

What Difference Does Difference Make? Position and Privilege in the Field

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Feminist standpoint theorists contend that the researcher's positionality affects all aspects of the research process—from the articulation of a research question to the analysis and presentation of the data. They argue that this influence becomes problematic when researchers occupying privileged positions in society elect to study those who are marginalized on the basis of race, class, and gender. In this article, we examine and compare the backstages of two distinct, cross-racial, ethnographic projects in order to understand how and to what extent the researcher's positionality shapes the structure and substance of the research study. We discover that the influence of racial privilege (and other components of researcher identity) on the research process is subtle and complex. It is apparent in the assumptions and narratives the researcher uses to make sense of her experiences in the field as well as in the relationship between the researcher and her respondents. We consider the implications of this in terms of the integrity of the ethnographies we produced, as well as for feminist research more generally.

KEY WORDS: feminist standpoint theory; reflexivity; identity politics; ethnography; master narrative.

Knowing is always a relation between the knower and the known.

Dorothy Smith, 1990

INTRODUCTION

The politics of identity and difference pose considerable challenges for the practice of sociological research. While feminist standpoint theorists have generated tremendous insights regarding the dynamics of identity and knowledge

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production, mainstream sociology has been slow to grapple with the theoretical and methodological implications of this work. Social theorists, for example, write about identity as if it were an obstacle to be struggled against rather than the object of political struggle (Calhoun 1994; Fraser 1997). Researchers have generally dealt with identity politics in one of two ways. In most cases, researchers omit considerations of identity from their discussions of study design and research methodology (e.g., Anderson 1999). In other instances, researchers briefly acknowledge crude aspects of their identities (such as race, class, and gender) without explicating how their data, analyses, and conclusions were shaped by their positionality (e.g., Sanchez Jankowski 1991; Venkatesh 2000). It may be that sociologists are hesitant to consider the effect of identity on their work because the groundedness of identity claims threatens to unearth those ghosts we are trained to ritualistically bury at the start of our research projects: bias and subjectivity. Concerns about the situatedness of the knower, the context of discovery, and the relation of the knower to the subjects of her inquiry are demons at the door of positivist science. The production of “legitimate” knowledge begins with slamming the door shut.

In this article, we take up the charge of feminist standpoint theorists and tackle the dynamic of identity and knowledge head on. We critically examine how the data and analyses that we each produced in the course of separate ethnographic projects were shaped by our relationships with the subjects of our research and, concomitantly, by the positions we occupied relative to them and within wider society. Our mission in this endeavor is to unveil what is routinely obscured in the practice of sociological research—the assumptions, motivations, narratives, and relations which are part of the researcher’s backstage. We examine the impact that this sort of legerdemain has not simply on the character of the data we collected, but on the kinds of analyses we produced. Subsequently, the first goal of this analysis is to provide a thorough, reflexive examination of the context of discovery and the ways in which identity politics are implicated in the knower’s relationship to the known. In revealing those aspects of the research process sociologists are often encouraged to conceal, we seek to do more than acknowledge the influence of our positions in the field of identity play (we are white, heterosexual, middle-class women). Instead, our analysis focuses on how and to what extent these positions affected the structure and substance of our research projects.

Further, because we embarked on research projects with women who occupied different positions than ourselves (McCorkel did a prison-based ethnography with women who were predominantly poor and African American, while Myers did an ethnography in a social change organization led by economically elite, African American women), we critically examine how “master narratives” (see Romero and Stewart 1999) regarding race, class, and gender shaped our efforts to make sense out of our personal and professional experiences in the field. Studies of dominant groups find that members conceptualize their experiences in the world through the use of narratives that reinforce the legitimacy of both their dominant social position and existing race, class, and gender arrangements (Delgado, Stefancic,

and Harris 2001; Frankenberg 1993; Sartwell 1998). Theorists like Smith (1987), Collins (1998), and Kelley (1997) contend that sociology itself has long been guilty of such practices. Collins (1998) and hooks (1984) extend this critique further by noting that even feminist sociology has proceeded in ways that distort, marginalize, and commodify the experiences of women of color. We wondered whether we were similarly influenced by master narratives when making sense of our experiences in the field and the degree to which these narratives found their way into our analytic interpretations of our respondents and their worlds. Subsequently, a second goal of this study is to investigate our own use of master narratives and consider the implications not only for our own studies but for feminist research and standpoint theory more generally.

THE CHARGE OF STANDPOINT EPISTEMOLOGY

Feminist standpoint theorists were not the first to challenge positivism's ontology and its epistemological assumptions regarding subjectivity and the production of scientific knowledge. Writing in the late 1800s, Dilthey called for social scientists to recognize that history and social life must be understood as the creative products of social actors. This distinction between the natural world and the social world mandated the use of social scientific methods, which emphasized *verstehen* and *erbelen*² (see Hesse 1980). Similarly, at their core, standpoint epistemologies share a focus on the critical connection between experience and knowledge claims. The integrity and uniqueness of standpoint epistemology is the emphasis placed on unmasking the contradictions inherent in positivist social science and in proposing an alternative approach for generating and legitimating knowledge claims about the social world.

Specifically, standpoint theories challenge positivism's dichotomy between "prescientific" (or nonscientific) thinking and scientific thinking.³ It is not simply prescientific thought that originates from the opinions and assumptions of materially, culturally, and historically situated individuals. Standpoint theorists argue that science and the "best" (i.e., most objective) scientific products are themselves socially situated (Hartsock 1983; Smith 1987; Collins 2000; Harding 1991). In other words, what passes as objective, neutral, and universal knowledge is not. To paraphrase Smith (1987; 1990), scientific research and products of that research—as all endeavors in our social world—are forms of social organization.

In her examination of Western science, Harding (1993) demonstrates that those who practice science are members of dominant groups (notably, white males drawn from the ranks of privileged classes) and that this membership serves to define what gets articulated as problematic. Not only do members of dominant

²In other words, experience as a means toward intuitive understanding.

³This dichotomy is made explicit in Durkheim's (1982) *Rules of the Sociological Method*.

groups cloak their concerns about the world in the language of science; but in the practice of “science,” they also advance an ideological apparatus that embeds *their* situated perspectives, concerns, and experiences into *our* cultural vocabularies. According to Smith (1990), this is more than an issue of sexism or racism. Rather, it is a fundamental privileging of elite men’s systems of thought such that knowledge emerging from their unique experiences and interests permeates our (lesbians, women, people of color) conceptions of the social world and influences how we interpret our lived experiences. Indeed, Romero and Stewart (1999) argue that stories we use to make sense of the world are often influenced by “master narratives” in which differences in power and privilege are regarded as inevitable, natural, and desirable. These master narratives originate within dominant groups but extend broadly throughout the society, shaping how members of both dominant and marginalized groups interpret and judge their experiences as well as the experiences of others.

Standpoint theory offers alternatives. There are significant divergences among standpoint theorists with respect to how feminists, social scientists, and others can produce less distorted claims about the world at large.⁴ Despite debates among standpoint theorists over the point at which lived experience is discursively constituted (and consequently, the value of using women’s experiences as the source of feminist truth claims), they maintain a fairly unambiguous set of standards for judging knowledge claims. Since standpoint epistemology asserts that all knowledge is socially situated, it requires researchers to specify the location and contexts in which their knowledge is produced. Romero and Stewart (1999, p. xiv) note, “Women’s stories cannot be fully comprehended without first considering the specific power structure (economic, political, and social) in which they are constructed and told.” An examination of such institutional contexts would allow audiences to ascertain which social locations produce the most objective and least distorted knowledge claims.

