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Gender, Reflexivity, and Positionality in Male Research in One's Own Community With Filipino Seafarers' Wives

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Key words: reflexivity; positionality; fieldwork relations; male feminism; male research with female participants; representation; narratives; interviews; seafarers' wives; Ilocos Norte, Philippines

Abstract: This article reflects on the epistemological, methodological, and ethical issues related to undertaking a cross-gender research (male researcher with female participants) in one's own community. It also examines issues of analysis and representation germane to taking a gendered perspective in this study of the lives and experiences of left-behind women. The article frames the discussion of these issues within four interrelated sites or levels of reflexivity: theoretical reflexivity, gender and fieldwork relations, positionality and the insider/outsider dynamic, and representation. The conclusion reflects on the ethical obligation a researcher conducting a study in one's own community bears and the consequences of this ethical burden on representation.

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1. Introduction

This article reflects on my cross-gender research with Filipino women married to seafarers working in the global maritime industry. The research, which I undertook in my home town, examined the spatio-temporality of subjectification, or the process of becoming historical actors. More specifically, I examined the spatial and temporal dimensions of the women's identity, agency, and subjectivity and located this examination within how they navigated their lives that were so shaped by the alternating absence and presence of their husband and by the contractual nature of their husband's work (GALAM, 2011). I looked at the seamen's wives not only as being affected by, and responding to, the consequences of migration but as also contributing to migration processes. In other words, I looked at the place of these women dialectically: I sought to

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examine how the times and spaces of migration directed them and how they, in turn, directed the times and spaces of migration. Hence, I examined whether and how the subjective experiences of these women may be linked to wider social and political histories and narratives bearing in mind that they were not the migrants themselves. Here, I probe the ontological, epistemological, methodological, analytic, and ethical issues engendered by my attempt to answer this question and in attempting to do so by conducting the research in my own community. I conducted my fieldwork from February to September 2010 in San Gabriel (a pseudonym) in the province of Ilocos Norte, northwest Philippines. The semi-structured interviews I conducted with 40 women generated rich narratives about their lives and experiences. [1]

This article first provides a short introduction to my research, situating its concern for the potential for subjectification among my participants opened up by international migration. It locates my research within migration scholarship in the Philippines and explains why the research was conducted in my own community. In doing so, the article highlights and unpacks my positionality with a consideration of my studying my own community and my male gender, and I frame this discussion within different levels of reflexivity. I conclude with a reflection on the ethical implications of studying my own community and of my male gender on the investigation and interpretation of women's experiences as left-behind wives.[1] [2]

2. Positioning the Research

My research's aim to understand the process of subjectification among seafarers' wives was framed within the possibilities for the renegotiation of gender ideology and relations among women married to migrant husbands opened up by the restructuring of Filipino families caused by migration (PARRENAS, 2005), that is, by the absence of the women's husbands for long periods of time. Left-behind wives, through the responsibilities and tasks they do in the absence of their husband, sustain male emigration (BRETTELL, 1986; KANAIAUPUNI, 2000). As Shawn KANAIAUPUNI (2000, p.2) has written:

> "women and their labours in origin villages are crucial to the migration process—*they make men's migration possible and ensure its continuity across space and time*. As such, they form the invisible backbone of this transnational migration process that has endured for over a century." [3]

Since the 1970s, the Philippines has pursued an aggressive labor export policy. In the last few years, the Philippine government, through its Philippine Overseas Employment Agency (POEA), has aimed to deploy at least a million workers overseas (TYNER, 2009). In 2013, more than 1.8 million Filipinos were deployed for overseas work. The export of labor has become a major part of both economic and foreign policies of the Philippine state, and a structural feature of the

1 Although I am aware of the negative connotations of the term "left behind" (see, for example, ARCHAMBAULT, 2010), I use it because the Filipino seamen's wives I interviewed referred to themselves as *nabati*, best translated as "left behind."

country's political economy (LINDQUIST, 1993; RODRIGUEZ, 2010). The Philippines has become very dependent on foreign remittances. Around 21.4 billion US dollars were sent by over ten million overseas Filipinos in 2012. This amount represents around 8.5 per cent of the country's gross domestic product (GDP). [4]

Aside from examining how the absence of the women's husband might provide experiences for the women that might lead to changes in their identity and agency, my research sought to help address, if not redress, the peripheral status of seafarers' families in Philippine migration scholarship. Migration scholarship in and on the Philippines has predominantly focused on land-based workers and has generally paid little attention to seafarers (AGUILAR, 2002; CONSTABLE, 1997; FAIER, 2009; GUEVARRA, 2009; PARRENAS, 2001; PINGOL, 2001; RODRIGUEZ, 2010). Although seafarers represent about 20 percent of the annual total number of deployed overseas Filipino workers (OFWs), they remain marginal in Philippine migration research. Even more peripheral are their families. Where migration research has examined the lives and experiences of those who stay behind, it has focused mainly on the families of land-based overseas workers. The Philippines is the global shipping industry's biggest source of seafarers; more than 25 per cent of the world's total work force of 1.4 million seafarers are Filipinos. In 2012, almost 367,000 Filipino seafarers were working onboard international ships (IOM, 2013). The Philippines thus provided an ideal case with and through which to examine how non-migrant women become implicated in overseas migration processes. [5]

I chose San Gabriel as my research site for its long history of migration, particularly to Hawaii. Internal migration, which began in the 1850s, and international migration, particularly to Hawaii and California in the USA, which began in the first decade of the 1900s, have been used to escape the economic and social limitations in the Ilocos Region. In order to see how migration brings economic and social improvement, and to get a sense of how migration is invested with the hope for improved life chances (see GALAM, 2015), it was necessary to conduct the research in a place that has participated in, and been significantly affected by, migration. Not only is there a large number of people from San Gabriel who have immigrated to the United States and elsewhere, it also has a considerable number of labor emigrants, those who left the country for overseas jobs through the labor export policy of the Philippine state. [6]

