A spy, a shill, a go-between[1], or a sociologist: unveiling the 'observer' in participant observer

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ABSTRACT Based on data gathered from two field research projects, the author examines two key questions that underlie her process of becoming a sociologist: (1) How did I negotiate my multiple identities in the field, and (2) What am I really, 'a spy, a shill, a go-between' or a sociologist? Drawing from Goffman's dramaturgical model, the author contends that much of the process of becoming a sociologist occurs within the shifting front and back regions in the field. Through a systematic examination of her 'personal' and 'methodological' field notes, the author captures these shifts from front to back and back to front, and attempts to elucidate the moral, ethical, and professional decisions that must be traversed along the way. Her aim, in other words, is to show how the identity negotiations that characterize 'doing fieldwork' are a key element of the process of becoming a (moral and ethical) sociologist.

KEYWORDS: *back stage, dramaturgy, ethics, field research, front stage, identity*

Introduction

Back stage excerpt from my field notes 1993:

> Phone call from Char [union organizer]. I started to take notes on this phone call but I stopped half way through. Something about our relationship crossing the line into friendship makes me uncomfortable taking notes. I also felt a twinge of this last night while talking to Sarah [another union organizer] but dismissed it and took notes anyway. It's not that I have a problem with keeping a record of what we talked about, it's the physical act of taking notes while she is talking to me – and her not knowing I'm taking notes and me not saying.

Front stage excerpt from my teaching notes 2001:

> In my office at the university talking with a graduate student about her participant-observation field study:

> **Graduate Student**: I feel a little strange taking notes on these people. I mean they're my friends and I believe in what they are doing.
>
> **Me:** Well you can be a part of their movement and still take notes.
>
> ***Graduate Student:*** Sometimes I just get so caught up I forget to take notes. Or, we are out all night doing an action and I don't have the time.
>
> ***Me:*** If you want to finish this (field study/thesis) you have to be a sociologist first. The only way to get through is to really see yourself as a sociologist – more than anything else.

What once resided solely in my field notes – my personal ethical dilemmas and debates – I am now required to reproduce as public knowledge (Edwards and Ribbens, 1998). As a thesis advisor to several graduate students conducting field research, I find myself in the difficult position of having to reveal the kinds of decisions one must make as a field researcher. Having completed two participant-observation field studies as a graduate student, I have pages and pages of field notes documenting my ethical and methodological journey alongside my observations and interpretations from the field. And while I feel personally comfortable negotiating the ethical quandaries of my own work in the field, it is quite another story to articulate those quandaries to a new generation of field researchers. To teach what I know about doing fieldwork means allowing a portion of my audience to see behind the scenes – offering them, in essence, a backstage pass to the everyday world of field research.

In my work and in this analysis, I use Goffman's (1959) dramaturgical metaphor to try and make sense out of the experience of doing fieldwork.[2] I find his analysis of 'regions' and 'region behaviour' especially instructive in sorting through the fragmented identities endemic to the work of participant-observation. According to Goffman, 'a region may be defined as any place that is bounded to some degree by barriers to perception' (1959: 106). Regions, in turn, contain both 'front' and 'back' stages – the former where the performance is given, and the latter where it is routinely contradicted. These constantly shifting regions are about place and identity. Some regions are defined and delimited by physical space. Other front and back regions arise in response to who is present or not present. And finally, within the confines of an individual performance, front and back regions develop and dissolve in response to shifting identities.

Applying this framework to the experience of fieldwork, I conceptualize the acts and actions I undertake as a 'participant' as a front stage performance, and my analysis of this performance [enacted in my field notes] as a back stage performance. It is my contention that much of the process of becoming a sociologist[3] – moving from a primary to a secondary identification with this identity – occurs within these shifting spaces in the field. Up to this point my backstage performance has had no audience – no one has been granted access to my field notes.[4]

My status as 'professor' now compels me to grant partial access to the back stage arena that characterizes 'doing fieldwork.' My status as qualitative

researcher necessitates that access be undertaken systematically. The article that follows is, therefore, based upon an inductive examination of my 'methodological' and 'personal' field notes. My reasons for pursuing this analysis are both practical, and at this stage in my career, morally necessary. Practically speaking, I need better answers for my students. As a qualitative researcher I know that good reflexive practice is endemic to the fieldwork process – data collection, analysis, and writing cannot proceed without reflexivity. In order to understand the structures that are generated through face-to-face interaction the researcher must become a part of those interactions. At the same time, she must be continually cognizant of the larger social structure of which that interaction is only a part. The methodological decisions that I must make in the field are made in reference to the larger structural (and political) context that frames my research. When I began teaching it came to me that, though I understand this connection, many of my students do not. The problem is that they grasp the methodological skills and techniques much more quickly than the theoretical framework that necessitates those skills and techniques. This analysis, in part, arises from my need to bridge this gap and offer my students clearer guidelines for their acts and actions in the field.

Morally, I need to feel certain that my performances in the field are guided by both professional and personal ethical standards. Two main questions guide this examination, (1) How did I negotiate my multiple identities in the field given the complex web of social relationships that characterized my field sites, and (2) What am I really, 'a spy, a shill, a go-between' or a sociologist?

