers and housewives at home.

Turning observations into data/writing field notes. This involves the issue of how to select from the material, information and impressions generated in the field. The ethnographer has to engage with the question of how diverse and experiential material can become the kind of data which can be used to construct convincing and meaningful arguments. In deciding what counts as data, the ethnographer has to answer the questions of what observations to record, how to record, and when and how often to record (Mason, 2002). Such decisions should be based on grounded critical judgments of what each can offer in relation to the research questions and their context.

Since writing field notes is perhaps the most significant activity in turning observations into data it is worth more elaboration at this point. According to Emerson et al. (2001)

> field notes are a form of representation, that is, a way of reducing just - observed events, persons and places to written accounts. And in reducing the welter and confusion of the social world to written words, field notes (re)constitute that world in preserved forms that can be reviewed, studied and thought about time and time again. (p. 253)

In the case of my study bulk of the field notes were written up at later points in time following observations. Here I decided to incorporate my own perceptions, interpretations and experiences into the field notes rather than keeping them separate from my observation of

others. Detailed descriptions of what happened in the field, discussion of my own feelings and impressions and finally my own analytical ideas were integrated into the field notes. The reason for such inclusion was the belief that I was very much implicated in the task of generating data. Also, such an effort was thought to be facilitative of a reflective and interpretive reading of data at a later stage.

The decision to integrate my own perceptions, experiences, feelings and impressions into the field notes was inspired by writings of feminist ethnographers. For instance, the practice of "ethnographers writing themselves into their field notes" is noticeably evident in Joke Schrijvers's (1993) paper, "Motherhood Experienced and Conceptualized: Changing Images in Sri Lanka and the Netherlands," where she draws on her diaries, letters and interpretations to reflect on the dialects of her changing conceptualization of motherhood in *Sri Lanka* and the *Netherlands* over more than a decade. Schrijvers (1993) identifies the epistemological base of this practice as

> …a dialectical approach considers the creation of knowledge as the non -
> replicable outcome of a socially and historically specific research process.
> Knowledge is a construct; it is created in the interaction between researchers
> and those whose ways of living they try to understand… (p. 143)

While field notes made up the major part of the data set of this study, interview transcripts, other documentary evidence (i.e., archival material, institutional records, workers' newspapers, letters and poems and photographs taken in the field) also contributed towards further enriching the ethnographic data. As such it is to a discussion of how this data set was analysed that I now turn.

Ethnography: *A "Narrative" Approach to Analysis*

Over the recent past social scientists (e.g., Bamberg, 2007; Cortazzi, 2001; Czarniawska, 2004; Gubrium & Holstein, 1999; Hammersley & Atkinson, 2007; Polkinghorne, 1995; Riessman, 2008; Tedlock, 2000) have increasingly focused on the usefulness of "narratives" for reporting and evaluating human experiences. Accordingly, the power of narratives as a tool in analysing ethnographic data has come to be strongly established in the literature. As suggested by Richardson (1990) a narrative mode is equally important to the organization of everyday life - in the form of mundane stories and accounts of personal experiences - as well as to the organization of ethnographic accounts themselves. Adams (1990) points out that the narrative mode is especially important to the character of ethnographic inquiry since it furnishes meaning and reason to reported events through contextual and processual representations. In narrating events ethnographers can show how people act and react in particular social circumstances (Hammersley & Atkinson, 2007). This approach allows ethnographers to "display the patterning of actions and interactions, its predictable routines and unpredictable... crisis... show the reader both the mundane and the exotic" (Hammersley & Atkinson, 2007, p. 199). Finally, the overall significance of the ethnography can be conveyed through its narrative structure for as Atkinson (1992) argues, "beyond the fragmentary narratives persons and circumstances are the meta - narratives that shape the ethnography overall" (p. 13). Ethnographers can carry out their task of transforming material from "the field" into "the text" by constructing narratives of everyday life. For this, ethnographers need to critically develop the craft of storytelling. By arming themselves with this powerful intellectual and aesthetic tool, ethnographers can effectively engage with the task of storing other people's stories (Hammersley & Atkinson, 2007).