3. Reflexivity and Conducting a Cross-Gender Study in One's Own Community

The examination of researchers' relationship with meaning, interpretation, representation, knowledge, and power in their work has become a necessary part of research methodology and practice particularly among qualitative researchers. There have been searching and self-critical examinations of reflexivity in sociology (KUSOW, 2003; SMITH, 1987; STANLEY & WISE, 1983), anthropology (BRANDES, 2008; DAWSON, 2010; ENGUIX, 2014; LAMPHERE, RAGONE & ZAVELLA, 1997; NARAYAN, 1993; SHERIF, 2001; WOLF, 1996),

and geography (CHACKO, 2004; ENGLAND, 1994; GILBERT, 1994; McDOWELL, 1992a; ROSE, 1997; SULTANA, 2007; TURNER, 2013) especially in feminist research. In addition, scholars have highlighted the importance of exercising reflexivity to understand how gender and gender relations affect or constitute interviews (HEROD, 1993; KUSOW, 2003; McDOWELL, 1992b; SCHOENBERGER, 1992; TAKEDA, 2012). This section focuses specifically on issues of reflexivity and I have categorized four interrelated areas through which to discuss different forms and levels of reflexivity. These are theoretical reflexivity, gender and fieldwork relations, positionality and the "insider-outsider" dynamic, and representation. Methodological and ethical issues germane to each of these sites are addressed accordingly. [7]

3.1 Theoretical reflexivity: Male feminism

The question of men being and becoming feminists has been a controversial one (ADU-POKU, 2001; ALILUNAS, 2011; DIGBY, 1998; FLOOD, 2011; GARDINER, 2002; KIMMELL, 1998; LEMONS, 1997; MURPHY, 2004; SHEPHERD, 1997; STANOVSKY, 1997) especially because feminism was involved in "the critique of masculinist knowledge" (McDOWELL, 1992a, p.401). For Gary LEMONS (1997, p.50), however, men and feminism were not incommensurable nor even incompatible: "The idea that men cannot in feminist alliance with women politically subvert the power of male supremacy is like saying white people in anti-racist solidarity with black people cannot divest themselves of white supremacist thinking." [8]

The important role of men in the feminist struggle has been noted:

> "Men who advocate feminism as a movement to end sexist oppression must become more vocal and public in their opposition to sexism and sexist oppression. Until men share equal responsibility for struggling to end sexism, feminist movement will reflect the very sexist contradictions we wish to eradicate. [...] In particular, men have a tremendous contribution to make to feminist struggle in the area of exposing, confronting, opposing, and transforming the sexism of their male peers" (HOOKS, 1984, pp.80-81). [9]

Nevertheless, the position that feminism was the preserve of women was widely held. As GILBERT (1994) pointed out, although there was much debate about what constituted feminist epistemology and methodology, it was generally accepted that feminist research should be "by, for, and about women" (GLUCK & PATAI, 1991, p.2 as cited in GILBERT, 1994, p.90). Thus, to justify their feminism, male feminists called into question the "natural" link between women and feminism and argued that feminism should be about positions. HEATH (1987, p.1) claimed that women "are not feminists by virtue of the fact alone of being women: feminism is a social-political reality, a struggle, a commitment, women become feminists." HOPKINS (1998, pp.51-52) argued that "feminism should be characterized by adherence to a basic set of beliefs and political positions ... the core of feminism would be feminist positions, not women's experiences." [10]

Over the years, feminist scholarship has shifted emphasis "away from women towards gender, allowing issues about the social construction of and geographical variations in masculinity as well as femininity to be raised" (McDOWELL, 1992a, p.400) and "women have cautiously called for men's participation in feminist movements" (PLEASANTS, 2011, p.231; see also EDWARDS, 2008). In addition, male scholars studying masculinity have pointed out that this required looking at femininity (BRANDES, 2008; GUTMANN, 1996). As BRANDES (2008, p.151) wrote, "you cannot study men with taking account of women, and vice versa." Increasingly, attention has been drawn to the need to build alliances and solidarities between men and women feminists. Rather than see feminism as exclusive to women, it should be seen as "sites for a discourse that acknowledges the connection between feminist concerns and issues of race, class, and sexual orientation" (BREEZE, 2007, p.60). Feminism requires the participation of a diverse population especially because feminist issues are deeply rooted in structures and relations of power. An intersectional politics that linked gender and other structures of power such as race, class, ethnicity, and nationality (CONNELL, 1995; GUTMANN, 1997; PLEASANTS, 2011) was seen to advance the sharing of feminist values and principles (SHEPHERD, 1997). Men could thus speak and act as feminists but their doing so is premised on collaborative and participatory voice and action. They speak with women (BREEZE, 2007; GALAM, 2008; STANOVSKY, 1997), not for or in behalf of them. My reflections on the methodological, analytic, and representational strategies I made hopefully will make explicit and demonstrate how I speak with the women who participated in my research. In critically examining these choices I made, I lay bare the ways by which I participated in making sense of how they navigated their lives and experiences defined by the alternating absence and presence of their husbands, an absence-presence caused by the limited social possibilities available in the Philippines, and that became the condition of possibility of their subjectification. [11]

The remaining sections of the article examine how I have been able to undertake my research using a feminist perspective, the challenges I confronted, and the decisions I made, which have epistemological, methodological, representational and ethical implications. [12]

3.2 Gender and the negotiation of fieldwork relations

Reflections on the impact of gender on the negotiation of fieldwork relations, particularly by researchers whose participants included genders other than their own, have spotlighted how gender is central and marginal to the research depending on the situation (AL-MAKHAMREH & LEWANDO-HUNDT, 2008; ENGUIX, 2014; GUTMANN, 1996, 1997, 2002; KUSOW, 2003; TAKEDA, 2012). This situational and contextual appreciation of the role played by gender in fieldwork relations engenders a dynamic perspective of how gender interacts with other social and cultural categories and factors germane to the research. It underscores the negotiatedness and relationality of research and compels the researcher to pay attention to where gender facilitates research and where it establishes boundaries. McKEGANEY and BLOOR (1991) noted that the cross-

gender nature of their study affected the physical spaces they had access to. BRANDES (1992, p.33) reported on his fieldwork in Andalusia where strict gender domains or sexual boundaries prevailed and which defined his "informant pool and research possibilities," and as a consequence, his access to the women was gained through their husbands (see also BRANDES, 2008). Although PINGOL (2001), who interviewed the husbands of migrant women, noted that gender did not pose serious problems to her relationship with her participants, she nonetheless acknowledged that she was able to discuss sexual matters more openly with the wives because she was also a woman and a wife. [13]