After a brief discussion of both theory and method, my analysis addresses the two questions above. The negotiation process is unveiled through a close examination of several key 'situations' from my field experiences. The situations are laid out as acts in a play. Each 'scene' both builds upon the last, and moves deeper into the methodological plot. This analytic process parallels my own emergent understanding of my self as a field research. The question concerning my status as 'a spy, a shill, a go-between' or a sociologist, is really a question about the ethics of doing fieldwork. These terms, taken from Goffman's (1959) analysis of *The Presentation of Self in Everyday Life*, characterize the issues of loyalty, keeping secrets, confidentiality, and informed consent that confront the field researcher during her daily rounds.

The theory behind the method

In his chapter on 'The Methodological Position of Symbolic Interactionism,' Blumer (1969:1–60) instructs the sociologist to enter the everyday empirical world that is the focus of their study. In this essay Blumer attempts to disrupt the 'nonsensical' and 'profoundly misunder[stood]' position of symbolic interaction and naturalistic inquiry within the discipline of sociology (p. 49). He is responding to the, then current, positioning of established scientific

protocol and direct examination of the empirical world as opposites. Blumer decries the position of the 'detached outside observer,' and instead asserts '. . . if the scholar wishes to understand the actions of people it is necessary for him to see their objects as they see them' (p. 51). From a symbolic interactionist perspective, the social world is composed of individuals continuously creating and recreating meaning through interaction. Social structure, social organizations, and indeed social stratification, are all structures-in-process upheld through interaction. To 'discover' how social life is possible within any given structure, organization, or system of stratification the sociologist must become a part of that complex web of social interactions. Through systematic observation and careful analytic reflection, the sociologist – as both objective social scientist and subjective participant – is able to re-present the social world under study.

Adding a feminist methodological voice to Blumer's convictions means paying even closer attention to the 'subjective' dimension of this methodological equation. Initial calls for a feminist revolution in sociology framed issues of subjectivity in reference to the race, class, gender, and power dynamics of both the researcher and those being researched.[5] In their demand for a deeper exploration of the knower/known relationship, Stacey and Thorne (1985) called specifically for a feminist epistemology that attends equally to race, class, sexuality, and gender (pp. 311). Paying close (and systematic) attention to the researcher's own social location is, in other words, an essential component of the ethnographic process. In addition, other feminist social scientists have cautioned researchers to attend to problems of trust (Reinharz, 1992), personal involvement (Brettell, 1997; Letherby, 2000; Oakley, 1981; Stacey, 1996), reciprocity (Adler and Adler, 1991), reflexivity (Hurtado, 1996; Letherby, 2000), and the dangers of emotional involvement in the field (Kleinman, 1991; Lee-Treweek, 2000). Like Stacey (1996: 90), I took up my fieldwork armed both with these 'feminist principals' and the ethnographic methodologies of Blumer (1969), Humphreys (1970), Liebow (1967), and others. And, therein lies the ethical challenge.

As a social scientist my definition of various field situations may stand in opposition to my definition as subjective participant in those situations. In the opening quote from my field notes, for example, I am – at the same time – a union organizer, a field researcher, and a friend. From the vantage point of these identities, the possible lines of action available to me may head off into two (or more) different directions. A phone call from another union organizer initiates a 'situation' that requires a line of action to be taken. As a field researcher, conducting participant-observation research on childcare union organizers, I pick up a pen and start taking field notes on the phone conversation. As a friend to the person on the phone, I hesitate in my note taking. Ultimately I stop taking notes on this particular call and later I try to explain this decision in my personal notes: 'Something about our relationship crossing the line into friendship makes me uncomfortable taking notes.' My

decision to not take field notes in *this* situation privileged my identity as 'friend' over that of sociologist. In the phone call the night before, I made the opposite choice. The question is, is one choice more ethical than the other?

These situations – these moments of ethical debate and decision-making – are typically hidden from view. They are part of the interpretive process that shapes my self as sociologist. As the 'game' (Mead, 1934: 152–157) commences and the internal dialogue begins, I take into account each of my identities and those of any other persons present. In addition, my graduate-level field methods class, the American Sociological Association code of ethics, my close reading of Babbie (1992), Blumer (1969), Bordo (1994), Denzin (1978), DeVault (1999), Emerson (1983), Edwards and Ribbens (1998), Oakley (1981), Schatzman & Strauss (1973), and Stacey (1997) all coalesce into a 'generalized other' that asserts itself in these moments of interaction. The attitude of my 'generalized other,' which Mead (1934) defines as 'the attitude of the whole community' (p. 154), forms the basis for most of my actions.