If we wish to understand the deepest and most universal of human experiences, if we wish our work to be faithful to the lived experiences of people ... or if we wish our privileges and skills to empower the people we study, then we should value the narrative. (Richardson, 1990, pp. 133-134)

Narrative analysis as used for the purpose of my ethnographic study is a way of weaving together elements of data to form coherent wholes or "stories," rather than separating them into constituent parts. Analysis is shaped in such a way so as to shed light on the uncertainty implied in the research questions through such storied productions. In so doing, this thesis moves away from the popular analytical tradition of "separation and fragmentation" of data towards a way of bringing together and synthesizing data to form coherent wholes. This approach is based on a belief, as advocated by Mauthner and Daucet (2003) among others, "that the researcher, the method and the data are not separate entities but reflexively interdependent and interconnected." As such, analysis, is not seen here as "as a series of neutral, mechanical and decontextualized procedures that are applied to the data and that take place in a social vacuum" (Mauthner & Daucet, 2003, p. 415). Rather, this work acknowledges the role played by the researcher as an "embodied," subjective and situated person who makes "choices" (i.e., how to interpret the voices of participants, which transcript extract to present as evidence and so on), and who works within specific interpersonal, social and institutional spaces which in turn influence these choices.

As such, "knowing" about the lives of estate and factory workers - marginalized women of the third world - takes on a position of "embodied subjectivity - knowledge as thoroughly located in our embodied selves, as against objectified disembodiment," as argued by Dale (2001, p. 58). As Dale further points out, seeing "knowledge as standing apart from the (embodied) person who knows" (2001, p. 58), and believing that it is only by objectifying, fragmenting, and splitting into its constituent parts that we can hope to understand something tends to "replicate the dominant and marginalize the different" in knowledge creation (2001, pp. 26-27). "Women and people of colour" clearly fall within such marginalized spheres, and knowledge about them continues to remain buried in the backgrounds; away from the dominant discourses. If so, adherence to an objectified, disembodied approach would only lead to continuous marginalization of "women and people of colour" in the realm of "knowledge." It is in the light of such thinking, that my study moves away from an objectified, fragmented approach towards a subjective, embodied way of "knowing."

Examining the use of "reflexivity" in ethnographic writing in the light of the above discussion, as Charlotte Davis (1999) states,

reflexivity, broadly defined, means a turning back on oneself, a process of self reference. In the context of social research, reflexivity refers to the way in which the products of research are affected by the personnel and process of doing research. (p. 4)

Plummer (2001) explains reflexivity as,

a much greater social and self awareness of the whole process of knowledge creation; of the participants of the research along with the social spaces in which knowledge is produced, as well as a much fuller sense of the locations - personal, cultural, academic, historical - of the researcher. (p. 208)

As such, reflexivity for the purpose of this study is taken as a practice that contributes not only towards producing knowledge about the workings of the social world but also as offering insights into "how" this knowledge was produced.

Results

The results section of my paper unfolds as an ethnographic narrative woven around my experiences in the field. Embedded throughout this narrative are instances where I was left wondering whether the existing theoretical discussions on "ethnography as a way of feminist research" is fully reflective of the specific ethical political conditions prevalent in a "third world" research setting as I experienced. Drawing on my chosen analytical method- narrative analysis - in this section I attempt to craft a reflective account of my ethnographic experiences comparing and contrasting them with existing theoretical arguments of feminist research principles; specifically, feminist ethnography. As such the discussion of my findings are not separated from but blended into my "stories" as narrated below.

Of "Tea Plukers" and "Sewing Girls": A Methodological Reflection

Jennifer Mason (2002) crafts her fourth cluster of questions on ethnographic design around the issue of "negotiating access." Bryman and Bell (2003) identify a range of tactics that can be employed to gain access to a particular research setting; which include using friends, contacts, colleagues, and academics, getting help from a sponsor or gatekeeper and so on. Having diligently gone through some of this literature during the preparation phase of my fieldwork, I had a meticulous plan on how I should go about the task of "gaining access to a research setting." I intended to put this plan into action as soon as reaching Sri Lanka, my home country where fieldwork was to be carried out. However, I was soon to realize that "negotiating access" doesn't always work out in just the same way as prescribed in "methodology" texts. Rather, as described below, I experienced, "negotiating access" to occur in startlingly different ways and during the least expected moments. As part of my predetermined fieldwork plan, I had initiated contact with colleagues, relatives and friends with a view to gaining access to a plantation estate, which was to be my first research site. I did succeed in negotiating access to an estate through one such contact person. However, by the time I was ready to begin my fieldwork, a few months later; I was told this particular estate was experiencing a situation of industrial unrest and possible strike action by unionized workers. Thus, management was reluctantly compelled to withdraw the permission to enter that they had granted me earlier. This left me in a somewhat difficult situation. With only a limited time during which to gather data, I needed to secure access to an estate as soon as possible.