Prior to my fieldwork, I was concerned about how my male gender was going to affect my ability to invite and recruit women to participate in my study. There was also the matter of how the cross-gender nature of the study was going to affect the interview itself. Admittedly, the sensitivity of some of the issues the research was dealing with, particularly the absence of sexual intimacy, caused me this concern. As a male researcher whose participants were women, I thought that there were questions and, therefore, spaces of these women's lives that were already foreclosed to me or that I would have difficulty negotiating. Two things might be said of how gender shaped my research. First, gender affected the kind of questions asked, the degree of probing done, and how comfortable the women and I felt during the interview, that is, the "micro-world" of the dialogic constitution of meaning and sharing of stories. Even then, other factors were present. First is the fact I am married and have children and away from my wife for seven of the eight months of the fieldwork. (Participants knew my marital and family status because they asked about these things when I was inviting them to participate in my study). Indeed, in many interviews, women said that as a father and a married man, I knew and understood what they were saying, for example, about the difficulties mothers face in disciplining children when the family's traditional authority figure is absent. Although this allowed me to draw on a discourse of "shared experience" and enabled me to exercise *Verstehen* (empathetic understanding), there was a limit to what we shared. For example, I did not live with my in-laws, whereas many of my research participants did and which vitally shaped their lives and experiences as women whose husbands were away. Nevertheless, the point is that my being married and a father helped me and the participants negotiate certain topics and aspects of their lives. Furthermore, even more importantly, my being married was crucial in securing the participation of a number of the women. They explicitly indicated that it was an important factor in their participation. My analysis of the dynamic of our conversation when I was inviting them to participate (particularly the questions they asked which suggested to me the concerns that were most important to them) indicated that they were weighing how their participation was going to be perceived by others (family members and relatives they lived with in the same house or neighbors who are either their own or their husbands' relatives). In other words, they were already considering the potential of their participation for being framed by others within infidelity. I thought that their questions about who I was and where I lived were not only to determine whether I was someone they would trust. They were also concerned about protecting themselves from suspicions of sexual misconduct. [14]

Second, there was the wider environment within which the interview was located and in which gender was only one factor influencing it. Cultural, social, and age factors dictated the amount of privacy I had in interviewing the women, and to a certain extent, the degree of openness that we could talk about private and intimate matters. Domestic arrangements, for example, shared residence, determined in which part of the house I could interview them. A few women, particularly those below 30 years old, were initially accompanied by another woman who would eventually leave to go elsewhere in the house. The husbands of two women were present during their interview and participated in the process. Although I thought that they would have left us alone had I been a woman, I was convinced their "interest" also lay in what would be talked about. Despite their presence, I was still able to ask my questions on intimacy. [15]

While I was rightly worried about how gender would affect how well I would be able to undertake the research, reflecting on the issue later on led me to agree with LOIZOS (1992, p.173):

> "It is not so much whether the fieldworker is male or female, *tout court,* which is likely to be decisive, but the combination of gender, age, marital status, and topic of research interest. If all the values in the equation are 'wrong', then the researcher is indeed disadvantaged." [16]

To what extent these values might go "wrong," I argue, depends in part on the character participants see the researcher to possess or demonstrate. In interview-based research, there is very little time, if at all, for researchers to cultivate a "reputation" or character based on interaction between them and their interviewees prior to the interview. That reputation preceded me by way of the people who introduced me to them. It was my responsibility during the interview to build on that. PITT-RIVERS (1992) writes that in research, there is the element of presenting a certain kind of persona, a projection of character meant to win acceptance. A researcher needs "a certain amount of charm or at least the sincere desire to make of himself agreeable to persons whom he might not in other circumstances have chosen as companions" (p.140). I certainly made every effort to make myself agreeable to the women and the people around them. Where other family members were around during the interview, I made sure to say "*addaak*, *tata*, *nana*, *manong* or *manang*" (a way of paying courtesy to them; *tata* for their father, *nana* for their mother, *manong* for an older male sibling or relative, *manang* for a female one). This was one way by which I negotiated my relationship with the participants prior to the start of the interview. All throughout my interaction with them, I used terms of respect (*manang* for women older than me; *ading* for those younger). I spoke in a tone and manner that I thought would indicate my respect for them. Michael BLOOR (2001, p.392) has written that

> "[a]ll encounters between researchers and researched are species of social relationships governed by conventions of politeness and etiquette; in the case of ethnographic research the relationship in question may well embrace fondness and regard. Fieldwork methods and fieldwork relations will shape the nature and content of members' responses." [17]

Below, I reflect further on my fieldwork experiences, ones that not only highlight the negotiation of fieldwork relationships but also methodological and ethical issues. [18]

Being accompanied by someone the women knew was crucial in speaking to the women and in minimizing suspicion about the purpose of my research. Kinship ties or the ability of the women to place me within "familiarizing" systems helped me recruit them to participate. In a number of instances, I was first asked what my surname was, which family I came from, who was my mother, etc. All of these were ways of identifying me in their own terms rather than relying on the researcher identity stamped on the research information sheet. The information sheet, which was in Ilokano, the native language spoken in the place, was not of much use. The women were more receptive and more positively predisposed to giving me a chance if I personally talked to them. Three women agreed to be interviewed because they knew my family or one of my siblings belonged to the same high school class as they did. They made clear they were agreeing to be interviewed because they knew my family or could link me to someone they knew. I think that this was their way of being assured that whatever it is that they were going to say, they knew that they told it to someone they could trust and someone trustworthy because ‘known' to them through other members of my family. [19]

Not all introductions, however, worked. I had seven rejections and two who backed out. Various reasons were given (although I did not ask for any) some of which had nothing to do with the research. One did not want to be interviewed because a week before I spoke to her, her house was burgled and was suspicious of anyone wanting to ask her about her life as the wife of a seaman. Understandably, she did not want someone she hardly knew to be inside her house. Indeed, there were a few who openly expressed suspicion about my intentions. One asked me whether I was doing surveillance (*agtiktiktik*). The women's initial hesitation was overcome when I assured them that I was not going to ask them about how much their husband earned although I made it clear I would ask them about budgeting. The wife who thought I might be snitching was reassured when I told her I had interviewed other women in a neighboring village one of whom referred her to me. She asked me who had given me her name and as I could not remember who of those I interviewed had done so, I mentioned, with hesitation, an interviewee off the top of my head, which was a marvelous stroke of luck as she happened to be this woman's best friend. (She probably had an inkling it was her friend who gave her name to me). She then quickly called her up by cell phone to confirm whether I had been telling the truth. She agreed to be interviewed after that phone call. [20]

Revealing the identity of one participant to a potential one is an ethical issue that brings up another. If the only way a potential interviewee can feel reassured about participating is by knowing who else has participated, which is also their rapid way of gauging a researcher's trustworthiness, is this a justifiable reason to do so? What is involved here is an "economy of trust" and as a researcher, I decided in favor of giving the information. In the case that prompted this

reflection, the potential interviewee was referred by another I had interviewed and as such, suggests that the former would have the right to know who had referred her to me. [21]