All along I assume this generalized other is a feminist other, a moral other. I feel comfortable making decisions, choosing lines of action, and relentlessly pursuing meaning as it is revealed in the empirical worlds I study. And then, in the process of turning this private knowing into public knowledge, I am confronted with how this must look to the outsider – to the novice. If my students do not yet have a clear understanding of the theory (and politics) behind the method, then how are they making (ethical) sense of the methodological techniques I am teaching them? When I advise my students to be – first and foremost – sociologists in all their interactions in the field, what am I really asking them to be? What characterizes the occupational realm that I am inviting and encouraging them to join? And, am I following my own advice? The goal of this essay is to make visible – to unveil – this backstage region of my profession.

The method inspired by the theory

Data for this article were collected as part of two field research projects (Murray, 1988, 1995). In the first project I spent two years as a crisis line volunteer and relief staff at a battered women's shelter. In the second, I worked as an administrative assistant, cook, janitor, and union organizer at several childcare centres. In my research, conducted primarily in Northern California between 1985 and 1995, I used a combination of participant-observation field methods, in depth interviews, and survey methods.

As a participant observer I kept systematic and detailed field notes. Following the advice of Schatzman and Strauss (1973), I divided my note taking into four categories: observational notes, methodological notes, theoretical notes and personal notes. After a day or a night in the field I would return to my computer and spend almost as much time writing field notes. My

writing would switch back and forth between the four categories as my observations in the field reminded me of additional field sites I agreed to visit, of something Goffman wrote about, or I simply became aware that I needed some sleep. In the end, the observational and theoretical notes grew into dissertation chapters and journal articles (public knowledge), and the methodological and personal notes remained as private documentation of my struggles as both participant and observer. They remained, in other words, in the backstage arena of my work life – until now.

Act one: battered women's shelter intake

The Setting: Three of us from the shelter drive to a local motel to do an intake on a woman who has called on the crisis line. The two other women with me are long time shelter workers (it is 5.30 pm – shift change). Normally we do not go to where a woman is directly staying. We meet her in a public place to avoid endangering ourselves. But this woman is afraid she is under surveillance and does not have a car.

Front Stage: We knock on the motel door and a woman's voice asks us through the door who we are, then lets us in. Maria (staff) and Katie (the woman) sit on the bed. Pamela (the other worker) and I sit in chairs on either side of the bed. Katie asks us our names. We tell her and Maria explains, 'we all work in the shelter.' Maria then asks Katie to 'tell us about your situation.' Katie says that she is on SSI (Supplemental Security Income) and had been staying in transitional housing and that one of the counsellors in transitional housing had raped her. She had called the police and filed charges. Her social worker had set her up in the motel. Today – before calling the crisis line – she had received a life-threatening note from the man who had raped her. During the telling of this story both the other counsellors are nodding at Katie. At one point Maria puts her hand on Katie's arm. After about 20 minutes Maria interrupts Katie and says she has to go. The other counsellor Pamela says 'I'll walk you to your car.' I follow them out. Pamela says to Katie, 'I will be right back.' (Field notes 11/18/86)

Implicit to every interaction are front and back stage regions. There are several front stage performances being enacted during this 25 minute interaction. Maria, Pamela, and I are presented as 'all working in the shelter.' No status is given. We are presented as workers in seemingly interchangeable roles. The back stage to this presentation of status consistency is that Maria is a shelter manager, Pamela an intern, and I am a volunteer. We occupy very different positions of status and power within the organization, but present ourselves as status equals. In our performances we also exhibit a consistent message of sympathy (constantly nodding, emitting sympathetic 'ummhm's,' Maria's hand on Katie's arm), and belief. 'Belief' is performed through the intentional avoidance of certain behaviours: no furrowed brows, raised eyebrows, no questions asked with an inflection of skepticism, no probing inconsistencies. From an organizational perspective we are 'peer counsellors' operating under the feminist and political assumption that women tell the

truth about abuse. We serve women by believing them. Other social institutions have failed women by not believing them.

> **Back stage**: We walked down the length of the motel porch. Pamela asks Maria what she thinks about Katie's story. Maria says 'we definitely should let her into the shelter.' Pamela agrees nodding and then continues, 'there is something troubling about her story.' Maria, 'I agree, but we need to let her in. At least for tonight.' They both look at me. I nod in agreement . . .
> Ten minutes later: After hugging Katie goodbye and telling her we would pick her up at 7.00 pm, Pamela and I are back in the car on our way back to the shelter: I ask Pamela what she finds 'troubling' about Katie's story. She says, 'well, she says she is on SSI and women on SSI sometimes have more problems than we can handle.' 'What do you mean?' I press. 'It's not that I don't believe her, it's just that there are certain inconsistencies in her story. It is difficult to sort out stories like hers.' (Field notes 11/18/86)

The feminist stance of unwavering loyalty and belief is confronted with the exigencies of running an emergency shelter for battered women. In this situation, the team performance of the shelter workers is immediately – though carefully – contradicted upon arrival backstage. What is '*troubling*' about Katie's 'story' is not the violence she described, but instead the veracity of her statements. On the porch and in the car – away from the performance in the motel room – the shelter workers indicate some disbelief in Katie's story. While the front stage performance is not entirely contradicted, it is also not entirely over. In the car Pamela's performance continues with me (an agency volunteer) as the audience instead of the client. Though she never says that Katie is lying ('It's not that I don't believe her . . .'), she does draw upon symbols that suggest fabrication or deceit. The use of 'story,' the reference to 'inconsistencies,' and the necessity of 'sorting out' her words all indicate an underlying current of disbelief.