My six year old son has two little girl friends; they live at the house adjoining ours, back in Sri Lanka. They have a habit of coming to visit us and forgetting to return home even after nightfall. One such night, a young girl whom I knew was the domestic help employed by these little girls' mother came to our gate to remind them to go home. As she stood near the gate under the light of a nearby street lamp, I noticed the red *pottu*[2] she wore, an unmistakable indication of her ethnicity; this was a Tamil[3] girl working as a domestic help in my neighbour's house. I asked her where she was from, and was told that she had been brought to the city from a faraway plantation estate. Later, prompted by this encounter, I asked my neighbour and friend if she knew anyone who worked in a plantation. She did know, for one of her husband's best

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[2] A decorative mark painted on the forehead.
[3] An ethnic minority group making up approximately 18 % of the Sri Lankan population. (www.statistics.gov.lk/census2001/index.html).

friends was in charge of a state-owned plantation at *Hewaheta*[4], the *Ceylonita* estate. She promised to speak to this person, the Superintendent of the estate, about my research project. A few days later, one of her daughters came rushing into tell me that *Nisal* uncle had come to visit them and their parents would like me to drop in for a chat. Nasal, who trusted his friends enough to accept their assurance about my trustworthiness, invited me to visit the estate and to do my fieldwork in any way I wanted. He even invited us, including my friend's family, to stay with him at the estate. I was amazed to realize that within minutes of meeting *Nisal* I had secured "unconditional access" to a large state owned plantation estate situated in the Administrative District of *Nuwara Eliya*, home to the largest number of estate workers in Sri Lanka - one of whom had been bought to the city of *Colombo*[5] to work as a domestic aid and whom I had marked out because of the red *pottu* on her forehead.

I reflect upon my experiences in "negotiating access" in the light of methodological insights or "tales from the field" as related by other feminist researchers where they tell us of their experiences in the field. As Wolf (1996) commenting on the gendered aspects of negotiating access says, entering the field is always difficult and may pose particular challenges for women. In this context, feminist researchers draw upon patriarchal relations, and even play upon their race, their class position, and their status as women, in a strategic manner, to gain access and acceptance in the field. Female researchers working in patriarchal societies (e.g., Abu-Lughod, 1990; Mankekar, 1993) have described having been introduced and gaining entry through "male privilege." As Abu-Lughod (1990), an Arab-American Anthropologist, who researched Bedouin women, reveals, she was brought to the field by her father, who introduced her to and entrusted her into the care of a Bedouin patriarch. Such descriptions shed light on my experiences and remind us that gendered relations are indeed a lived and material part of ethnographic fieldwork. In seeking access to my second ethnographic site, an apparel factory, I was again compelled to adopt strategies somewhat similar to those described above. In this case as well, I relied upon "male privileges" as identified by Abu-Lughod, (1990) and other feminist researchers. I have since considered why I was unable to negotiate access to either of these sites without having to rely upon such "male privilege."

As identified by Diana Wolf (1996) all these are indeed "gendered aspects" of ethnographic fieldwork. Even though I may be able to "methodologically" position my access strategies alongside the writings of other feminist researchers, one question will always linger in my mind. This is a question I am unable to position, either "methodologically," "ethically" or in any other way. Have I been playing the role of an "opportunist" in "negotiating access" to my research settings? Finally, I see this as a state of affairs that calls on feminist researchers to closely consider the implications of reliance on "male privilege" for feminist research, on a much broader and deeper level of analysis.

The final cluster of questions on ethnographic designing, as crafted by Mason (2002) focuses on "relationship" work. Here the primary concern is on how to develop relationships in the research setting? How to gain acceptance? And so on? As I experienced, developing relationships in the field is something that happens naturally and more often than not in unforeseen ways. As such, prescriptions telling us "what to do" and "what not to do" have little relevance once we are in the actual research setting. Relationships spring up and flourish, we gain acceptance, trust and friendship; not through any pre - determined plan, but as part of day to day living and interactions with people. My acquaintance with *Biso Menike*, the midwife of *Ceylonita* estate, as described below, is an example of how the least thought out meeting can turn out to be a most rewarding and enduring relationship.

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[4] *Hewaheta* is the name of a small town located in the Administrative District of *Nuwara Eliya*.
[5] *Colombo* is the commercial capital of Sri Lanka.

On the very first day of my visit to *Ceylonita* estate I told *Nisal* I would like to spend the first few days of my stay getting myself familiarized with the surroundings. He agreed to this and promised to send someone to assist me; it was to be the estate midwife, who had over twenty years experience working in the estate. I was to meet her around 8.00 a.m. on a Sunday. Looking back at my stay at *Ceylonita* estate I now think of this meeting as a turning point in my entire fieldwork in the estate. Indeed, this woman, called *Nona* (madam) by the workers, proved to be my trusted friend and also my main contact person with the "women" right throughout my work. I vividly recall the first time I saw her, slowly walking up the gravel path that ran alongside the tea bushes towards the bungalow. Her white uniform standing out in contrast to the lush green tea bushes from among which she emerged. She was in her mid-fifties and had a kind motherly face, and I instantly knew I had found a friend in her. We had our first conversation standing in front of the bungalow- she declined my offer to come in and sit down. I sensed that the Superintendent's bungalow was not a place that she would be comfortable in. So, I agreed to talk with her outside, where it was still misty and cold with the sun yet to come out of its hiding place in the far off mountains.