Methodological and ethical concerns about anonymity might be raised specially in the case of participants who were interviewed in the presence of a third party. One could ask why I did not arrange for the interviews to be done in a neutral and more private place. There were very few establishments in the town where I did my fieldwork that could have served this purpose. There is a snack house in the town center (*poblacion*) where I could have done the interviews but such a "neutral" place would not have given the participants and me the quiet and privacy demanded by such methodological and ethical considerations. It did not guarantee their anonymity and the confidentiality of what they shared as others would most likely have overheard our conversation. [22]

Interviewing the women in a "neutral" place to ensure their anonymity would have been more methodologically and ethically questionable as it would have put them in a difficult and compromised position. The women would certainly not have agreed to such an arrangement. I cannot overemphasize the power exercised by the absence of the women's husbands in terms of their behavior and mobility, particularly with being seen with a man who is not their relative. This is specially the case with the younger wives I interviewed. The women were very concerned that people who know their husbands would talk about them being seen with a man and that stories would be spun around it. That some of these women were accompanied by another during the interview in their own place of residence gives an indication of the care they took to be above any suspicions of infidelity. That others (including their neighbors) know that a male visitor came to speak to them helps vouch for their character. Earlier, I talked about the questions the women I was inviting to participate asked me, ones that inquired about my personal and family background. These questions were not only about helping them decide whether or not to participate. They were also about putting me within as thick a network of social relations as they can possibly accomplish within that time of getting to know more about me. Its purpose is that the more they can say about me to people who will ask them about my visit, the more able they are to show that there is nothing to be suspicious about. The more they can say about me, if and when they are asked, the more open they are about it. The more open they are means they are not hiding anything. Anonymity in the sense that others do not know they have participated in my research is not desirable or appropriate because it put the women at great risk of suspicion of doing something they should not be doing. [23]

3.2.1 The absence of husbands and the sensitivity of intimacy

Asking the women about intimacy particularly with their husbands or the absence of it was fertile ground for misinterpretation. The absence of their husbands so suffused their lives that questions about what they talked about during phone conversations or about what wives found difficult during their husbands' absence were almost always understood to allude to issues of sexual intimacy or its

absence. My generic question about what the women and their husbands talked about during phone conversations elicited giggles and laughter from interviewees. My interview question concerning the couples' topics of phone conversations was asked at the beginning of the interview and was intended to inquire into non-intimate aspects of their lives. My question concerning how wives dealt with missing their husbands was placed near the end of the interview for strategic reasons. Let me use my interview with Maricel, in her early 30s and who has two children, as example here.

> Researcher: When your husband calls, what do you usually talk about?
>
> Maricel: [Giggles]. The children, *manong*. He would ask about how we are. I do the same. Also ... [begins to laugh] the usual with couples ... "How are you?," he asks.[2] [24]

Although she tried to give a generic answer, when Maricel struggled as she alternated between giggling and suppressing her giggles, she inadvertently alluded to something more private. The interview with Maricel on this question lasted much longer than is indicated by the brevity of this extract. She eventually asked me if we could skip the question and come back to it when she had collected herself. [25]

How my question was framed or understood within absence and intimacy is also evident in my interview with Laura, a chief engineer's wife. Laura had given birth to her third child only ten days prior to the interview. She had told me that her husband was going back to sea in a few days' time. I asked her what she felt about her husband having to leave so soon after her having a new baby. Before this, she had talked about how her husband liked her cooking and how she shifted the focus of her attention from her children to her husband. Because of the birth, she had had very little chance to do either. This was what I had in mind when I asked the question but Laura understood it in terms of sexual intimacy. I asked follow-up questions according to my framing of my original question and Laura answered based on what she understood I was referring to. Here is the relevant part of the interview:

> Researcher: How is that then, of course, you've just given birth and he is soon to leave?
>
> Laura: Yes, that is true.
>
> R: How do you feel about it?
>
> Laura: Sad. It's because I wasn't able to give him my what, my responsibility to him because it is not really possible. That's why. I try instead to make him enjoy his time with the children. So he is at least happy before he goes. As he told me, "when I come back."
>
> R: You will make up?
>
> Laura: Yes. I said I will make up for it. No other way.

2 The interview with Maricel was conducted in Ilokano but for reasons of space, I use only the English translation. All interview extracts in this article are quoted in English. All names used are pseudonyms.

R: You owe him. Of course he understands it?

Laura: He understands. [26]

I realized when I transcribed the interview that a misunderstanding had occurred here. As I read Laura's responses, I became increasingly convinced she had taken my question to refer to sexual intimacy. What she said—that she "was not able to give him [her husband] her what, my responsibility to him because it is no really possible" and what she reported as her husband's response, "when I come back,"—clearly do not refer to her cooking but rather to her not being able to have sex with her husband. I wanted to make sure I had not forced or imposed this interpretation or realization so I went back to the recorded interview for clues such as tone of voice, pauses, and hesitations. Indeed this exercise supported my realization. I asked myself if I had not really referred to sex in my question or that I had really missed the whole point during the interview. What proof is there in the interview that I had genuinely missed the sexual reference in Laura's response? My simple answer is that I kept asking questions. Had I understood what she meant right from the start, I would not have asked the last two questions in the interview extract.[3] [27]

Asking the women how they dealt with the absence of their husbands was sensitive to them for at least two reasons. First, it was a topic that was too private and sensitive to talk about. To bring in or introduce the topic, I reminded them that they could choose not to answer it. Although the fact that the participants could choose not to answer any question they did not feel like answering was made clear in the participant information sheet and before the start of the interview, and that everything they were going to share would be confidential, in many instances I had difficulty transitioning to this topic. The easiest and most effective way I found was to say that what I was going to ask next was one they could refuse to answer. Second, it was sensitive, and this I would only realize in the course of my fieldwork, in that the question somehow put them in a defensive position and it did so because the question resonated with perceptions and suspicions of infidelity that surrounded them. And so when I asked what activities they did in order not to miss their husbands too much, women parried it immediately with a response that defended their honor and dignity: that they were not like other women who would seek to satisfy their sexual needs by having relations with another man. In turn, this put me on the defensive because I had no intention to even suggest infidelity and I was concerned not to offend them. Needless to say, I assured them I was not questioning their character. Aware that the matter was sensitive but only in the first sense discussed above especially in my early interviews, I always made sure that my tone was especially polite. [28]

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3 Although this interview extract, especially the last question, could potentially be seen to betray gendered assumptions about what I expect women to do for their husbands, I wish to make it clear here that I do not actually hold this expectation. The follow-up questions about Laura being unable to give her husband the attention she usually did when he was home were meant to get details of how she felt about it, as well as how her husband understood the situation. In other words, I wanted to know more about how both were adjusting to the further demands made on both of them by the birth of a new child, demands that did not enable Laura to shift the focus of her attention from her children to her husband, which she said is what she did when her husband was home.