Harding’s (1991) distinction between strong and weak objectivity provides a clear illustration of how a feminist method would differentiate between better and worse claims. Positivism’s “weak” form of objectivity examines the context of justification (i.e., methods and procedures through which hypotheses are tested) and ignores the context of discovery (i.e., origin of scientific theories and the problematics that require explanation). “Strong” objectivity, on the other hand, is the acknowledgment that all cultures and societies maintain assumptions about the nature of the social world. Like other social institutions, scientific communities are permeated by these assumptions. Given their epistemology, standpoint theorists are less concerned by the presence of values than by their origins and implications. Subsequently, strong objectivity demands that researchers disclose both the context of justification and the context of discovery, particularly through an examination

⁴For expanded discussion of standpoint’s divergences and weaknesses, see Hekman (1997), Scott (1991), Harding (1998), Clough (1993), and Smith (1993).

of how background assumptions and beliefs grounded in the researcher's social location generate a problematic to be studied and a set of preliminary hypotheses (Harding 1991, p. 149).

Strong objectivity must necessarily be coupled with "strong reflexivity." Feminist methodologists like Fonow and Cook (1991), Hertz (1997), and DeVault (1995) argue that researchers should subject themselves to the same level of scrutiny they direct toward the subjects of their inquiry. Doing so requires not only that researchers present the results of their research to the affected community(ies), but also that the agent of knowledge be placed along the same critical plane as the object of inquiry. In this way, the researcher gazes back at her socially situated research project and examines the cultural assumptions that undergird and historically situate it (see Harding 1991, p. 163). For standpoint theorists, a dualistic emphasis on both the front and backstage of the research process is a crucial resource for obtaining objectivity and legitimating knowledge claims.

SPEAKING TRUTH TO POWER? RESEARCHER IDENTITY AND THE SLANT OF THE FIELD(WORK)

In her ethnographic study of a group home for incarcerated teen mothers, Haney (1996, p. 776) grappled with the implications of her race, class, gender, education, and age for how she viewed and was viewed by her informants. She concluded by noting,

In short, I never knew clearly how my "long line of adjectives" affected me in the field. My position was quite situational and variable . . . [Identity] changes with context, some contexts draw out certain aspects of our "selves" and mute others. Because of this flexibility I found it difficult to locate myself socially in my work. It was also nearly impossible to determine how these locations affected my analysis.

Haney is not alone in her inability to specify how the complexities of identity affected her relations and analysis. Similar dilemmas plague feminist and traditional ethnographers alike. In fact, longstanding debates among ethnographers over the desirability of "insider" and "outsider" status represent, to a large extent, efforts to understand how identity is implicated in the structure and process of fieldwork (see Adler and Adler 1987; Horowitz 1986; Kreiger 1985; Thorne 1983). This debate takes on added significance for feminist researchers working within the standpoint tradition because we are sensitized to the fact that women are differently implicated in the relations of ruling. As Collins (2000), hooks (1984), and Harding (1991) point out, white, heterosexual feminists have not only left the experiences of women of color and lesbians out of feminist scholarship, but their own class and status privileges often prevent them from seeing how this research often follows from racist, heterosexist, and classist assumptions. When such researchers do problematize inequalities, they risk commodifying people's pain to further their own careers (Collins 1998).

Generally, researchers have grappled with this dilemma by examining points of connection between the researcher and the researched. Connectedness, as a theme in the literature on reflexivity, is discursively mapped by detailing the investigator's commonalities and differences with research subjects. A number of ethnographers have questioned whether sharing one or two aspects of identity with research subjects confers insider or outsider status (Naples 1996; Reinhartz 1997; Tamale 1996). Arendell (1997), for example, struggled to determine whether her status as a divorced woman made her an insider or an outsider relative to the divorced men she studied. On the other hand, ethnographers like Abu-Lughod (1991) and Haney (1996) challenge the notion of a static insider/outsider dichotomy brokered by racial and gender identities. Instead, they suggest that the researcher's status as both an outsider and insider is constantly shifting as relationships are continually negotiated during fieldwork.

This debate has presented a number of problems for researchers concerned with eliminating or at least attenuating the power imbalance implicated in relationships with research subjects and meeting the standards of "strong" objectivity and reflexivity. A good deal of recent feminist scholarship has sought to document the ways in which aspects of the researcher's identity hinder understanding of differently situated respondents (El-Or 1992). The strategies developed to counter this are numerous but can be broadly divided into three types. The first strategy involves active attempts to level the playing field, usually by dismantling or suspending some aspect of the researcher's identity. Reissman's (2000, p. 115) study of childless women in South India is consistent with this strategy:

As an outsider—a white Western woman with grown children, studying South Asian women who wanted to conceive—I hoped to give voice to their invisible concerns in a country dominated by a discourse of population control. The subcontinent of India and its southern coast (Kerala), where I lived and worked from 1993 to 1994, dissolved distance . . . I lived in a fishing village and assisted in wedding preparations, working alongside childless women I had formally interviewed. Witnessing them in everyday life decentered my earlier notions of their subordination.

The second strategy involves reorienting the focus of the inquiry in order to "study up." This strategy limits researchers to examining only those respondents who occupy similar or more powerful positions in society (see, for example, Sparkes 2000; Kurzman 1991). A third strategy involves the examination of how aspects of identity can both impede and facilitate different forms of understanding (see Creef 2000). In this case, researchers identify limits to how far their understanding can go based on their inability or unwillingness to alter certain aspects of their identities (see El-Or 1992; Van Maanen 1988).

Serious criticisms have been leveled against each of these strategies. Reflexive attempts to locate oneself as researcher have been criticized as "rhetorical maneuvers" that are facile and ineffective (Benmayor 1991, p. 159). Similarly, Wasserfall (1993) and Smith (1990) critique ethnographers' efforts to deconstruct power differences in the field, arguing that such efforts often skirt the political

issues that organize the process of knowledge production and consumption. Efforts to “study up” are not always practical; additionally, the locations of “up” and “down” are neither obvious nor static, and the categories themselves represent a value judgment. The drawback of the third strategy, acknowledging the limits of one’s knowledge, is linked to the requisite language and form of social science. DeVault (1997, p. 225) and Mykhalovsky (1996) argue that the *style* of reflexive writing affects how it is seen by critics—very personal writing is often rejected as self-indulgent and narcissistic.

It is our contention that standpoint’s standards for “strong” objectivity and reflexivity hold the key for overcoming some of the disadvantages associated with a slanted playing field. Understanding how power and privilege shape social science research requires an analysis of the context of discovery and, therefore, an identification of the researcher’s positionality. This alone, however, is not enough. Smith (1990) argues that acknowledging one’s structural location neither dissolves power differences nor necessarily suspends the researcher’s self-interested assumptions about herself, her project, or her research subjects. Strong objectivity must coincide with strong reflexivity and this requires the researcher to subject herself to the same level of scrutiny as she directs to her respondents. Taken as a whole, standpoint epistemology requires that the researcher put her taken-for-granted assumptions, beliefs, and stereotypes on the table for dissection. It requires an analysis of how her own use of master narratives give form and substance to not just her experiences in the field, but her sense of her own identity as well as the identities and “differences” of others. It requires the researcher to consider how she reproduces her own privilege through the analyses she produces. It is only through such candid examinations of the researcher’s backstage that the implications of identity and difference on the research process can begin to be explored.

In the sections that follow, we undertake just such an analysis. In so doing, we offer a more nuanced delineation of the power dimensions of the researcher/researched relationship and, through the use of strong objectivity and reflexivity, consider the consequences for truth claims. We do not suggest that this type of analysis levels the playing field between ourselves and our research subjects. Rather, we strive to demystify the rules and strategies by which the game gets played.