As stated earlier, I had prepared vigorous plans and procedures on how to conduct my fieldwork before coming to the estate. Once I was in the field and specially after talking with *Biso Menike*, I reworked this design to suit- what I thought to be - the ground realities. Both these sets of plans proved to be of considerable value to me in the field. However, I experienced another set of events and happenings; unplanned and unstructured, that I now perceive as even more useful than the planned, structured set. Meeting *Rajedra* on his way back from the boutique, talking with the group of women and men who gathered in front of *Sita Devi*'s house, recording Mary's story at her brother's house, being invited by *Nisal* to watch the "labour day," *Parameshwari* coming to sit beside me in the field; none of these happenings were part of a rigid design, but a string of events occurring in the daily life patterns of women and men of this estate. These spontaneous meetings and chance happenings are the ones that gave me access to "data" that I now regard as most valuable and insightful for the purpose of my study.

Just as *Biso Menike* became my guardian angel during my stay at the estate, *Pramila*, a young mother who led a formidable Non-Government Organization (NGO) within the Free Trade Zone (FTZ) in *Katunayake*, became my friend and informant within the factory setting. From the minute, I was introduced to her, this time by a friend of my sister's, I felt the warmth and kindness that was the very essence of this remarkable and courageous woman. From the time garment factories were first set up in Sri Lanka, as far back as 1977, NGOs have been playing a prominent role in safeguarding workers' rights. *Pramila* had first joined a NGO as a young girl to help out her mother - now in her mid-forties and with kids of her own (who in fact even accompanied me on my visits to the boarding houses) she resides in the vicinity of the zone and is engaged full time in looking after the welfare of these female workers.

Pramila now took up my project as her own, and went out of her way to help me in every possible way. It was based on information given by her that I decided which boarding houses to visit, who would make the most suitable informants and so on. Not only did she give me invaluable insights and information about the lives of the "girls," she introduced me to them as one of her friends. These introductions worked to breach any barriers that might otherwise have been created between the "girls" and myself. Being a friend of *Pramila Akka* (sister) as the "girls" fondly called her, was enough for them to accept me as a friend who would neither exploit them nor betray their trust. *Pramila* only asked one thing of me in return for everything she did, she made just one request,

> ...go back and write how my girls live…. what it is like, living inside these little concrete rooms and being worked to death inside the factories.... tell the people who buy the garments they sew, … the suffering "my girls" are going through....

I promised her I would; I intend to keep my promise. In this aspiration, lies the ethical political justification for striving to understand the "lived realities" of their lives, as well as of the lives of "women and girls" living and working in the plantation estate.

Mason (2002) identifies "identity" as another cluster of questions, significant in ethnographic design. Here the focus is on what kind of identity, status or role to adopt? What impressions to create and so on. These questions are thought of as concerning the "ethnographic self" (Coffey, 1999) to be negotiated and renegotiated during fieldwork. Before commencing fieldwork, I had selected the role of "confidante" denoting a mature, attentive, impartial outsider (Bryman & Bell, 2003) as the "identity" I wanted to adopt. However, at the time I was totally unaware of the influence that other actors in the field might have on negotiating my identity. I soon realized that this was indeed the case, for *Biso Menike* thought it her sacred duty to describe "who I was and what I was doing" to the "women" we met, and of course to every living soul we happened to meet around the estate. I had little choice but to let her do it her way, for the last thing I wanted to do was to offend this motherly, kind hearted woman. So, in a least anticipated way the task of negotiating my identity in the field was taken over by someone else. Sometimes I didn't even catch some of the things she said, for the initial conversations were mostly in *Tamil*[6]. The results however, were better than I anticipated. The women welcomed me into their modest homes and spoke to me about their lives; their fears and worries about the present and their dreams for the future, without any reservations or apprehensions. Thus, in spite of the multiple ethnic, cultural and language barriers, I experienced the women as willing to talk with me trustingly and wholeheartedly.