This raises the issue of why I inquired about this matter aside from the fact that their husband's absence was obviously something the women had to face. I had been interested in finding out how the women rationalized their situation. More specifically, in one of my pilot interviews, a seaman's wife described her life as "*kasla agibaklay ti krus*" (like bearing a cross), a clear reference to Jesus Christ's suffering on the road to Calvary. This led me to think of this issue in terms of how they would have worked through the absence of physical and sexual intimacy within a discourse of sacrifice that I thought might potentially be informed by religious discourse. In other words, I wanted to explore whether this deprivation would be made sense of as a necessary part of providing for the lives and futures of their family (see GALAM, 2012). [29]

The sensitivity of the situation of the women, particularly their being cast as "vulnerable" or women who could be tempted and led astray presented a situation in which I, as a male researcher visiting and talking to them, potentially put them in a compromising position. Moreover, the topic of my research and the questions I was asking also necessitated careful negotiation of the dynamic involved particularly pertaining to issues of intimacy and sexual longing. In the section that follows, I reflect further on how the topic of conversation shapes the dynamic of the interaction between a (male) researcher and (female) participant. [30]

3.2.2 Testing the limits of what is ethical

Whereas I was hesitant to probe the issue of intimacy out of respect for the interviewees as it was not a topic of conversation that is talked about openly let alone with a stranger, and out of my own discomfort, I did not have the same hesitance or tentativeness in asking questions about the women's relationship with their mothers-in-law. When the subject was raised in relation to other aspects of the women's lives, I followed it up and kept asking questions to such a point I had to ask myself whether going further was still ethical. At least two women asked to be reassured before answering that what they were going to say would be confidential. In the split-second I had to decide whether or not to proceed further (albeit cautiously) with my probing, I had to weigh whether doing so already constituted gratuitous "voyeurism" or necessary for a "thick description" and understanding of how these women lived with and suffered from their mother-in-law. For the women who lived with their mother-in-law, their relationship with her was a major factor that shaped their agency and the power that they were able to exercise. Asking the women why their relationship with their mother-in-law was not good meant that the women had to confront emotional pain and fear (of the consequences of disclosure). The question also put a number of the women in an uneasy and awkward position as it involved talking about their mothers-in-law in their mothers-in-law's own house. Whereas I needed a "misunderstanding" between Laura and me (discussed in the previous section) in order to ask follow-up questions, my pursuit of the discussion of the conflicted relationship between the women participants and their mothers-in-law was done with dogged but careful persistence. [31]

My negotiation of fieldwork relations clearly shows that negotiating the cross-gender nature of the study did not have to be limited to dealing with the gender dynamic. Successfully negotiating linguistic, cultural, social, demographic (such as age) and spatial factors, which might also help attest to the good character of the researcher, is equally vital not only to researcher-participant relationships but also to the wider relational context (for example, how husbands, parents, or in-laws might react to or perceive the interview) within which wives, particularly, are located. Researchers, however, do not have the sort of power nor influence to enable them to control for the contingencies of human relations and interactions. As BRANDES (1992, p.38) has put it:

> "Our gender identity, like our age, marital status, and personality, will always in some way intervene. It is neither we as individuals nor the structure of the societies and cultures we study that alone determines the information available to us. Rather, an interaction of all these factors is what yields the final corpus of data. Under such inevitable circumstances it is comforting to know that whatever we learn is a lot, as long as we are honest about the conditions under which the learning took place ... the way we interact with our informants can be as filled with cultural information as their words and deeds." [32]

In negotiating fieldwork relations, I made decisions that have methodological and ethical implications. These decisions centered on not putting the women at risk of being suspected of infidelity and I discussed how this issue determined and dictated the place where I interviewed the women (see last two paragraphs of Section 3.2). I also discussed how the place of interview, aside from embodying an ethical obligation to the women, also existed in tension with holding on to a strict notion of anonymity shaped and defined in research practices and contexts of societies and cultures that are very different from those where I did my fieldwork. Farhana SULTANA (2007, p.374) has noted the differing conditions surrounding the implementation of "institutional ethics formalities" in the Global South and in the Global North and urges researchers conducting international fieldwork to bear in mind "negotiated ethics in the field" (p.375). The place of interview also left me with very little choice as to who else would be present or around. I do not doubt that the presence of others affected what the women shared or could share. Despite the presence of others, I made every effort to cover the issues that my research sought to examine. This is demonstrated in the part of the interview which dealt with the women's relationship with their mothers-in-law. In order to examine critically how the lives and experiences of the seafarer's wives were shaped by their relationships with their mothers-in-law, relationships that the women revealed to be fraught with difficulties, I needed to ask the wives questions that became more difficult to answer because of where the interview was taking place. Of course, the degree of following up I did took into serious account the presence of others especially the mothers-in-law who, despite not being physically within hearing distance, exercised a "haunting" presence. It was necessary to pursue the discussion because the issue spoke fundamentally to structures and relations of power and their consequences on the agency, subjectivity, and identity of the participants. Also, asking follow-up questions not only would clarify the issue but also, as a consequence, would

generate accounts that would serve as better bases for analyzing and interpreting structures and relations of power between the wife and her mother-in-law. [33]

3.3 Language as marker of class and educational hierarchy

In multilingual societies, fluency in one or more native languages in addition to the national language has its advantages. This becomes even more advantageous to the research in a country that has gone through colonialism, such as the Philippines, where a language becomes implicated in hierarchy. My pilot interviews emphasized how the use of Ilokano (the native language of the Ilocos Region and most of Northern Luzon) or Filipino would help minimize asymmetries of power relationships between researcher and participant. It flagged how English was a marker of social and class or socio-economic differences in the Philippines. My first pilot interviewee, for example, asked whether she had to speak in English; if she had to, she was not sure she had enough reserve of English. She worried in jest that the interview would not yet have finished but she would have run out of English. The language factor had the potential of putting her in what WETHERELL (1998) called "a troubled subject position." Her anxiety about the use of English was not only due to whether she spoke it fluently enough but also with what her English fluency level implied. In the Philippines, English is the language of government (together with Filipino), of the elite, of the educated. It is frequently a marker of class position and educational attainment. This was tellingly demonstrated when she said that she did not want to come off *boba* (thick). Her concern spotlighted not only the colonial history of this linguistic relation but also that it was freighted with a history of economic, social and class hierarchy. [34]