RESEARCH DESIGN AND FIELD SETTINGS

The idea for the present study emerged during a conversation between the two of us about our experiences on the academic job market and the reactions we encountered while hocking our post-dissertation wares. Although our empirical work shares few similarities beyond the use of qualitative methods and an analytical focus on social inequality, we found that we routinely encountered a similar set of questions about race (“What effect did your race have on the study?”) and,

more specifically, our whiteness (“How did your whiteness affect relations with respondents?”). We were not asked about other elements of our identities, nor did other aspects of our projects appear to generate the same level of interest. Interestingly, it was this single, observable difference between our respondents and ourselves that seemed to define the projects in the eyes of our public.

At first, we handled the race effects question using many of the same discursive strategies that appear in the literature—with a nod to our privileged position as white academics; a shrug regarding the influence of our positionality on our relationships with respondents and our analyses of their worlds. Our answers were not intended to be evasive; they were, for better or worse, reflective of the fact that we had not subjected ourselves to the same level of analytical scrutiny that we applied to our research subjects. This article represents our effort to do just that. Here we answer, directly, questions regarding the ways in which social privilege affects the process of knowledge production. The data for this study are the fieldnotes and personal diaries we kept during the duration of our projects, as well as recorded interviews and conversations with respondents. We discuss each project below.

McCorkel’s project was an ethnography of a drug treatment program (“Project Rehabilitate Women”) housed within the walls of a medium-security prison for women.⁵ The program was intended to target repeat drug offenders, though most of the women in the program had a much more diversified offense history, ranging from petty theft to attempted murder. The initial research project took over four years (from January 1994 through July 1998) to complete. During that period, the program admitted a total of 264 women. Seventy-seven percent of the women are African American, 17 percent are white, and 6 percent are Latina. The median age of the women is 30 years, with 50 percent of the study participants ranging between the ages of 19 and 30. Prior to their incarceration, the vast majority of women in the program lived in poverty or near-poverty conditions. According to case records compiled by the program, 91 percent are the survivors of sexual and/or physical abuse. In total, McCorkel conducted semistructured interviews with 74 inmates and 29 employees of the prison and the state’s criminal justice system (including administrators, judges, politicians, correctional officers, counselors, and directors of funding agencies). In addition to transcribed interviews, data for the present study include fieldnotes that report on hundreds of conversations with just over three-quarters of the 264 women who were incarcerated during the study period and hundreds of hours of participant observation.⁶ For an overview of the original study, see McCorkel (1998a).

Myers’ project concerned a voluntary organization of wealthy African American and white women, Women Against Violence (WAV), geared toward the

⁵Pseudonyms are used in place of identifying information.

⁶McCorkel generally spent three days a week in the facility for eight- to ten-hour periods per day. During the summers, she spent an average of 40 hours per week in the prison.

eradication of violence and its causes. Her analysis of this organization focused primarily on the intersections of race, class, and gender—on the contradictions between power and disadvantage (see Myers 1996). Myers collected data from 1992 through 1996, through archival data collection, participant observation,⁷ and in-depth interviews. She spent about 150 hours in the field, conducting 14 in-depth interviews with board and staff members and six interviews with leaders of related community organizations. WAV's board consisted of 25 women (50 percent African American and 50 percent white), and they had about 250 general members. WAV's leadership was primarily wealthy, but a growing contingent of WAVers were younger and middle class. During the study, WAV's African American board members were lawyers, artists, professors, professional volunteers, and administrators. WAV's white board members were accountants, administrative assistants, managers, and administrators. All women ranged in age from mid-30s to early 90s. WAV's major activity was to provide financial aid to the indigent.

Although they are markedly distinct, a comparative analysis of our studies provides a unique vantage point from which to assess the meanings of race and of privilege in different institutional contexts and across variegated socioeconomic lines. Although the most common reaction to each of our projects involved assumptions that racial difference was a barrier to participation, rapport, and trust, race was far from a settled project in either of these institutional contexts. A comparative analysis of this kind allows us to explore identity politics in two very different organizations—one a total institution characterized by surveillance and repression, and the other a volunteer organization notable for its wealthy, racially diverse leadership. We find that aspects of our identity beyond race took on considerable importance not only in our relations with respondents, but in influencing what we regarded as noteworthy in the setting and the interpretations we ascribed to various events and exchanges. This study allows us to examine how multiple dimensions of the researcher's identity take shape in relations with her respondents and influence the researcher's perceptions of and access to power and privilege in the setting.

We have divided the analysis into three sections. Each section roughly corresponds to the requisite components standpoint theorists suggest are necessary for achieving strong objectivity and strong reflexivity. In the first section, we examine the context of discovery by situating ourselves as agents of knowledge. In it, we detail the motivations that gave rise to our research projects, the assumptions we maintained about our respondents early on, and the narratives that were available to us as we struggled to make sense of our encounters in the field. In the second section, we expand considerations of the context of discovery from the biographical to the institutional. Here, we are concerned with how institutional politics and organizational dynamics shaped the meaning of identity and difference in each

⁷First as a volunteer in the office and later as a board member.

of the settings. As part of this, we critically examine our relationships with the subjects of our research and consider the implications of these relationships for our actions in the setting. In the final section, we assess how each of these elements (the situated knower, the context of discovery, and the relationship between the knower and the known) influenced the structure and substance of our studies. We explore how privilege and self-interest are implicated in the production of knowledge—shaping what we chose to write about, whom we shared our work with, and whose voices we silenced.

SITUATING THE KNOWERS: BIOGRAPHICAL CONSIDERATIONS

One of the most serious critiques leveled against the reflexive efforts of feminist social scientists is that such efforts do not decenter the researcher's privilege and thereby fail to provide a meaningful account of how power organizes knowledge production (Wasserfall 1993). In our review, this is most often the case when the researcher reveals select aspects of her identity and biography but does not connect such information with a larger analysis of how her positionality shapes (and is shaped by) the power arrangements within specific institutional contexts. In this section, we explore our biographies in an effort to situate ourselves as agents of knowledge. Doing so is a crucial element of strong reflexivity and objectivity in that it reveals how hidden (and not so hidden) assumptions and master narratives shape the problematic to be studied and the direction of the researcher's gaze. Here, we discuss the personal motivations that gave rise to each study, how we formulated a "problem" to be explored, and our initial assumptions and expectations regarding our respondents. At the same time, we recognize that this does not go far enough analytically; we will link the biographical and the institutional in the section that follows.

Project Rehabilitate Women

I would like to suggest that my research began with a set of concerns and questions about women's lives and the criminal justice system. Unfortunately, this was not really the case. Although I had more than a passing interest in gender inequality within the criminal justice system, my foray into East State Prison and the PRW drug treatment program was driven initially by the pragmatics of graduate student survival. I was 23 years old and had just completed my first year of graduate school when I was hired by a well-respected criminologist to work as his research assistant on an evaluation study of the PRW program. At the time, I had no desire to pursue my own research agenda in the setting. The criminologist granted me considerable autonomy to organize the qualitative component of the evaluation and reviewed my quarterly reports but little else. In fact, he sent me into the prison

with little more than a few articles on process evaluation and ethnography, and a warning: “Prisons—even women’s—can be a tough place. Watch your back.”