My encounter with *Sita Devi* as narrated below is an example for one such incident. *Sita Devi* lives in the middle division of the estate with her family of three children. Two of her children are attending the middle division crèche; her eldest son is of school going age. All this information I had gathered from *Biso Menike*, before actually going to visit *Sita Devi* one afternoon. The middle division was an area in the estate I had not visited so far, and *Biso Menike* had agreed to come and show me the way after work. It was late afternoon by now, but it seemed as if the last rays of the setting August sun had decided to be kind to the people who were yet to finish their work for the day, for it still gave out some light. We could see women descending from the mountain range with bundles of firewood heaped on their heads, some men were watering the vegetable plots near their houses and young boys were dragging the goats they owned homewards. As we walked further on, the landscape started turning desolate. We had to pass some barren land on our way to *Sita Devi*'s house, and this part of the estate looked to be even more remote and isolated than the other places we have visited. There was hardly a living soul in sight and I was glad I had not attempted this long walk by myself. At last we were there - *Sita Devi* was out doing some washing at a nearby well and greeted us with "*Nona* [madam], how good of you to come this way." She hurriedly went inside the house, leaving her unfinished washing by the well, and asked us to come in. *Sita Devi*'s little house was not part of a long "line" of houses but stood alone, and was in a worse condition than the average "line room" I had visited so far. This part of the estate had no electricity or tap water, and had very few houses, which could more accurately be described as huts made of wood and roofed with dried coconut leaves. It was dark inside the house and we had to bend our heads to get through the door. *Sita Devi* was looking around her searching for a place for us to sit, but all she managed to find was a small stool, which she placed in the middle of the room. She covered the stool with a piece of cloth and invited me to sit on it. Inside the room, I could see a bundle of fire wood as the only other item of furniture. I suggested that we talk outside, as it was not yet dark. This proved to be a good idea, for we were soon provided with some chairs

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[6] Language spoken by the Tamil community.

from a nearby house and more importantly were joined by some more women. I asked *Sita Devi* how old her children were, and she looked at *Biso Menike* for help. *Biso Menike* laughed at her, but she, unlike the mother knew the ages of all the children and even their names. *Sita Devi* was carrying her youngest child, a one year old boy, while she talked with us, and her husband stood nearby, carrying the other. After the initial introductions, I asked *Sita Devi* who took care of the children while she was at work:

> I have no one of my own [meaning female relatives] to help me with the children. My family is at *Lulkandura*[7] estate. I came here only after getting married. I take the children to the *pulle kamaraya* [crèche] and the *pulle amma* [crèche attendant] takes care of them.

Recalling the experiences of the birth of her three children, *Sita Devi* told me she had been paid the "three month bonuses" for all her children. She had used the money to pay off some of the family debts. "We would have liked to repair the house with some of it...but the debt owner came after us and made us pay him," *Sita Devi*'s husband said regretfully. Another woman who had joined in the conversation said she had done the same with her "child birth" money. At this point *Biso Menike* took it upon herself to remind the women that the money given to them after child birth was meant to be used for the new baby and the mother, for giving them nutritious food and so on. The women openly laughed at her for this, remarking:

> *aney Nona* ! [oh Madam], you know very well how poor we are. We always use this money to pay our debts...that is why we have three children...and come to you after that [meaning to get advice on family planning].

After this candid remark by one of the more outspoken women *Biso Menike* made no more mention of what they should or shouldn't do with this money, which the women called the "three months bonus." By this time, we were joined by two or three men, who apparently had felt encouraged by the presence of *Sita Devi*'s husband in the group. So I had one big group of people gathered in front of *Sita Devi*'s modest house, all ready to talk and share their life experiences with me.

Reflecting on this encounter, which gave me rich and insightful information about the productive and reproductive labour roles of these women workers I don't think "negotiating identity" had any significant impact on my data collection efforts in this setting. In fact, *Sita Devi* didn't even ask the reason for my visit before inviting me to her house. She was happy I had come to see her and was willing to talk with me. Of course, I explained why I had come in great detail, but I don't think she bothered too much about that. She was happy to talk of her troubled life to anyone who cared enough to listen, so were all the other women and men who joined me that late August afternoon.

Reflecting on my experiences in the factory setting, negotiating identify took on a somewhat different form

The factory setting itself, as I describe below, was vastly different from the estate setting. The first time I visited the area I was overcome by a strange feeling of disbelief. This was different from anything I have ever experienced in my life -families living in slums were a sight I was very much familiar with. But this was different. There were no families here, only young girls around the age of 18-25 could be seen in and around the small rooms, built of

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[7] An estate situated nearby, which incidentally is home to the first tea plant to be planted in Sri Lanka.