While it is true that I speak fluent English due to my having obtained university degrees, I share the socioeconomic background of many of my research participants. Nevertheless, I am aware that my education has enabled me to achieve more social mobility and that the participants might assume on the basis of it that I have a higher socio-economic standing. I felt that this might predispose them to behave towards me in a deferential way rather than occupy an equal position in the researcher-participant relationship; indeed, that they were doing me a big favor. I needed them for my research whereas they had absolutely no need of me. I tried to minimize how this might impact on the interview. Where interviews were conducted in the women's house, I removed my shoes or sandals and left them by the door as a gesture of respect. Also, I wanted to demonstrate through my actions that although I was at a personal level a stranger to them, culturally and socio-economically, I was not. Visitors, especially strangers, whom hosts perceive to occupy a higher socio-economic position would unlikely be asked to leave their footwear outside of the house. By voluntarily removing mine, I wanted to indicate a shared socio-economic background. I invested in these acts with these meanings hoping, by virtue of shared culture, that they would see them as such. I spoke in Ilokano, our native language, and assured them the interview would be conducted in the language they were most comfortable with. Where appropriate, I spoke only in Ilokano taking care not to mix in Filipino or English words. Where my interviewees mixed languages, I also did. Speaking in

a language and register that mirrored my participants' was another way by which I tried to address the socio-economic differences that my educational attainment might have implied or brought to relief. [35]

My fluency in two Philippine languages (Ilokano and Filipino), which all of my participants spoke, had two significant consequences on my research. First, it had epistemological, analytic, and representational implications for my research. Because I was interested in narratives and accounts of women's experiences of the migration of their seamen-husbands, I had to speak their language so that the disclosure of life narratives and their analysis would not suffer from language issues and the mediation of a translator. How women talked about their lives and themselves, the particular words, images and metaphors they used are significant to a description and conceptualization of their spatio-temporality. Hence, the importance of speaking the native language and the careful transcription of the interviews (see, for example, BECKER, 1999; BLOOR, FRANKLAND, THOMAS & ROBSON, 2001). Interview extracts quoted in the study had to be translated into English. A translation based on a paraphrased transcription (which in itself already constitutes a translation) of interviews risks losing the nuances of the women's representation and narration of their lives and experiences. Translation is a complex activity and in translating culturally loaded terms, concepts, and metaphors, the aporia of rendering them into English adequately is always present. Translation always involves interpretation so that what is presented as coming from the interviewee already carries traces of the researcher's or translator's mediation. In this sense, translation, and research more broadly, is not only invested with biographical and identity work (COFFEY, 1999) but also with interpretive frames and politics (GIORDANO, 2008; PAPASTERGIADIS, 2000; RAFAEL, 1988, 2005; RIESSMAN, 2008). [36]

The second consequence of my fluency in the women's native languages on my research is that it spoke directly to my position vis-a-vis the participants. My language competence went hand in hand with my cultural competence both of which significantly made it easier for me to negotiate fieldwork relations and the analysis and interpretation of what the participants shared. I examine further the relational construction of my positioning in the next section. [37]

3.4 Positionality and the insider/outsider dialectic

Positionality is a praxis of examining relationships and interactions that develop and unfold during the research and of making sense of insights and knowledge generated by such relationships and interactions. It might be taken to refer to "aspects of identity in terms of race, class, gender, caste, sexuality and other attributes that are markers of *relational* positions in society, rather than intrinsic qualities" (CHACKO, 2004, p.52). Because positionality arises from the negotiation of various relationships and interactions, it is "contingent and contested" (ibid.). Consequently, a researcher does not occupy a fixed and clearly defined position in her relation to the research and participants. [38]

The dichotomous construction of insiderness and outsiderness as privileged positions or standpoints of knowledge has received sustained interrogation, with Robert MERTON (1972) being one of the first (in sociology) to provide a systematic critique exposing its untenability. The oppositional construction of insiderness and outsiderness would make researchers "either objective and credible or subjective, biased, and generating questionable research outcomes" (SAVVIDES, AL-YOUSSEF, COLIN & GARRIDO, 2014, p.414). Qualitative social science researchers have nuanced the relationship between these two positions pointing to the reality of researchers occupying a spectrum or continuum of insider and outsider positions in relation to their research and interaction with participants (CARLING, ERDAL & EZZATI, 2014; COURT & ABBAS, 2013; DAWSON, 2010; ERGUN & ERDEMIR, 2010; HELLAWELL, 2006; MERRIAM et al., 2001; SAVVIDES et al., 2014). In this section, I look at this insider/outsider dynamic highlighting instances in my research and interactions with participants and others that became the vehicles for me to reflect on how I was both an insider and outsider. [39]

My interviews were supplemented by observations and my access to the cultural and social worlds of the women's lives, which I share because of shared ethno-linguistic identity. My sharing with them, for example, the native language also allowed me to grasp the nuances of their responses, as well as the cultural, social and religious allusions of the metaphors they used not only to talk about, but also to configure, their lives and experiences. Here, one example will suffice to illustrate this. One wife spoke of her family's financial situation as sometimes *nakibbutan* (emptied). *Nakibbutan* is a term used in *sungka*, a game played by two players whose goal is to accumulate as many shells from the competitor's reserves (49 for each player) distributed equally at the start of the game into seven pits. When one of the players loses all his/her shells, then s/he is *nakibbutan*. The road to *nakibbutan* is always preceded by one of the players having empty pits. Each pit must always contain seven shells or stones at the start of every round. If one player lost even just one shell to the competitor, s/he will have one empty pit called *puor*, fire (or gutted by fire). I found the wife's use of the idiom of a game to describe her family's situation, and by extension her efforts at financial management, suggestive of how she had to deal with outside forces over which she had very little control or influence and also of the ways by which she can distribute her "shells" in order to protect her house from being *nakibbutan* (emptied) and ultimately gutted by fire. [40]

I do not intend to overstate the advantages of my coming from San Gabriel nor do I wish to downplay the advantage and vantage point it has afforded me. Shared culture made it instinctive for me to negotiate interview appointments with women when they, particularly those with young children, would be most and least busy. This minimized the "inconvenience" of the interviews because they were plotted around their time. There obviously was not only a good time, but also a right time, to do things and this is socially and culturally determined (see, for example, PERTIERRA, 1997). I tried to make myself as least intrusive as possible, bearing in mind that they were doing me a big favor. [41]