This telling shift from front stage to back region becomes one of those defining moments for me – one of those interactional planes where I find I must choose. I am a feminist. After coming of age during 'the second wave' of feminism and after 30 hours of domestic violence training I 'buy' the peer counsellor approach. I am a feminist sociologist acutely aware of the privileged interpretive power that I possess (Acker, Barry, and Esseveld, 1983), I am an agency volunteer who is fully cognizant of the fact that I may, someday, be left 'in charge' at the shelter. There are some women who – though survivors of domestic abuse – have personal needs that are beyond the scope of shelter services. I am a symbolic interactionist. I see the contradiction (the hypocrisy) engendered by this clash of perspectives. I see the emotional danger confronting me (Lee-Treweek, 2000). My response is to slide further into my sociological self. I don't have to live in the contradiction. My job (my moral escape hatch) is to observe interaction, to write about these moments of contradiction, and try to make sociological sense of this slice of social life.

My job, in other words, is to uncover the (power-laden) structures that compel the various social actors in this interactional situation to pursue specific

lines of action. Though we are 'peer' counsellors we are not peers in class, status, or situation. Though we are feminists, we are not equal in our access to privileges, resources, and power. We (the shelter workers) are women with homes and jobs, Katie is homeless and reliant on government benefits. And while the shelter began as a feminist collective, it has since been reorganized into a hierarchal agency run by a Board of Directors, an executive director, and the government agencies that fund it. Each of these strands of power form the larger context, within which, the daily decisions about 'who is let into the shelter' are made. I justify my escape from the face-to face dilemma (of deciding who gets to be safe) by viewing the dilemma as part of the larger social and political inequities that my work is attempting to address.

I am not, however, the only one making decisions about my performance. While I may choose among the various identities I occupy, others may also choose for me. Audience and performers are, of necessity, a matter of perspective. And, much of what occurs interactionally is far more complicated than any one actor's definition might take into account. What is front stage to one team of performers may also be front stage to their audience who is, in turn, giving her own performance.

> **Even further back stage:** At the Shelter 7.30 pm: Pamela arrives at the shelter with Katie. As she heads to the office at the back of the shelter Pamela asks me to show Katie to her room. Katie follows me down the hall and into a small bedroom with two beds. I indicate the bed near the door for her to put her stuff on. I ask her how she is doing. Before answering me she says, 'you won't tell anyone this, will you?' and gets up and closes the door. I say 'no,' and she proceeds to tell me a slightly different version of what happened to her starting with the fact that she is a lesbian and was raped by her 'lover' and not a counsellor. (Field notes 11/18/86)

Through the telling of *this* truth, Katie is trying to pull me on to her team. By revealing to me that she has, indeed, given a phoney performance she is attempting to remove me from the audience. From her perspective, the audience is both the presumably heterosexual and feminist shelter staff and the shelter organization itself. Her 'performance' was designed to meet the expectations of this audience. 'In every social establishment, there are official expectations as to what the participant owes the establishment' (Goffman, 1961: 304). In a battered women's shelter – governed by a feminist analysis of violence against women as rooted in patriarchy – heterosexual violence is the officially expected explanation for entry. From a feminist perspective male violence against women is understandable, is indeed expected; women's violence against other women is not. In telling a heterosexual story, Katie was giving the performance that she surmised would gain her entry to shelter. An assumption that proved correct. This is not to say that she wouldn't have been let in had she outed herself during the intake, she probably would have. However, the subsequent 'lines of action' chosen by the various participants might have been more difficult to 'fit' into existing feminist scripts.[6]

In this case, the back and front stage boundaries are delineated both by identity and by place. Katie assumed I was a lesbian. This assumption, in all likelihood, was based around my physical presentation at the time: short spiky haircut, jeans, boots, and black 'silence = death' tee shirt with the sleeves cut off. The 'back stage region' in this case was demarcated by an assumption of congruent sexualities and a closed door. As lesbians in a predominantly heterosexual world Katie and I (presumably) shared a world view different from other non-lesbian interactants. By outing herself to me Katie was calling upon that shared understanding. The creation of this type of identity-based backstage region is a common experience whenever gay and lesbian folks find themselves in 'situations' devoid of heterosexual people. When no straight people are present, gay and lesbian people routinely contradict front stage performances that assume heterosexuality. These 'contradictions' are really about connection; they are interactional routines designed to establish a lesbian and gay normative world view.