concrete blocks and roofed with asbestos sheets. These were small blocks of land - fenced in by barbed wire - each holding some 30 to 40 rooms, and hardly anything else. There were no trees, no free land, no breathing space, nothing but heaps of concrete, asbestos and barbed wire. The land owners around the "zone" certainly knew how to make the most of whatever land they possessed, for the entire landscape was dotted with makeshift structures of varying shapes and sizes, all serving as boarding houses or rooms for the "girls." At first glance the whole place appeared to be unreal, a scene out of a movie maybe. It was too overcrowded, too congested, too appalling to be real. But it was, this entire set - up and most of all the scores of young "girls" I saw outside their "rooms" this Sunday morning were indeed real. This was how they lived. These were the "homes" to which they returned after eight or more hours of arduous work at deafeningly noisy factories at the other side of this land. It was around 11.00 o'clock in the morning by now, but I could see some of these "girls" had only just got up as they were washing their faces near a common well[8]. I later learned they had worked the night shift and had come back only at 2.00 o'clock in the morning. Some others had piles of clothes to wash, some were chatting with friends, and the few who were inside the rooms were getting ready to cook lunch. I got to know this because once the doors were opened everything that was happening inside the rooms could easily be seen by anyone walking down the road. So even before entering her room I could see *Dishanthi*. She was standing in front of a small table washing some rice, and the first thought that stuck me on seeing her was, how pretty and refreshing she looked even within these seemingly unpleasant surroundings. Her boarding house, for which she paid (as she later told me) one third of her monthly wage, consisted of a single room; which served as the bedroom, the living room and the kitchen. It had a single window and a wooden door in front, through which *Pramila* directly walked into the room, calling out in her loud cheery voice, "I have brought a visitor to see you," whereupon *Dishanthi* who was standing by the table on which she had all her groceries and cooking utensils including a small cooker, looked up. "Why didn't you tell me you were coming *Pramila Akka*, I would have postponed cooking, now that I have put the rice in the water, I will have to finish washing it and keep it on the cooker, or it will get spoiled." Both *Pramila* and I told her to finish what she was doing, and she asked us to come inside and sit down on her bed which was only a few feet away from where she stood cooking. There was a small television set on a stool, a fan that stood near the bed, a chair and a suitcase, in addition to the bed and the table in the room. So far *Dishanthi* hadn't asked what I was doing inside her room. Then *Pramila* explained the purpose of my visit to her, like she did to all other girls we met in the zone, telling them that I was gathering data for a "research."

In contrast to the estate workers who didn't seem to care who I was here, as soon as the word "research" was mentioned, the "girls" identified me as an academic researching the lives of garment workers. One of them even asked me if I had met an USA based Sri Lankan scholar, researching the "identity" of female garment workers in Sri Lanka. As far as these "girls" were concerned, my "identity" was already partly created, and I found myself having to step into a half way moulded model. As Wolf (1996, p. 2) argues the type of "identity" that was more or less enforced upon me, in this setting would lead to "power differences" and to "unequal hierarchies" that are created, maintained, and re-created during and after fieldwork. Wolf (1996) identifies three levels of such differences as: power stemming from differences in race, class, nationality, life chances, urban-rural backgrounds, power exerted during the research process, and finally power exerted during writing and presenting (p. 2). According to Wolf (1996)

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[8] A well is a deep hole in the ground from where water was drawn out.

the first level of power difference cannot be avoided if one is studying marginalized or poor people. She also states that while "first world women" may experience multiple levels of difference when working in "third world countries," postcolonial feminists working in their own countries experience their class and educational privilege, at the very least. (p. 2)

While agreeing with Wolf's identification of the junctures at which power differences are likely to arise, I fail to identify any material evidence for seeing such differences as insurmountable in so far as my experiences in the field are concerned. Rather, I experienced numerous instances where such "power differences" were circumvented by "women and girls" keen to relate the stories of their lives to an "outsider" who was willing to listen. I attribute such evading of "differences" partly to being accompanied by a person who was not just respected and trusted, but loved by the "girls" as an elder sister and partly to a socio-cultural heritage embedding values of friendliness and openness even with "outsiders." As I sat on the bed inside *Dishanthi*'s tiny room with *Pramila*, talking with her while she cooked lunch only a few feet away; when *Dilrukshi* walked into the room and sat down in the small space between the two of us; when *Pramila* picked up and gently stroked her recently infected hand; there were certainly no insurmountable "power differences" among us. Each of us was different, yet all of us were also similar in significant ways. There was an unspoken understanding; a common thread that bound us together as women workers of a third world/postcolonial location, each of us, in our own way, resisting the oppressive structures imposed upon our lives.