The circumstances I found myself in that summer and over the years that followed could not have been more different than the circumstances of the women I was sent to interview. I was, of course, a free person in terms of the criminal justice system, but I also enjoyed a freedom associated with the unregulated character of my research job that even the prison’s correctional officers and counseling staff did not possess. Beyond this, I was single and my schedule was unencumbered by the demands of family. For a long time, I was fairly oblivious to the privileges and respectability my graduate student status and the promise of upward mobility afforded within the institution. My inability to critically assess my own situation was particularly egregious given that I was the only person save the warden who entered and exited the prison with relative abandon.⁸

Despite my initial naiveté, I was critical of master narratives that cast incarcerated women as dangerous (a characterization that was implicit in the criminologist’s warning and consistent with the rhetoric of the state’s war on drugs). I was suspicious of this sort of representation for three reasons. First, both my training as a sociologist and my leftist political leanings made me suspicious of discursive schemes that served to legitimate the institutional order. In this case, constructions of women as “dangerous,” “violence-prone” and “hard-core criminals” emerged during a crisis in which the state had enacted mandatory sentencing guidelines for drug offenders and the prison was grossly overcrowded and severely underfunded. Second, I regarded “danger” and “violence” essentially as gendered phenomena—conditions that were enacted almost exclusively by men. I envisioned men’s prisons as the embodiment of a Hobbesian state of nature. When I thought of women’s prisons, on the other hand, I conjured images of sewing factories, pseudo families, and passivity. These images were consistent with what little literature existed on women’s prisons and while it did not take me long to discover that life in PRW differed markedly from those earlier accounts, I still had a difficult time regarding women inmates as dangerous or even particularly rebellious. The third reason I rejected the construction of the women as dangerous was based on my early interactions with them. Most were anxious to share a cigarette, exchange prison gossip, and show me photographs of friends and loved ones. Many knitted blankets, watched talk shows, read romance novels, and wrote letters to children in their spare time. There was not much traffic in shivs (i.e., homemade knives), fighting was infrequent, stabbings were rare, and riots were nonexistent. All in all, I regarded the setting and its residents as fairly banal.

My fieldnotes during the first three months of the evaluation reveal a preoccupation with the similarities that existed between the inmates (particularly those who were white and working class) and myself. I had grown up in predominantly

⁸I was awarded a high-level security clearance from the state department of corrections. This meant I was free to enter and exit any of the state’s prisons at any time on any day of the week.

white, working- to lower-middle-class neighborhoods and had committed various acts of delinquency through my teen years. My early delinquency, trouble with school authorities, and involvement with law enforcement was similar to the experiences of a number of the women in PRW. In fact, I attended the same high school as a woman doing time for murder (she was three years younger than me) and I was familiar with the social networks of two other women (one white, one African American) in the program. By finding and focusing on the handful of similarities in the early lives of some of my respondents, I ignored crucial differences and the very real impact of race, class, and gender on all of our lives. This contributed to my own flawed, partial, and biased assumptions about incarcerated women. These assumptions are revealed in a fieldnote description of a conversation between myself and Judy (24 years old, white, mother of two) during my third month in PRW:

The talk turned to drugs, different kinds and the different highs. We were joking, I was telling her a story about getting paranoid on cocaine and she looked at me really seriously. She asked if I had used a lot of drugs. I told her I had experimented here and there but never got into it. Then she said, "So if you were doing all these drugs and into all this trouble, how come you're not in here?" I kind of stuttered because it hadn't really occurred to me before and I said, "I don't know, I guess I don't like losing control." She got this disgusted look on her face and said, "So what are you saying, that I'm, that *we're* out of control?" I denied meaning that but from the look on her face I wasn't terribly convincing. I don't know what I meant . . .

Characterizing the women as out of control allowed me to preserve the illusion that we all faced similar disadvantages early on. It meant that I could regard my early success in graduate school (and, concomitantly, my avoidance of prison) as the product of hard work and strong character rather than a byproduct of race, class, and other distinct advantages (e.g., I was not the victim of physical or sexual violence, etc.). It meant that in the course of our relationship I deserved to be the knower and they deserved to be the known.

The assumptions that I harbored about the women incarcerated in PRW and my criticisms of the institution's construction of them as dangerous led directly into the formulation of a "problem" to be studied. My original research question asked why women inmates did not resist the conditions of their confinement. The women, of course, did resist, challenge, and subvert the conditions of their confinement (see McCorkel 1998b). My failure to recognize this was based, in part, on a romanticized (and sexist) vision of men's prison violence and "dangerousness" as a form of resistance. In PRW, the women were rarely violent and when they were it was largely self-inflicted. I read this as passivity. This interpretation was fueled directly by my assumption that the women's preprison lives were "out of control" and the path to prison beaten by feet leaden with drug addiction. This was a direct product of my positionality—a positionality that was comprised of at least two dimensions. The first was fictive in the sense that I overemphasized the similarities between my experiences with drugs, delinquency, and trouble and those of my respondents. The second dimension of my positionality was the material

location that I occupied. I am white, upwardly mobile, highly educated, and free from physical and sexual violence. Even my criminal record has been expunged. Denying the second dimension while romanticizing the first clouded my vision not only of myself but of my respondents. Through my partial, distorted gaze, we were the same but different and *their* normatively marked difference became the stuff of my early research agenda.

Women Against Violence

Like McCorkel, my entrée into the field was characterized by naiveté. As a neophyte researcher, I entered the field expecting that my sociological training would pave the way for a nuanced analysis of WAV. I fancied my proper credentials and skills to be sufficient for gaining access to and staying in the field for an extended period of time. What I did not anticipate is the role that my personal biography would play in facilitating my research. Ironically, I had learned master narratives about race, class, and gender through my personal experiences that affected the quality of the connection between me and my subjects. These master narratives were as important as my formal education to the research process. To complicate matters further, my “situated knowledge” (Collins 2000) often conflicted with my sociological knowledge.

For example, as a graduate student, my understanding of race was profoundly shaped by black feminist and anti-racist literature (e.g., Collins 2000; hooks 1984, 1994). However, my personal experiences with racism as a child born and raised in the South in the late 1960s and 1970s also informed my conceptualization of race. I was among the first generation in the South to attend integrated schools, yet I witnessed none of the tensions evident in many other Southern towns. As a first grader, my best and only friends were black classmates, Lisa and Freddie. But in the second grade we were academically re-tracked, and I was no longer in their class. By the fourth grade, there was only one black kid in my class. I hung around with other white girls. I never made a conscious decision to do this; it just seemed natural. Despite all of the official efforts at racial integration, a *de facto* racist legacy prevailed in my community. Although ambiguous in its effect, segregation alters our perceptions and helps create an “other” (Fine and Weis 1998) who is kept at a distance—both literally and figuratively. Further, segregation insulates dominants from input and critique by people of color about the ways that we (mis)conceptualize race. Segregation may enable even critical whites the luxury of critiquing racist structure without really dissecting the ways that we internalize and promote master narratives about race. Thus, although an intellectual anti-racist, I lived a life largely sheltered from daily confrontations with my own internalized notions of the ways that people organize by race.

My understandings about gender were more deliberately constructed. I recall overt, often painful lessons about how to be a “proper” [white] girl in the South.

I was taught how to offer unsolicited compliments; laugh politely at jokes; avoid taking political stances; and, above all, avoid “tacky” behavior. I often failed publicly, due to my “coarse” sense of humor and “inappropriate” outspokenness. But I did try to be a lady. I went to a private, predominantly white, Southern Baptist women’s college, and my education in ladyhood hit the fast track. My peers came from privileged families who had trained them through cotillion, horseback riding, trips to Europe, and formal parties. I learned which hairstyles were appropriate for “girls our age”; when to wear white shoes; and how to word invitations and RSVP cards. Although I did not have the economic background of most of my peers, I absorbed these “dos” and “don’ts” in the hopes of avoiding venomous criticism. Even today, I am conscious of the textures and tones of the clothing I wear, and I carefully monitor my behavior in social gatherings.