Katie's offer of a 'back stage' pass to her performance intensified my dilemma. After describing the above situation with Katie in my 'observational notes,' I made the following entry into my 'personal notes':

> **PN:** I just didn't know what to do at this point. Really I felt pulled in several directions all at once with my various identities competing for my attention. As a lesbian my response to Katie was one of sympathy. I felt a desire to protect one of 'my own.' As a shelter advocate 'in training' I didn't feel ready to take this upon myself, and I felt a certain loyalty and responsibility to the other workers. Finally, as a sociologist I was thrilled to be taken into confidence by one of the site participants. I am poised to record the 'underlife' of this organization. (Field notes 11/18/86)

The social and personal identities that surface in this fieldwork entry provide a rich illustration of the moral dimensions of the ever-shifting front and back regions. To tell or not to tell, forms the core of this dilemma. My 'participant' role as a shelter worker obliged me to 'tell' the other shelter workers the 'truth' about Katie's story. Part of this was about loyalty to that role, and part was about safety. Because Katie's batterer was a woman, the possibility existed that she [the batterer] could also gain access to the shelter and the other workers needed to know this. My front-stage identity as a shelter worker obligated me to tell. My back stage identity as a sociologist compelled me to keep my mouth shut. My interpretation of these events was circumscribed by my theoretical position as a symbolic interactionist and my methodological stance as an 'observer.' To meaningfully re-construct and re-present the social world that is – in part – the 'under life' of the battered women's shelter, I needed to gain the confidence of the women who stayed there. Ethically and morally these discrepant roles [participant and observer] put me into a rather untenable position. I was – in Goffman's (1959) characterization – on the brink of becoming a 'double-shill or go-between'[7]:

The go-between learns the secrets of each side and gives each side the true

impression that he will keep its secrets; but he tends to give each side the false impression that he is more loyal to it than to the other (p. 149).

Keeping secrets and impression management are necessary tools for the field researcher. Gaining the confidence of all participants in each research site is always the goal. Sometimes, however, the various participants in each setting are at odds with one another. As a sociologist my job is not to judge which side is 'correct' or 'morally wanting', instead, my job is to understand, contextualize, and re-construct the definitions that each side holds to be true. But when competing definitions have potential to cause harm, and I am the only participant privy to both world views, I am forced to make some choices. Guided by the code of ethics for my discipline (and good 'common sense'), I weighed the import of confidentiality against 'do no harm' and quickly surmised that the latter concern was paramount. In this case I was able to convince Katie to tell the truth about her situation to the shelter staff (partly by assuring her that I would stand by her through the whole process) and was saved from having to choose sides. I was able, in other words, to maintain my rapport with staff *and* gain entry to the everyday world of shelter residents. I was able to continue doing fieldwork.

In retrospect, Goffman's depiction of the 'go-between' does resonate with the experience of doing fieldwork.[8] In all of my interactions at the shelter I fostered a sense of confidentiality with whomever I was speaking. Like all the other shelter advocates, it was my job to give a 'true impression' that I could keep secrets. But unlike all the other shelter advocates, my ultimate loyalties were not with the shelter, with the shelter residents, or even with the battered women's movement. Though I chose violence against women as 'my cause,' and I chose this field site as my feminist trench, my final goal was 'to become' a sociologist. And while giving the not-quite-true impression of being loyal to all 'sides' of this arena, my ultimate loyalty lay with my chosen profession. It is this choice that provides the key to my moral absolution. I wanted to become a sociologist and one of the requirements 'of becoming' is to conduct original research. Like any job, the job of field researcher involves impression management. In this way, it was possible to conceptualize these ongoing identity dilemmas as simply 'doing my job.'

In my current analysis of these field experiences it occurred to me that my behaviour was, perhaps, even more in line with Goffman's characterization of a 'spy.' 'The individual who all along has meant to inform on the team and originally joins only for this purpose, is sometimes called a spy.' (Goffman, 1959: 145)

Act two: informing on the team

My work with the battered women's shelter ended in 1987 and my field notes from those months of participation were transformed into my Masters thesis. Though my intention was not 'to inform on the team,' in the end my analysis

did just that. As is the case with inductive research, the researcher never knows what she will find until she begins collecting and analysing data. My thesis, which was eventually published as a journal article (Murray, 1988), focused on the contradictions between feminist ideology and not-so-feminist practice. As a crisis line volunteer and shelter relief worker I underwent a fairly rigorous (and feminist) domestic violence training. As a part-time staff member I attended staff meetings, fund-raisers, and workshops. My analysis of these experiences led to the following findings:

> The shelter has two main goals, the first is to provide immediate and safe shelter for battered women who are in crisis situations. . . . The second and less tangible goal of the shelter is 'empowerment' of shelter residents. This goal, consistent with the feminist analysis of violence against women, is seen by staff as a need of shelter residents: the women must recognize their place within the cycle of violence and simultaneously recognize their own power to end this cycle if they so choose. (Murray, 1988: 78–9)

As a field researcher committed to 'discovering' the underlife of this social organization, I found competing goals among the shelter residents:

> In sharp contrast, shelter residents themselves say they have needs of a more practical sort: 'I need a place to live and a job.' If these needs are not met and a battered women has no money, family, or friends to turn to, then returning to the abusive situation seems to be the only viable alternative. Since most of the women who come into the shelter are in desperate financial circumstances, the priority of these needs become apparent. This poses a conflict between the ideological goals of the shelter and the reality of the resident's needs. (Murray, 1988: 79)