Furthermore, these events and happenings as I experienced fail to reinforce the views as expressed by Judith Stacey (1988) where questioning the feminist values embedded in ethnographic fieldwork she argues that "inequality and potential treacherousness of relationships (formed in the field) are inescapable" (p. 23). I do not see my relationships with any of these "women and girls" as exploitative, neither do I view the stories of their lives that they so willingly shared with me, merely as data or "grist for the ethnographic mill." As far as I am concerned, there is an underlying purpose for striving to "listen to and write" their stories. As these "women and girls" of the estate and the factory themselves told me, very few people want to listen to what "they" have to tell about their lives. The way they "live and work" is accepted as the way it "should" be. Few if any, bother to question the status quo. To question would be controversial; for it is this feminized work force that energize the two most viable and valuable industry sectors of the country[9]. If few want to listen to them, fewer still want to write about them. The socio-political milieu in which such writing takes place could easily make it counter- productive for the writer. Within these contextual peculiarities, my interactions or relationships with these "women and girls" fail to fit in with the descriptions of "exploitative" and "treacherous" as feared by some feminist writers themselves. The "girls," who unlike the writers raising such concerns live within these settings, seem to be aware of these circumstances, for during the last few days of my stay with them, *Dishanthi* casually told me:

> …I still can't understand why you choose to come and talk with us…after all, very few people even like to visit the areas where we live, …let alone talk with us. Either they are afraid or they just don't' bother. …after all, they think of us only as "stupid garment girls."

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[9] Garments and Tea bring in the highest amount of export earnings to the country. Both industries depend on a heavily feminized labour force.

The scorn in her voice was evident when she told me this: "Stupid" is the last word I would use to describe *Dishanthi*; she is one of the most intelligent and courageous girls I have ever met in my life. She is a young girl who dares to resist an unimaginably powerful capitalist setup often at immense personal risk. Explaining further, during the period 1989/90 (known as the era of terror) more than 60,000 young Sinhalese men and women were tortured and killed by state backed para-military forces in the South of Sri Lankan. They were activists of a leftist political party which spearheaded a brutal armed uprising aimed at overthrowing the democratically elected government and capturing state power. The insurgency was aborted, but the fear instilled in the minds of people, had left a void of dissident thinking and activism yet to be filled. This is the context, in which *Dishanthi*'s is attempting to organize female workers inside the factories against exploitative working conditions. As such, I do not see listening to and writing about her "life and struggles" as containing "elements of inequality, exploitation, and betrayal" (Stacey, 1988), and I do not think she would either. In fact, what most of these "girls" expected of me was to write about them, because the world needed to know they existed, and more importantly "how" they existed, in their tiny space in a distant third world location.

However, what I write about these "girls" could be subjected to a test of "representation." They do want me to write; that much was clear to me right throughout my interactions with them. But how can I make sure that I am writing what "they" want me to write? And not something "I" want to write. How can I get a judgment on this? Who is capable of making such judgments? These are deeply problematic issues to which I do not have clear cut solutions. Indeed, the best solution would be for the "girls" to write about their own lives. In fact, once I asked *Dishanthi* about this, and she replied:

> …we do, when we can find a little time, and *Pramila Akka* [sister] prints our poems and essays in her paper. But we have to be very careful what we write…we have not forgotten about what happened to *Menike*. …. our paper is read only by the "girls" and some of the managers [for the purpose of surveillance] …for us, it is what we already know.

Menike's fate, which resulted from her writing a poem titled "life" describing her life in the "zone" and working conditions of *SriKnit* garments, is well known within the industry. The poem, through an oversight, had been published with both her name and the name of the company included; within days she lost her job and had to return home. Even though she was later reinstated, the event had apparently left its mark. Even when they did write, the "girls" were cautious about what was written. The situation in the estate, as I describe below, is drastically different from what I experienced in the "zone." During the second day of my visit to the estate, the women (and some men) were paid their monthly salaries. I happened to be inside the office when the women came into the office to be paid. There was an opened ink pad on the desk, one by one the women dipped their right hand thumb in the ink pad and pressed it on the pay sheet. They were legally certifying the receipt of payment of their wages. This done, they took the money in both hands and carefully tucked the notes inside their jackets. I did not ask the question, "Why aren't they signing the pay sheet?" I already knew that none of these workers could read or write.

As argued by some feminist theorists (e.g., Lather, 2001; Stacey, 1988; Visweswaran, 1997) ethnographic traditions of romantic aspirations about giving voice to the voiceless are much troubled in the face of the manipulation, violation and betrayal inherent in ethnographic representation. Visweswaran (1997) raises suspicions of "the dangerous ground between intimacy and betrayal" that characterizes feminist work intended to "testify" and "give voice" (p. 614). She engages with the limits of representation and the weight of research as surveillance. While agreeing with such concerns about the "limits of representation," I

nevertheless perceive a "need for representation" that outweighs this concern, as far as the lives of the "women and girls" of my research are concerned. I identify a gap in writings about them that applies to both settings but for somewhat different reasons. Perhaps this justifies my attempt to "represent" them through my writing; how far I am able to do so, however, remain problematic. As I see, it is only these "women and girls" themselves who would be qualified to make a final judgment of my endeavour. As I presume, any accusations of "violence in representation" would be less harsh than an accusation of not writing at all; leaving them to live their lives as they always have, out of theoretical debates and discussions, out of history itself.