In relation to my consideration of a "culturally-informed" temporality, the spatiality of my interviews was informed by a system of negotiating the world held by people who were assisting me. A number of middle-aged and older women advised me to be extra careful when interviewing in certain villages as, purportedly, many *mannamay* (roughly, witches) lived there. I was never to go there on Fridays as this was when they were most powerful and that if I went, I should bring with me a *pauli*, a sort of amulet or protection. This mattered to my research because one of the women who was to accompany me to a village to introduce me to the wives I wanted to interview there taught for a number of years in the village's elementary school and believed herself to have been a victim of a *mannamay*. There was no way she was going there on a Friday. This sociocultural system of apprehending and negotiating the world, of which, incidentally, women are the bearers and transmitters, was drawing me into an "imagined" community in which some members were seen as suspect. In other words, there were insiders who were at the same time treated as outsiders precisely because they threatened the well-being of others. The issue here is not so much that I do not believe in *mannamay* as that this system enforced a temporality and spatiality on my research built on the temporality of the potency of these *mannamay*. Although an outsider to this particular sociocultural system of apprehending the world built around these *mannamay*, I nonetheless temporarily operated within and through it, inasmuch as it guided the way those assisting me in my research navigated San Gabriel's physical, social, and spiritual environment. [42]

The way I negotiated my relationships with the participants, how I interacted with their husbands and relatives, and my analysis of the interviews and observational data made full use of my linguistic fluency and access to the history, culture and sensibilities of the people. As GUTMANN (2002, p.38) has written on this matter:

> "Interpreting and explaining cultural and individual feelings, attitudes, and practices is a dicey business that takes more than mere linguistic fluency in a language. Rather, it requires cultural fluency to be able to contextualize the words, inflections, and nuances, to distinguish kidding from kvetching." [43]

Yet my research in San Gabriel made clear to me the limits of my insiderness. In some ways, my fieldwork was the time for me to get to know San Gabriel more and my tricycle rides to villages I had never been to before provided me with a way to imagine and experience a small slice of the women's lives and their concerns. As one interviewee said, she and her children went to Sunday mass in the capital city, some 12 kilometers away from where she lived because the fare was cheaper than that to San Gabriel's Roman Catholic Church, less than five kilometers away. Because her village is not frequented by tricycles, she would have had to hire one for 60 pesos. But because the village is close to the national highway (motorway or autobahn), she can take a *jeepney* (a form of public transport) or a bus coming from other towns and that pass through San Gabriel and for which she would have had to pay only 30 pesos. [44]

While I had always thought that fare was determined by proximity or distance, this particular wife's experience showed otherwise. It also revealed that although she was closer to San Gabriel's *poblacion* (where the town center and Roman Catholic Church are located) than to the province's capital city in terms of physical distance, the *poblacion* was in fact less accessible. Where my interviewees lived helped me understand better their quotidian lives as it put into relief some of the daily concerns, like the availability or affordability of public transportation, which they had to contend with and that people who lived in the *poblacion* did not have to or simply took for granted. Where they lived consequently acquired much more importance to my research than I had initially recognized as it alerted me to such issues as geographic marginality. My prior ignorance of the impact of where women lived on their lives, especially transport mobility, which had financial implications, is not due to class or economic status. It has to do with where we lived: I lived in the town's *poblacion* ("urban" center where the seat of local government, the public market, and the Roman Catholic Church are located). Residence in the *poblacion* does not equate to a higher economic status. It almost always means proximity to government, commercial, educational (especially secondary), and religious institutions. The proximity of where I lived to these institutions meant that the experiences of some of the women related to their spatial distance from these institutions (and the activities they embodied) were alien to me. Even as a "native" of San Gabriel, I had and continue to have experiences that are, to borrow from GEERTZ (1973), near to and distant from those of others from San Gabriel. They have been important to my capacity for reflection, to my capacity to step back in order to see things more critically and to disabuse myself of common-sense assumptions. [45]

3.5 Narratives and the burden of representation

In linking the personal and the political, I relied on women's narratives of their experiences: their descriptions of their routine and quotidian lives; the practices that sustain their families and their ties with other significant people; the difficulties they face, endure and try to overcome; as well as the hopes and aspirations they have pinned on migration. These narratives have been profoundly insightful, enlightening and moving, and they have made it possible to give a richly textured account of the nature and experience of what it is to live and sustain a family in the context of the alternating absence and presence of their husbands and other social and economic conditions they have to deal with. [46]

These narratives, however, are limited and partial, and their telling motivated (ABU-LUGHOD, 1993). First, there were aspects of the women's experiences that simply were "unspeakable," that is, they were too private, intimate and sometimes painful to talk about. Second, they were the product of a specific interaction between a researcher and a participant. The reflections and narratives this interaction engendered were made at a specific point in the lives of the women thus providing perspectival accounts of their experiences. Third, the meaning and salience these narratives came to take no doubt was shaped by my own "partiality." How I made sense of them was informed by my own set of experiences, perspectives, biases and, not least of all, my position within the

research. Their narratives have therefore been framed by my own critical interpretation of their life projects. However, in analyzing the women's narratives, I endeavored to situate the women, their experiences and what they said in their material, familial, social, religious, cultural, historical and economic contexts. [47]

This attempt to understand them in context constituted the reflexivity that I hope always attended and informed my representation of these women. My account, indeed, narrative, of how these Filipino seamen's wives navigated their lives is based on these stories that they shared with me. The spatio-temporal dimensions I identify and discuss as critical to and constitutive of their gender identity, agency and subjectivity are based on these narratives supplemented by observations and my momentary occupation, sharing and inhabitation of some of their physical-social spaces. So is my examination of their negotiation of these spatial and temporal dimensions. Narratives were therefore central to my study in two ways. One, they constitute the empirical basis of my study. It is through how the women described their lives that I examined the spatio-temporality of their experiences. Narratives lend themselves to this undertaking for, as SOMERS (1994, p.616) has written, they are "constellations of *relationships* (connected parts) embedded in *time and space.*" Two, narratives were crucial to how the women made sense of their experiences, that is, they provided the women with a way to locate themselves in time and space. It was through narratives that they expressed how they understood and saw themselves in relation to their "past, present and imagined future" (HENRIQUES, HOLLWAY, URWIN, VENN & WALKERDINE, 1998, p.xiii). As SOMERS and GIBSON (1994, pp.58-59) have argued, "it is through narrativity that we come to know, understand, and make sense of the social world, and [...] that we constitute our social identities." [48]