In the end – after detailing the basic social processes maintaining these (and other) contradictions – I conclude, 'Shelters, while necessary and worthwhile, are a response and not a solution to the problem of violence against women in our society' (Murray, 1988: 92). Though my research was not 'news' to the women who work in shelters, it did provide an open window into a feminist space that was then available for use by both feminists and non-feminists alike. To the extent my participation on the shelter team began and ended with my field research project, it is possible to conceptualize the goal of my membership as informing on the team.[9]

In the shelter I gained permission for conducting research by meeting with the director of the larger organization that housed the shelter. In my next field project, childcare, I resolved to gain entry by asking permission of 'the team.' Though I started at the top – applying for an administrative position at a childcare centre and interviewing with the director – once offered the job I made my acceptance contingent on receiving permission from the staff to conduct research on their profession. I attended a staff meeting and explained to the teachers that were present that I was applying for the administrative assistant job, in part, so I could conduct research for my PhD dissertation on childcare workers. I explained that I would eventually be asking each of them

to consent to be interviewed, and that I would be making daily observations of life at the centre. I even supplied them with copies of the journal article that my thesis had become so they could get a sense for the kind of research and analysis I had conducted in the past. I explain in full that my ultimate intention was to write a book about the working lives of childcare workers. I explained, in other words, that my intention was to 'inform on the team.' They agreed to participate, they were excited about my research, and their enthusiasm (momentarily) absolved me of any lingering doubts about my ethical status.

Act three: the tiny tot toddler center

The Setting: In January 1990 I began working and conducting research at the Tiny Tot Toddler Centre. The centre, serving one to two-and-a-half year olds, was located in a small coastal city in Northern California. My administrative assistant position at the Tiny Tot Toddler Centre fit well with my research agenda. The job put me in an ideal position (literally and figuratively) to observe workers. My desk at this centre was located in the office along with those of the two centre directors. This office also served as the break room for the teaching staff. On their breaks, many of the teachers would come to the office and talk about their experiences 'on the floor.' The office served, in other words, as a 'back stage region' for teachers. In the office, teachers were not constrained by the presence of the children. They spoke freely about the children, the children's parents, and other co-workers who were not present. It was a situation ripe for observation.

When I was sitting at my desk in the role of administrative assistant, I was privy to a constant flow of insider information. The teachers talked about the children and parents. The directors talked about the teachers. The directors would even talk about one another when one or the other of them was out of the room. Much of this information was confidential. A good portion of the talk was the back-stage bantering one would expect to hear from workers who spend most of their time on the front stage of the classroom. I did not remind workers that I was doing research each time they came into the office. I did not take notes on their conversations in front of them. I did, however, record many of these conversations in my field notes. As is evident from the following excerpt from my field notes, my status as a researcher did not inhibit those around me.

> **The Stage(s):** Friday afternoon: I am in the office getting book orders together. [The program director – PD] is at her desk working on her computer. Mary [head teacher in training] rushes into the office. She has something in her hand. She thrusts her hand under the PD's face and says (loudly) 'Oh my god, is this lice?' I turn to look just as the PD pulls her head back and grabs on to Mary's hand to push it away from her face. The PD says, 'Calm down and let me see. And keep your voice down.' Mary continues: 'This is so disgusting. I'm not surprised that they [the child's parents] were reported to CPS.' The PD looks at me and opens

her mouth to say something when Christina [an assistant teacher] opens the office door and walks in saying 'Is it lice? She has them all over her head. How gross.' The PD says [a bit sharply] 'I can't tell in this light' and walks out the office door into the yard [presumably to see in the sunlight]. Christina looks at Mary and says 'What is her trip.' Mary replies, 'I don't know, she seems to really want to protect this family or something.' Christina says, 'it just seems so unprofessional. They are terrible parents.' The PD walks back into the office. Mary and Christina stop talking. The PD says 'It looks like lice. I'll call her parents. You [Mary] take [the child] out in the back yard and keep her away from the other kids.' Mary and Christina leave the office. The PD sits at her desk. She sighs and says 'those two' and looks over at me. 'I can only imagine what they said while I was out of the room.' I say,' probably what you would expect them to say.' She says: 'They are so unprofessional.' I just kind of nod at her. (Field notes 5/6/90)