Whenever I met and talked with anyone in the estate, I (or *Biso Menike* if she happened to be with me) made it a point to explain the purpose of my visit to them beforehand. The same is true for my interactions with "girls" within the environs of their boarding houses in the "zone." As such, in the majority of my fieldwork encounters I was following the principle of "informed consent" - a salient feature of ethical fieldwork practice. However, there was one instance where I faced a dilemma of practice in this regard. Inside the premises of SriKnit garments I was "requested" not to speak with any of the shop floor workers, so that I would not disturb them and disrupt the work flow. I agreed to this, indeed I had little option, if I wanted to visit the shop floor. So while I stood in my assigned corner among the rows of noisy sewing machines and silent sewing girls, observing them at work, I did not have their permission or "consent" to do so. Neither did I have any opportunity of obtaining their consent for I was watched over (under the pretense of assistance) by someone from management, every minute of the time I spent on the factory premises. Having concluded my fieldwork, I was left with the issue of whether or not to include what I heard and saw while on the shop floor, in my writing. I have decided to include them, as this understanding of the conditions they worked under is crucial to their stories.

Finally, reflecting upon my experience, even though I had meticulously drafted schedules of how I should conduct my interviews with "women" and lists of possible questions to ask them, I rarely, if at all followed them to the letter. In fact, how I did conduct these "interviews," which I prefer to call as informal meetings or gatherings, was to let them "evolve naturally." I found that the "wome"' had stories to tell me which were by far more insightful and relevant to my study than the questions I had prepared beforehand. So, I let them relate their stories, in their own way, taking their own time. When I did intervene, it was to make them tell me a bit more about an issue I found particularly insightful. My meeting with *Krishna Devi* at her "line room" where I prevented *Biso Menike* from translating and asking any of "my" questions from this elderly woman, but attentively listened to the powerful and painful narration of her past and present life in the estate, is an example of such methodological reworking. During many such instances, I merely watched and listened; seeing the multiplicity of expressions appearing and disappearing on their faces, hearing the tones of resentment, detachment, hopefulness and eagerness in their voices as they told me their stories. Everything I saw and heard made its own impression in my mind. At the end of my fieldwork, I had stacks of field notes, hours and hours of recordings and hundreds of photographs. But to me, most precious and powerful are the images of these "women and girls" as they live in my memory; *Lakshmini*'s tired face, the anger in *Rajeswari*'s voice, the despair in *Krishna Devi*'s words, power of *Dishanthi*'s resistance and *Dilrukshi*'s beautiful smile. It is these memories that inspire me to write the stories of their lives and to reflect upon the specific contexts within which their stories should be situated and read.

Reflecting finally on the limits of my "stories" as Grosz (1995) points out "no matter how aware and reflexive we try to be, "the author's intentions, emotions, psyche, and interiority are only partially accessible to the readers, in fact they may be only partially accessible to the author herself" (p. 13), My reflexive account as narrated above is inevitably situated within

such "limits of reflexivity." Simply, the intentions and emotions which I claim to reflect upon may only be partially accessible even to me, if so, they will inevitably only be partially accessible to the readers. Further, I do not argue my stories to be the stories of all female workers living and working under similar conditions across the "third world." Rather, as I believe, the findings of qualitative research are to generalize to theory rather than to population. And it is the quality of the theoretical inferences made out of data that is crucial to the assessment of generalization in qualitative research. (Bryman, 2011). In summary then, my reflective account is woven around the question; what is the extent to which "existing principles of feminist ethnography" reflect the context specific ethical political issues as experienced by feminist researchers working in third world/postcolonial locations? Embedded throughout this narrative are situations where concerns raised by feminist ethnographers on issues such as; possibility of betrayal of the informants' by the ethnographer, producing ethnographies that pay lip service to emancipation of participants, power differences maintained throughout the research process and representational violence have failed to fully reflect the context specific ethical political issues as experienced by me; a feminist ethnographer working in a third world location. Here I have also reflected on the tensions and dilemmas I have experienced when compelled to breach the "accepted" principles of feminist ethnography in finding out and writing about the lives of women workers – "tea pluckers" and "sewing girls" of Sri Lanka. This reflective narrative further enriches the debate of "ethnography as a method of feminist research" by bringing within its fold, stories of a group of marginalized women workers of colour, as told by a feminist "story teller" of colour - stories that have mostly remained "unsaid," hidden and outside the realm of feminist methodological concerns. This study opens up a space for future researchers working under similar conditions to reflect upon their own fieldwork experiences - adding their stories to mine - and thereby creating a new realm of knowledge; knowledge of feminist methods from a third world perspective.

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