When I went to graduate school and became a feminist sociologist, I foolishly thought that I could jettison the baggage of my past. I underestimated the power of the master narratives to structure perceptions and interpretations, despite learning more critical perspectives. I also expected the people whom I studied not to be constrained by these larger cultural pressures. Therefore, when I began studying WAV, I expected to find a revolutionary organizational dynamic that allowed these women to transcend racial divisions in the interest of their goals. I anticipated discovering a utopian, color-blind sisterhood. I approached the site with high expectations. What I found was disappointing at first. WAVers had indeed united in spite of color. But they did not unite as activists (a bad word), as feminists (a worse word), or even as sisters: they united as Southern ladies (Myers, forthcoming). My research conjured many ghosts. I often had to work through my disappointment with WAVers: these were ladies as I was raised to be; not feminists, as I had become. Race intertwined with class and gender in this organization, making the negotiation of my different statuses difficult. However, the complexities eventually forced me to contend with the fact that many of my own assumptions remained intact, despite all of my training. Indeed, as I discuss below, I drew upon many of these old ideas to assist in my fieldwork.

GROUNDING THE CONTEXT OF DISCOVERY: INSTITUTIONAL CONSIDERATIONS

In the cramped quarters that the data and methods section occupies in most articles, ethnographers and others often do not have the space to elaborate on how their positionality was shaped by the institutions in which they did their work. This is an unfortunate omission because the context of discovery is shaped by personal biography as well as social structure. In situating ourselves as agents of knowledge, researchers refer to abstract categories like race, class, and gender, which serve as proxies for more complex and precarious elaborations of identity, relationships, and power. The meanings of gender, race, whiteness, and of difference itself are

modified within specific institutional contexts and organizational settings (Calhoun 1994). Settings influence which aspects of our identities are “in play” during any given interaction and provide the mechanisms by which these aspects are interpreted and differentially punished or praised. In this section, we reveal the institutional scaffolding along which our identities hung and analyze how our relationships with research subjects influenced our vision.

Project Rehabilitate Women

East State Prison, like other total institutions, organizes its members into one of two mutually exclusive groups. Setting members are either staff (counselors, administrators, and correctional officers) or inmates. This is a rather crude description, of course, because it obscures variation within each of these categories (e.g., staff are granted differential levels of authority while inmates are subjected to differential levels of surveillance) and fails to specify the group into which outlanders like attorneys, family members, community activists, and social service workers fall. Nevertheless, the distinction is a useful one because both staff and inmates recognize it as the nucleus around which identity claims are constructed and contested. Consider the similarity between the explanations offered by a veteran correctional officer and an inmate serving her ninth year regarding membership in the prison community:

All prisons are the same, whether it's men's or women's. There's a line that divides the inmates from the staff... sure there are differences between, you know, between a CO [correctional officer] and a counselor or a line officer and a sergeant, but it all still comes down to that basic division between us and them (Ted, late 40s, African American).

Listen, it breaks down to us and them. So-and-so can be nice and stand up for me in court or listen to my problems and help out with getting my kids in for a visit but in the end, I'm the inmate. You see? Lawyers and all of them gonna punch out and go home at the end of the day. I'm their work and that's it. They're not here and that's a big deal. It is. It's what they got in common with all the staff in here, you know? If the shit hits the fan, they not gonna give that up to help me. They staff when you really think about it. They're not inmates and they ain't never gonna be (Star, 36, African American).

Over the course of my four years in the prison, the actual role I assumed could be considered an intermediary one. For example, I did retain a set of privileges consistent with the role of a staff member (e.g., I came and went as I pleased in the facility, I was not required to share my thoughts and experiences during group therapy sessions, I could meet privately with inmates, etc.). Also, my involvement in the evaluation meant that I was afforded opportunities to provide input into matters relevant to the PRW program and its relationship with the larger prison. On the other hand, I was held to the same general set of rules as the inmates in the PRW and was sanctioned by counselors and inmates alike for rule violations.⁹

⁹The program maintained strict regulations over language, comportment, and behavior. When I was sanctioned, it tended to be for fairly minor violations such as failing to push in my chair after leaving

In addition, I refused to assume the role of an auxiliary counselor despite the encouragement from PRW counselors. The position of auxiliary counselor would have required me to report and sanction inmates in PRW for rule violations as well as to periodically direct group therapy sessions. The decision not to participate in the program's disciplinary structure was critical and, in many ways, facilitated my intermediary position within the prison hierarchy. In classifying myself this way, I want to emphasize that my privileges and freedom in general meant that my role was obviously closer to that of a staff member than an inmate. Nonetheless, the critical distinction between my role and that of a staff member was my nonparticipation in the prison's disciplinary structure. It was of crucial importance to both inmates and staff that I did not report anyone for rule violations, I did not participate in public disciplinary routines (except when I was the target), and conversations with me were kept strictly confidential.

At times, my silence on disciplinary matters was a threat to my own involvement and safety in the setting. In one case, I was suspected by prison authorities of having evidence regarding the identities of inmates who had smuggled several bags of heroin into the facility. Although I was threatened with immediate expulsion from the facility among other things, I did not turn over any of my research materials.¹⁰ This act had a significant impact on my relations with both staff and inmates. When introducing me to other inmates, my respondents would usually identify me as "family" (a term used by inmates in PRW to refer to one another as well as inmates they were friendly with in the prison's general population) and as "cool." A second element that was crucial in how my role was constructed involved my familiarity with the social networks of several of the inmates. I have discussed this previously, but would note that during my first year in the prison, I was routinely introduced by inmates through references to these networks. One day a few months after the heroin scandal had died down, I was taking great pains to explain to a new inmate that I was not a lawyer, social service worker, counselor, or prison employee. Another inmate whom I had spoken with informally on a few occasions but whom I did not know well interrupted me and said, "Basically, she's our friend. What you tell her stays with her."

Defining myself to prison newcomers was difficult for two reasons. First, there was no word that adequately captured my role in the prison (the term "researcher" meant nothing to inmates, while "friend" was quite presumptuous). Second, race, class, and gender mattered. Inmates, white and black alike, regarded white outsiders, particularly those sporting symbols of middle-class wealth (leather shoes

the table, talking out of turn, or failing to announce my departure from the PRW unit. Since I was not an inmate, I was subject to public censure as the sanction for rule violations and not other common types of punishments such as loss of phone privileges, cleaning detail, and the like.

¹⁰On one occasion, I was threatened by a correctional officer who told me I was "on my own" in the facility (i.e., officers would look the other way if I found myself in a precarious situation with either an inmate or another officer). She later apologized when one of her fellow officers was implicated in the heroin scandal.

and briefcases, name-brand business apparel, “real” jewelry) as representatives of “the system.” White men were cops, white women were counselors. Middle-class African American men and women were seen more broadly, as representatives of the system (cops, lawyers, probation officers, social workers), religious practitioners, and members of reform groups. Outsiders who appeared from their apparel to be poor or working class were regarded as the family members and friends of inmates. Although I displayed visible markers of what inmates and staff would consider working-class standing (e.g., unlike outsiders who were regarded as “professionals,” I regularly wore jeans and had a visible tattoo), my whiteness and the privileges I enjoyed within the institution meant that many inmates initially defined me as one of the staff.