This scene – one of many I experienced during my tenure as an administrative assistant – really captures the kind of impression management endemic to centre-based childcare giving. The scene begins when Mary rushes into the office with a note of hysteria in her voice *'Oh my god, is this lice?'* Presumably, she did not respond to her initial discovery with this same emotion. Professional child caregivers are trained to respond to any kind of medical situation with calm reassurance. Mary's appearance backstage in the office enabled her to drop her professional 'front' and express her true emotional state. The program director, however, is still on front stage and she responds to Mary with the expected calm (though not quite as reassuring as one would be to a child). My presence does not register on the scene until Mary opens another backstage door: *'This is so disgusting. I'm not surprised that they [the child's parents] were reported to CPS.'* By virtue of my presence in the room Mary has crossed the line of confidentiality – though she does not recognize her own violation. Then when Christina enters the stage and the program director momentarily exits, the two teachers form a quick alliance. This alliance is established through a direct challenge to their boss's professional credibility: *Christina looks at Mary and says, 'What is her trip.' Mary replies, 'I don't know, she seems to really want to protect this family or something.' Christina says, 'it just seems so unprofessional. They are terrible parents.'* When the program director returns, the momentary coup dissolves as the teachers are sent back to work. I am then pulled back into the scene in order to re-establish the professional authority of the program director – to repair any damage done in her absence: *She sighs and says 'those two' and looks over at me. 'I can only imagine what they said while I was out of the room.' I say,' probably what you would expect them to say.' She says: 'They are so unprofessional.' I just kind of nod at her.*

Being able to capture this type of interplay is necessary for the study of human group life.

Human groups are seen as consisting of human beings who are engaging in action. The actions consist of the multitudinous activities that the individuals perform in their life as they encounter one another and as they deal with

the succession of situations confronting them. . . . The import of this simple and essentially redundant characterization is that fundamentally human groups or society *exists in action* and must be seen in terms of action This picture of human society as action must be the starting point (and the point of return) for any scheme that purports to treat and analyse human society empirically (Blumer, 1969: 6).

Capturing this type of interplay as a morally neutral and seemingly unobtrusive participant observer exacerbates one of the major dilemmas of the field researcher: the issue of informed consent. Both the teachers and the directors at Tiny Tot Toddler Centre knew I was doing research on child care workers. Before hiring me, the teachers and directors consented to being a part of my research. I told them that my research involved observation and interviews. I told them I would be taking field notes on my experiences and observations. I did not, however, tell them when exactly I would be observing or what 'taking field notes' meant, and nobody asked me about this. Thus I did not *completely* inform those I studied about all my research interests (e.g., Thorne, 1980).

Childcare workers are used to being observed. Their jobs involve routine and daily observation by parents, student teachers, co-workers, and administrators. By agreeing to be 'observed' did they really understand that I would be describing verbatim what I saw and what I heard? Did they understand that my observations of them extended beyond the classroom, beyond their interactions with the children in their care? Did they know that when they called me at home I often made notes of our conversations *while we were talking*? Did Mary and Christina and the program director realize that their interactions in the office would someday end up in a journal article?

On the one hand my response to these questions would be 'no.' Though I clearly explained my research intentions to all of the participants in each research site, I cannot know for certain how they interpreted my explanations. I do know that some of the workers forgot that I was doing research because they would act surprised when my status as a graduate student would come up in conversation. I never tried to hide or disguise my status as a researcher, but as I have illustrated, it was not always my master status. I talked openly about my ongoing analysis. I conducted in-depth interviews with workers at my research sites the entire time I was doing fieldwork. But none of the people whom I studied had previously participated in ethnographic research and none had been trained as sociologists.

The issue of informed consent posed an ongoing ethical dilemma that served as the final catalyst in my transformation from a primary to a secondary identification with my self as sociologist.[10] In order to continue 'doing sociology' within acceptable ethical boundaries, I scoured the work of other ethnographers (feminist and non-feminist) for practical advice on doing fieldwork. I followed their suggestions, I began to internalize their normative codes, and I continued with my fieldwork. I kept my field notes confidential,

and I used fictitious names in all my observations and interviews (cf. Liebow, 1967). I made up a list of aliases for each of the Tiny Tot Toddler Centre employees and kept this list secure and physically separate from my field notes (cf. Schatzman and Strauss, 1973). When using excerpts from my field notes in my writing I change names and other personal identifying characteristics (except gender) (cf. Liebow, 1993). When certain relationships in the field crossed over an indefinable line into friendship, I stopped taking notes on our personal conversations. It felt, at some point, dishonest to take notes on people I felt emotionally close to. If our conversations centred on a subject related to my research concerns, I talked directly with my friends about my 'observations' of their comments and we discussed them openly. In some ways, these friends became my informants in the field. With them, I more readily shared my observations, explored tentative hypotheses, and verified my interpretations of events at the centre (cf. Hendry, 1992; Oakley, 1981). Through these (and other) techniques I worked to meet my obligation to adhere to the practiced ethics of informed consent. In so doing – in meeting this and other ethical concerns and dilemmas – I was able ultimately to complete my transition and secure my identity as a sociologist.

The final curtain call: the sociologist takes a bow

Throughout this analysis I have employed Goffman's (1959) framework for understanding the organization of social life within the confines of a concrete social establishment. Ostensibly the establishments under examination have been – in order of appearance – a battered woman's shelter circa 1985–1987, and a childcare centre circa the 1990s. Each 'act' of this article documents the generic social processes that comprise social interactions that sustain each of these establishments. In this analysis I have relied on Goffman's (1959) metaphor of front and back stage regions to illustrate current applications of impression management techniques.