Here, Mary STEEDLY's notion of "narrative experience" is useful "to evoke the ways that stories and experience are coproduced in everyday life" (FAIER, 2009, p.82). FAIER explains that for STEEDLY "stories do not simply express life experience; they are themselves lived out, structuring imaginations and assuming flesh and quotidian form" (FAIER, 2009, p.82). For STEEDLY, narratives are "part of the ongoing, dialogic, and constrained ways that people selectively make and remake the past as they craft lives and selves in the present" (FAIER, 2009, p.82). My own research would show that how the women experienced their present and saw themselves was profoundly informed by their consideration of their future. Their present was inextricably bound up with the horizon. [49]

In relation to this, I wrote my discussion of the accounts and narratives of these women in the "ethnographic past" (RUTHERFORD, 2003) to clearly indicate their partiality and particularity. This is not to say that they are no longer true and that the problems and issues no longer obtain in their lives. It is to point out that what was said represents a particular way in which the women's experiences were apprehended and represented linguistically and affectively, and the particularity of the way I have interpreted and conveyed them in the thesis, a way that can only be seen to demonstrate the "deeply perspectival nature of cultural knowledge" (PELETZ, 1996, p.35) and that, as James CLIFFORD (1986) has noted, all ethnographies can only present "partial truths" (see also FARMER, 2005). It also

registers the specific, that is, research, context in which these accounts were generated and by the specific group of people I interviewed: wives/mothers. My use of this ethnographic past is also in recognition of the certainty that as these women move on with their lives, they will be engaged in a process of reinterpreting their life narratives. What they shared with me was but one instance of this process, a process I had helped to put in motion. [50]

Finally, the narrative analysis I did looked at how these women's narratives might be linked to Philippine history and its economic and political conditions (see MISHLER, 1999; WHOOLEY, 2006). This was not to revert to a form of functionalism or determinism. My strategy took the narratives of these women's experiences of migration as useful frames for thinking about how the country's economic and social conditions, and the state policies obtaining from them, are reshaping the lives, desires, and aspirations of Filipinos. In other words, these narratives were crucial not only in these women's understanding of, and positioning of themselves within, the labor migration of their husbands. Equally critical, they were important for braiding the personal and the political, for enabling the possibility of looking at these women who are not the migrant workers themselves as central to migration. My narrative analysis moved from a thematic analysis of the personal stories of these women to an examination of how their stories might be linked to the social and economic condition of the Filipino nation. The analysis therefore sought to politicize, that is, make more political, their experiences (and not limit them as merely personal) since much of what they are going through is linked to, if not the direct consequences of, the limited social and economic possibilities in the Philippines. [51]

4. Conclusion

In the context of this discussion of representation, here I wish to address more explicitly some thoughts about studying one's community. Questions have been raised about doing home ethnography (BACA-ZINN, 1979; ZAVELLA, 1997) and the critical issue for me, following LOIZOS (1992), is that I am able to remain adequately aware of my position in the society and culture where I conducted my study as well as in the issues I investigated. This is not so much about being able to keep my objectivity as much as it is about being able to exercise what SCHEPER-HUGHES (2001, p.318) has called a "highly disciplined subjectivity" or what TURNER (1975, p.8) referred to as "an objective relation to one's own subjectivity." Of equal importance is the quality of the relationships I had with my participants (see NARAYAN, 1993) which influenced the quality of our interaction and their sharing of their stories. Nevertheless, I have always borne in mind that my interaction with the participants was based on "ethically and emotionally fraught relationships" (RUTHERFORD, 2003, p.xx), ones that were acutely temporally and spatially limited (see also PELETZ, 1996). [52]

One ethical issue of studying my own people or society, one that I grappled with most particularly after the fieldwork, is the sense of accountability I felt I had not only to the women and their families but also to San Gabriel. As ZAVELLA (1997, p.43) has written of this predicament: "[a]long with the cooperation engendered

by one's insider status comes the responsibility to construct analyses that are sympathetic to ethnic interests." Because many of the stories I had been privileged to hear were very sad, I thought about what sort of San Gabriel is going to be represented and what sort of stereotypes of the Philippines held by outsiders would this representation be taken to somehow affirm (cf. CRAPANZANO, 1985; SCHEPER-HUGHES, 2000, 2001). Although I was fully aware, and remain fully aware, of the non-generalizability of my findings and that readers would be critical enough to see this, the fact is that it led me to think more seriously about how I was going to use the data, about how I was going to write, about representation. BACA-ZINN (1979, p.218) has argued that such dilemmas "serve to remind us of our political responsibility and compel us to carry out research with ethical and intellectual integrity." [53]

The ethical obligation that underpins the question of representation I am highlighting here raises an ethical consideration that specifically concerns the research after the fieldwork has been conducted. Janet FINCH (1984, p.82) has pointed out that ethical considerations "tend to focus upon the point of access or of data collection rather than upon the use of the material." FINCH, as a feminist sociologist, was concerned about the potential for information given by women to be used "against the collective interests of women," a concern that resonates with that expressed by BOURGOIS (1995) and NADER (1972) on the poor. Although it might be argued that the issue of representation with which FINCH is concerned involves people other than the researcher and the participants (for this involves the reception and use by other people of such representation), the substantive point she makes concerning the focus of research ethical considerations is relevant here. My study flags the issue of how to represent, for example, other women, specifically mothers-in-law, who are implicated in the lives of the women I interviewed. How do we go about representing people who are part of other people's lives but whose own versions or accounts are not sought? I argue that providing socio-cultural and economic contexts to, for example, the sources of conflict between the daughters-in-law and the mothers-in-law would help minimize the injustice that might be committed against people who are not thereby sought to speak for themselves. What I have done, particularly in the context of conflicts between the wives and their mothers-in-law, was to examine the cultural and economic contexts of these conflicts. The dilemma I faced led me to take a more socio-historically and culturally contextual approach to my analyses, which helped me locate the women and their relationships within material and social practices. Although the result may be an unflattering picture of people, place, etc., I have provided a means by which they could be better appreciated or more fairly judged. In doing so, my analysis did not rely upon, and hopefully did not reproduce, prejudicial representations of mothers-in-law (see BROWN, 1997, 2004). In doing so, I think (or hope) that I have also positioned myself in relation to the women and to the data in ways that have not been prejudicial to them and made my research the better for it. [54]

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