In acknowledging the relevance of my whiteness, I want to emphasize that race was a more complex matter than many prison outsiders assume. Indeed, I have discovered that the “race effects” question asked by many academics following discussions of my work is premised on a set of assumptions about the polarized racial order of prison and their interpretations of whiteness in settings where racial/ethnic minorities constitute a majority of members. Indeed, what is left implicit is the belief that in settings where whites are a minority, whiteness is an enduring, fundamental, and pervasive barrier to participation, rapport, and trust. These assumptions are flawed on at least two counts. First, conceptions of a polarized racial order (particularly the splintering of inmate relations along racial/ethnic lines) are premised on studies and first-person accounts of men’s prisons. There is very little in the way of comparable investigation of women’s prisons, although the few studies that exist as well as my own observations reveal that the salience of race for inmate organization is both more subtle and more complex than earlier studies of both men’s and women’s institutions suggest (Kruttschnitt 1983; Diaz Cotto 1996). Second, the question presupposes the centrality of race in subjects’ construction of my identity and in determining the course our interactions would take. I am not arguing, of course, that my whiteness was not a salient feature in how I was identified, how things I said were interpreted, and how my actions were evaluated. Whiteness in prison as in wider society is a critical feature of the interaction order because it represents both a position of structural privilege and a legacy of cultural practices premised on exclusion, domination, and exploitation (West 1994; hooks 1994; Abu-Jamal 2000). However great the symbolic potency of whiteness, though, it is not a singular or sole determinant in the construction of social identities, nor is it interpretable according to some monolithic code.

Race was certainly a salient feature of who I was in the setting, particularly among inmates who were apt to define white outsiders as representatives of “the system.” It was not, however, the most salient feature of my identity. My status as a “free person” was by far the most noticeable, enduring, and troubling aspect of my identity to inmates. Indeed, while I was able to overcome being regarded as a social worker or other sort of professional outsider fairly quickly, I was never

able to bridge the divide between freedom and incarceration. Freedom implied an autonomy far more complex and wide-ranging than anything that was achievable by inmates. Beyond this, it also implied a freedom over and against objectification. This involves more than the privileges conferred by skin color or class position; it involves issues of bodily and psychic integrity. Indeed, during the course of my involvement in the setting and relationships with countless inmates, I slowly began to understand what inmates knew implicitly—that in the free world which I inhabit, no one has the *right* to strip away my clothes, search my body for contraband, force medication down my throat, assign me to live with strangers, take away my food, place me in isolation, disclose my private experiences to numerous audiences, take away my possessions, prevent me from defending myself from physical and psychological threats, publicly humiliate me for forgetting to push in a chair or sneaking a cigarette, deprive me of sleep, and subject me to countless assaults on my sense of myself as a person.

One day after a large group of university students had taken a tour through the prison, Ann, who was serving her third term in prison for drug trafficking and prostitution, called me over to her:

Did you see them, them kids, students whatever they are from that university of yours? They came through here and I felt like a zoo, like an animal. They looked at me like I was a goddamned animal. Nobody's ever looked at me like that before. Not all the men, those fucking pricks, that I sold my butt to, no one of them ever looked at me like that, you know, you know what I mean? No, you can't know what I mean 'cause you ain't in here, you'll never be looked at like that—no one will ever see you in some pen somewhere and ask themselves what caused that animal over there to be like that. Was it 'cause she's some dope-fiend addict, was it cause her mommy's old man got off on her when she was some kid? No, no way—they'll never ask that about you.

She's right, they never will.

Women Against Violence

The context of discovery at WAV was marked by the intersection of structural privileges and disadvantages. Race was a central focus for WAV, at least bureaucratically. WAV's stated mission was to eradicate violence and its causes. The members believed that racism was a key root of violence and subsequently made race an organizing principle in 1969. In the bylaws, they mandated that the board should be racially balanced, with the African American founder as a permanent member. Committees were to be co-chaired by women of both races, and presidents changed from white to black annually. The founding members believed that the best way to avoid privileging one racial group over another was to mandate numerical equality. In this way, WAVers sought to overcome internal racist divisions that divided so many social action organizations of the 1960s and 1970s. WAV institutionalized racial difference in order to overcome it. Their symbolic colors were white and black, and their banner depicted a white hand clasping a black hand.

Ironically, WAVers actively avoided problematizing race or any controversial matter in any way. They were virtually silent about race as it affected the members and their community. When I asked Jenny, WAV's white secretary, if WAVers discussed race, she replied, "NO, NO. It is just a sort of matter of fact. Like, well we need to have at least this many black women and this many white women. It doesn't seem to be a real issue, just as a matter of fact." On an official level, the founder forbid board and staff members from publicly denouncing violence in the name of Women Against Violence. When violence broke out in the community, newspaper reporters called WAV to ask for statements. WAVers were instructed to make "no comment." Despite their activist aura, WAV contented themselves with providing minimal aid to the profoundly poor. Even in this capacity, WAV mandated silence. For example, as part of her duties in running the welfare office, Carla worked with other agencies. If she attended a meeting of a politicized nature, she could not refer to herself as a representative of WAV. She went to a workshop on gun control, but WAV would not endorse her presence. Carla said,

It was so embarrassing. Here I am at these meetings with my colleagues from these other agencies, and they introduced themselves as, "I'm Mary from the Battered Women Shelter." At my turn, I stood up and said, "I'm Carla." Everyone knows I'm from WAV, but by not saying it, I made it clear that I was on my own.

Staff and board members who broke the code of silence were punished in various ways, including expulsion. It took me a while to reconcile the disjuncture in WAV's rhetoric and their actions in my analysis, and I violated rules along the way.

Myriad status differences between WAVers and me compounded my troubles: we differed by age, wealth, race, and marital status. The women were accomplished ladies, and they expected ladylike affectations from me in our interactions. I walked a fine line: On one hand I had to be a demure, polite, deferent "girl"; on the other, I had to be a confident, assertive, qualified researcher. And I had to be both of these things simultaneously in order to be seen as credible. I solicited interviews using letterhead stationery and official phone calls; but I followed up interviews with gracious personal notes on monogrammed note cards. My manners were constantly in the foreground of interactions. To ingratiate one reluctant (and important) white respondent, I drew on all sorts of gendered resources. Because she was a Mary Kay beauty consultant, I hosted a Mary Kay party at my house. I watched Mary Kay videos at home. I bought many lipsticks and nail polishes. She never agreed to be interviewed, though, so my wiles were limited.

My manner of dress was scrutinized. At my first board meeting, I dressed in slacks, a blouse, and some coordinating low-heeled shoes. I recognized immediately that I was underdressed, and my observation was punctuated by an older white member's exclamation: "Don't you look comfortable! I wish I felt as comfortable as you look." At the next board meeting, I wore a skirt, a jacket, tights, dressier shoes, and more ostentatious jewelry. No one commented on my appearance. Although I felt less physically comfortable in this outfit, I felt more comfortable as a

board member. Because of my biography, it was easier to negotiate the gendered expectations than those related to race. When I embarked upon the interviews, I was unaware of WAV's taboo against problematizing race. Thus, I blundered into several awkward settings, and occasionally complicated data collection. In talking with the African American women about racism, I felt like an interloper. Aside from the founder, who spoke passionately, the Black women gave nonverbal signals—sighs, monotonal responses, fingernail tapping, eye rolling, disinterested smiles—that encouraged me to move on. They seemed to say, “How could a white person like you understand the legacy of this oppression?” *Of course* racism had affected their lives, and it affects them still. My race marked me as an outsider in many instances. I naively thought that my familiarity with black feminist thought would credential me as a trustworthy “knower” for these women; it did not. When I discussed racism with the white women, they often stared at me blankly until I probed. One woman put her head in her hands, sighed frequently, and told me the questions were hard. Several of the white women became tearful while remembering observed instances of racism.