In reality, the *capricious* social establishment under examination is the everyday working world of the participant observer. Again the foci of analysis are the generic social processes that comprise social interaction. Similarly, Goffman's (1959) metaphor of front and back stage regions provides the field researcher with a map for understanding the field experience. The field researcher, like all other participants in the setting, engages in social interaction by 'fitting together lines of action.' Just what action is taken in any given situation depends upon the definition the researcher holds to be relevant to that particular situation. Definitions held, and therefore actions taken, shift and change in response to the participants that are present and the various statuses and identities they invoke. These 'shifts and changes' have been herein identified as transformations from front to back stage regions.

In his depiction of these transformations, Goffman also provides the field researcher with an ethical escape hatch from the quandaries of doing

fieldwork. At the end of his treatise, Goffman reveals to the reader that, in fact, all the world is not a stage. As he raises the theatrical curtain from his analysis he reveals his true focus on '. . .the structure of social encounters – the structure of those entities in social life that come into being whenever persons enter one another's immediate physical presence' (1959: 254). The field researcher, like all other *persons* who engage social encounters, is not engaged in deliberate deception, misrepresentation, or obfuscation of her intent, she is simply engaged in the maintenance of a particular definition 'in the face of a multitude of potential disruptions' (1959: 254).

The key, then, becomes my definition of what it means to be a sociologist. As a sociologist my job is to study social inequality and power. Broadly speaking, my intellectual goal is to identify structures of inequality, and my political goal is to dismantle them. My chosen methodology means that I study inequality at the level of face-to-face interaction. In so doing – by virtue of my own various social locations – I become a part of the very inequities I am researching. At times I may even end up perpetuating power inequities at the interactional level in order to continue my analysis at the structural level. In maintaining my particular definition it seems as though I have answered my own question: What am I really? I am a spy, a shill, a go-between, and (consequently) a sociologist.

NOTES

1. These terms are from, Goffman (1959) *The Presentation of Self in Everyday Life*. New York: Doubleday, pp. 145–51.
2. My use of Goffman's framework is not out of some undying loyalty to my sociological roots, or a dismissal of the last fifty years of sociology. Goffman's work – especially when applied to the profession of sociology – seems to parallel (and, perhaps foreshadow) postmodernist discussions of epistemology and partiality (cf. Bordo, 1994; Clifford and Marcus, 1986; DeVault, 1999; Okely and Callaway, 1992; Stacey, 1996).
3. For those of us whose primary method is participant-observation field research.
4. The exception to this is, of course, my observational notes. These notes – which include direct descriptions of the field, the interactants, the physical setting of various situations, and the conversations therein – have been extensively excerpted in various publications (cf. Murray, 1988, 1998, 2001).
5. For a more complete understanding of the feminist revolution in sociology see, Baca Zinn, 1979; Cannon, Higginbotham and Leung, 1988; Collins, 1986; Smith, 1979; Stacey and Thorne, 1985.
6. There is historical precedence for this fear on Katie's part, in the preface to the book, *Naming the Violence: Speaking Out About Lesbian Battering* (1986), the editor Lobel describes the first ever national gathering to raise awareness on domestic violence: In January 1979, the US Commission on Civil Rights held a national hearing on woman abuse in Washington DC, and out of that meeting the National Coalition on Domestic violence was formed. During the meetings forty lesbians met unofficially in an empty meeting room to discuss: homophobia in the battered women's movement and lesbian battering. Of the meeting Lobel writes:

The revelations about lesbian abuse by and of our partners was shocking and frightening. Although we could not reach consensus about the causes or dimensions of lesbian violence, we quickly agreed that this discussion could not be taken to the non-lesbian women in the movement. We felt it would destroy our credibility and that it would make us even more vulnerable to homophobic attacks by our sisters and those people in the dominant culture that wanted to discredit the efforts of the battered women's movement. So we pulled a tight lid down on the subject and didn't discuss lesbian battering even among ourselves until the NCADV conference two years later. (p. 12)

7. From the perspective of either side – shelter client or staff – I was, in all probability, seen as a 'shill.' 'A shill is someone who acts as though he were an ordinary member of the audience but is in fact in league with the performers'(Goffman, 1959: 146).

8. Though, of course, in Goffman's (1959) analysis he disparages the role of the 'go-between' as an individual while, at the same time, lauding it in reference to teamwork (p. 149). In the field, however, one of my teams – the professional sociological team – is invisible.

9. In retrospect, I think this is the moment where I began to see my sociological self as 'partially feminist' (Stacey, 1996), and partially not. While I was not comfortable 'informing on the team,' with my class-based analysis of resident interests verses staff interests, I was also not willing to stand down from my analysis. I agonized over my choice (to publish). The question that occurs to me now is whether this really was a feminist agony, or was it something else? As DeVault (1999) so insightfully comments:

    Concerns about the ethics of representation, and attempts to equalize interpretive authority, are central to feminist methodological innovations. However, I wonder if they also reflect an unwitting collusion with ideological constructions of 'woman' as especially moral or caring, or perhaps, a learned discomfort with authority that many women feel. (p. 189)

10. For an important corollary to this process, see Lemert's (1967) discussion of the shift from primary to secondary deviance.

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