In reexamining my post-interview fieldnotes, I noticed a difference in my interactions with the white versus the African American women. For the most part, my field analysis focused on my feelings in the interviews with black women. With the white women, I focused instead on the substance of the interview. Here is an excerpt from my fieldnotes after my first interview, with an African American board member named Terry:

This is the most hostile woman I have spoken to in a long time. She is young, beautiful, and she has light skin. She is very reluctant to talk to me about general issues of race and class; I think she felt attacked. . . . She is cold, although intelligent, but not so intelligent. She wanted a great deal of guidance in the questions, like, “What does peer group mean?” That is pretty straightforward. I didn't get a good feeling; it was a 35 minute interview. I didn't know how to probe.

With Terry, I began the interview by asking about her life experiences as a black woman, and then I worked toward discussing the dynamics of WAV. I surmised that I had thrown her off balance by beginning with personal topics when she was only prepared to discuss WAV. In my fieldnotes, I tried to explain her hostility as a mismatch between my questions and her expectations of the interview.

My second interview was with a white board member, Sandra. I used the same interview schedule. I compared the two interviews in my fieldnotes: “Sandra was much easier to talk to. But I do wonder if my questions are more like exam questions. Especially with Terry. Sandra was much more easygoing.” In this preliminary analysis of differential tension with a white versus a black woman, I explained Terry's clipped answers as a sign of the questions themselves, not of poor rapport. I investigated the research process through a lens of white privilege, underplaying the effect of my whiteness on the way my questions affected Terry versus Sandra.

After Terry and Sandra, I restructured the interview schedule to flow from specifics about WAV to general questions about race and gender. Reordering the questions did not erase the tensions in the interviews. My whiteness communicated information to the black members that I neither expected nor investigated for some time. Thus, the interviews were still flawed. For example, here are fieldnotes from a subsequent interview with another African American member, Sarah:

I don't think this was a very in-depth interview; it could have been done by questionnaire. Which is unfortunate, but these women are busy. She took my questions seriously and answered them. She wasn't up for probing. I didn't get a single—maybe one—story out of her.

I rationalized the tension by the fact that Sarah was very busy, that my tape was running low, and that I had arrived late. All of these factors exacerbated rather than explained the difficulty of the interview. In actuality, my unexamined whiteness and my ignorance of the norms impeded rapport.

This contrasts with my seemingly natural rapport with the white women. I interviewed Marion, whose status sharply contrasted with mine: she was in her 70s, WAV's president, and an accomplished "lady." Despite these differences, I was not intimidated:

This was a fun interview. I was very nervous about this, but it went very well, I think. Marion was very open and candid. She elaborated on every point she made, which was great; I didn't have to probe her at all. . . . She was very direct. She rarely looked away except when she was ruminating on a particular story. [In contrast] Sarah only looked at me when she was finished with her answers. Very different. But I know Marion, and that helps. My very best interviews have been with people I know.

I had become acquainted with Marion while I volunteered at WAV. Knowing her personally made a difference in my comfort level. But I began to notice the complexities of rapport after this interview: "I got lots of input from Marion on racial issues. In contrast to Sarah, whom I didn't poll on these issues. I seem to have this tendency to avoid race with black women but confront white women." In my next interview with a white woman, Elaine, I had an epiphany:

I noticed right off that when I am talking to white women, I sit back, relax. I am comfortable. When I talked to the two black women (whom I have interviewed so far), I was on the edge of my seat. I was working hard on my facial expressions to make sure they knew I was listening and that I found what they said important. Very different. I am sure this has to do with my race and the heavy weight it carries in trust and communication.

I was troubled by my lack of rapport, and I began looking for various artificial routes to connect to the members in the interviews. In my notes, I frequently commented on the black women's appearance—skin tone and beauty. For example, I said this about Marjorie: "She's a very beautiful woman. She is probably 60. Has curly gray hair. Most beautiful teeth, I kept staring at her teeth." When I made a connection, I became excited about the interview and analyzed the woman's answers:

Marjorie has a very clear racial identity, she has experienced racism, she's always been the "other." She is sometimes the "lone black child." She pointed out that class is different in

the impact of race on people's lives. Especially women. I didn't talk to her that long, but I feel I learned a lot from her. She's just a wonderful woman.

Making ideological connections was important for the tone of the interviews: if the women echoed theories that I espoused, then I positively embraced them. For example, in my interview with Meg, a white woman, she discussed poverty and racism as "systemic issues," and she used Hegel to frame her analysis of inequality. My fieldnotes show that she resonated with me intellectually: "This woman is a Marxist and I love her." The connection was effective.

Altogether, my interviews and fieldwork were a complex act of juggling privileges and disadvantages. My race, gender, and class affected every interaction, subsequently governing my ability to collect data. I used my gender enactment as a resource for connecting with women whom I otherwise could not, due to socially constructed, reified race and class barriers. Collecting data itself became a social act, and the character of the data was affected.

DIRTY DISCOVERIES, BURIED SECRETS: THE SINS OF SCIENTISTS

For Smith, Harding, and other standpoint theorists, the trouble with science is not necessarily that the origin of research springs from the location of the researcher, rather than from the location of the researched. Since we all occupy definite locations in the social landscape and our research motivations are rooted in the soil that surrounds us, this is unavoidable. The problem is that in the act of "discovering" scientific truths, the skeletons we dig up are often our own. What passes as a scientific discovery about "the Other" is often the very assumptions and narratives we used to construct our subjects and their "difference" prior to entering the field. In this section, we analyze how our positionality and our relationships with respondents influenced the ways we practiced sociological research. Here, we critically examine the artifacts of our studies in an effort to understand what we dug up and what we buried.

Project Rehabilitate Women

One of the dirty truths I have long kept buried about my project is the substance of the initial research question. Earlier in this article, I revealed how the dueling elements of my biography were the immediate source of my belief that the women were "out of control." This belief subsequently gave rise to a problematic to be studied: Why don't women resist the conditions of their confinement? On the surface, the question is not a professional embarrassment. Members of my thesis committee regarded it as "groundbreaking" and an editor of a major criminology journal encouraged me to send him the completed manuscript, noting that

it would probably “sail through” the review process. Nonetheless, I never took him or other professional gatekeepers up on their offers to publish this work because the original research question is a personal embarrassment. No matter how well the question may have resonated with *my* sense of my respondents (and the assumptions professional gatekeepers maintained about women inmates) it was inherently flawed. It ignored the fact that the women in my study did actively resist oppressive institutional arrangements and obscured the magnitude and intensity of their struggle.

Perhaps more troubling is that the question violated my sense of fairness and mutuality in my relationship with respondents. They trusted me with their stories and vouched that I was “cool” to encourage families and friends to do the same. They agreed to take me at my word and to regard me as something more than a staff member. They asked for something similar from me. As Teria (21 years old, African American) noted at the end of a life history interview, “Don’t slant my story, ok? Just tell it straight.” Sandra (33 years old, white) completed her interview by emphasizing broader aspects of her identity: “See, I’m really not much of a criminal. I’ve hardly been a ‘criminal’ at all. Be sure to say that.”

The articulation of that early research question is only the beginning of my story. Had I not identified with my respondents, it might also have been the end. The substantive relations between the knower and the known mediate the relationship between the knower’s standpoint and the production of knowledge. In this case, my relentless pursuit of similarities between my experiences and the experiences of my respondents (the “fictive” dimension of my positionality) contributed to a distorted set of assumptions about their character and why they were in prison. At the same time, this emphasis on similarity and the intermediary role I assumed in the prison community contributed to a sense of identification and, ultimately, respect. Beyond this, it forced me to regard their identities along multiple dimensions rather than along a single axis of criminality and drug usage. It also meant that I endeavored not to “slant” their stories about the institutional contexts they inhabited. A fieldnote written during my second year in PRW about Veronica (44 years old, African American) reveals my expanded sense of my respondents and the prison:

She is so much more than people on the outside would see. The ways the staff reduces identities to one narrow dimension like “addict” or “criminal” really gets me. It’s so convenient and allows for mass processing. Imagine if they considered Veronica in all her complexities—an activist, a hustler, a student, a daughter, a singer, friend, and lover. Telling her story means challenging the lens of the prison. It is a distortion to focus on her criminality to the exclusion of her activism and scholarship.

The process of data collection was shaped by my growing closeness with a number of my informants. Indeed, on a practical level, my identification with inmates meant that I was far more comfortable spending my time with them than with the staff members. In addition to some overlapping social networks, I shared with both white and black women similar linguistic expressions and cultural points

of reference.¹¹ During my first year in the prison, I actually avoided hanging around the staff. In part, I was aware of how busy the staff members were on a typical day, so I thought I was doing them a favor by spending the balance of my time with inmates and staying out of their affairs. I was also intimidated by their power and worried they would discover I was a fraud. In one case, a counselor who often boasted of her “psychic” powers would stare at me for minutes at a time during staff meetings. During one meeting (after she had been gazing at me for several minutes) I leaned over and asked her if she was trying to get my attention. She laughed and asked me what I was “trying so hard to hide.” What I was hiding, of course, was the fact I was really a student and not a “professional” and that I shared some of the characteristics (e.g., drug use, delinquency, a troubled history with authority figures, etc.) that the counselors regarded as symptoms of addiction and “criminal thinking patterns.” I later learned that this invasive, hierarchical style of interaction was fairly common among addiction counselors and was used to “throw addicts off their guard.” It left me feeling anxious and unnerved.

After several months of hanging out almost exclusively with inmates, I abandoned the earlier research question in favor of one that was grounded in the concerns raised by the women about their social worlds. Jenna’s (19 years old, African American) question was echoed by most of the women in the program:

The counselors be cussing at you and all that . . . They can break you down only so far. So much humiliation and then to break a person down. And what happens if that person can’t pick theirselves up? Who is she then? Where does her old self go?

Women in the PRW program worried that the program’s intense therapy techniques were a form of “brainwashing” that would dismantle their conceptualization of the self. Subsequently, the aim of my project was to explore the process and politics of identity construction in an institutional environment committed to radical transformation of “flawed selves” (McCorkel 1998b). Like my respondents, I wanted to understand the consequences of this radical form of therapy for their self-identities. Beyond this, I wanted to explore the institutional arrangements that gave rise to situations in which they found themselves.

My distance from the staff conferred both advantages and disadvantages. It propelled me into closer relationships with inmates and our relationships sharpened my interest in critically interrogating institutional rhetoric and master narratives. This meant, for example, that I did not refer to my respondents as “addicts,” nor did I make use of the explanatory schemes staff adopted to comprehend and diagnose their behavior. Respondents who were inmates helped me understand that addiction is a constructed category and, in this setting, had less to do with the ingestion of drugs and alcohol than it did moral judgments about the *kind* of

¹¹ Expressions such as “Do ya know what I mean?” asked at the end of nearly every statement and “for real” were associated with working- and lower-class culture in the greater Philadelphia region. These statements were used liberally by both whites and blacks from the area. Cultural referents included music (particularly hip hop and hard rock), the local club scene, clothing styles, and drug lore.

woman who participates in drug-related crime. Additionally, “addict” is a contested category of identity which most of my respondents challenged as inapplicable and offensive. Finally, the term itself implies that the women in PRW have a distinct set of cultural or personality traits that identify them as “addicts” and “criminals.” This inhibits an understanding of the ways in which discourse and the practice of therapy effectively depoliticize sociopolitical arrangements by rendering the troubles poor women experience in their everyday lives as products of individual pathology. Although I was initially guilty of the latter, my relationships with my respondents (particularly their trust in and acceptance of me) forced me to interrogate and, ultimately, abandon my own flawed assumptions about their identities and our “difference” from one another.

There were distinct disadvantages in my distance from the staff. In an institution where alliances are brokered along the division of inmates and staff, my close association with inmates meant, from the staff’s perspective, that I was untrustworthy. As a result, I did not have the same access to staff members. Conversations with staff members of all levels, but particularly PRW counselors, were fairly guarded. Early on, I fell prey to regarding them as power-hungry, one-dimensional beings and portrayed them this way in my fieldnotes. During my 3rd and 4th year in the prison, I did take greater pains to improve the relationship.¹² This allowed me to uncover the ways they constructed the women and the various strategies that staff at all levels employed to solve problems associated with drugs, crime, and prison overcrowding. Nonetheless, my analysis of PRW was incomplete. While my study emphasized how structures of race, class, and gender disadvantaged women inmates, it did not consider the influence of these structures on the staff. I routinely downplayed the fact that PRW’s counseling staff was made up predominantly of African American women from working-class backgrounds.¹³ I failed to connect their experience in the prison and their everyday worlds to a larger set of institutional arrangements. While I kept my promise to inmates to tell their stories “straight” and present them as whole people, I fell short of doing so with the staff. Their story remains buried.

During a job interview, I was asked whether or not another researcher going into the same prison would have made the same discoveries as I had. I answered no. It was not the answer the inquirer was looking for. If my observations and conclusions could not be replicated in another study, how could they be considered “science” rather than “mere journalism”? My findings are unique because they are ultimately based on a standpoint that was multilayered and a set of relations that were complex, shifting, and dynamic. Indeed, my close relations to women

¹²I began to regularly eat lunch with them. I made a point to sit in on weekly staff meetings, and I offered to help out on any number of odd projects. Most importantly, I hung out and listened to gossip, treatment plans, and the trials of their personal lives.

¹³Staff at higher levels within the prison and the wider criminal justice system were predominantly white and male.

incarcerated in PRW sensitized me to the fact that a significant portion of what passes as “good” prison research is fundamentally flawed because the questions that structure the study as well as subsequent analytic constructs are often generated from the perspective of the “keepers” and, as such, distort images of the “kept.” By starting with my respondents’ questions about the world (rather than questions generated by myself, clinicians, or academic criminologists), I had to abandon my assumptions and challenge discursive schemes that legitimated the institutional order. As a result, I was told by one of the PRW counselors that I lacked the “clinical mindset” necessary to understand what was taking place in the program. A journal reviewer admonished me for my “naiveté” regarding “inmate fictions.”

Women Against Violence

My analysis centered on understanding the contradictions within WAV: they advocated racial equality but refused to discuss racism. They believed in “woman-power” but they eschewed feminist activism. As explanation, I asserted that WAVers’ class alliances overshadowed their racial politics. African American WAVers—who occupied a tenuous social standing given the racist historical legacy of the community—risked losing ground by being seen as activists (Gartner and Segura 1997). Their wealth more closely aligned them with the white power structure than with the majority of blacks in their community. This was not the anti-racist utopia I anticipated. WAVers’ standpoints were characterized by both privilege and disadvantage, and they protected their privileges (Myers 2001).

Convincing readers of the validity of my analysis proved to be a tough chore, largely due to my own standpoint. I was a white, middle-class academic critiquing the effectiveness of African American elite community leaders. I risked being seen as appropriating and commodifying black women’s issues for my own academic gain (Collins 1998). As such, I faced several quandaries in the research process.

The first involved data collection. I worked hard to provide compelling evidence about WAV’s class dynamics. Influenced by the preponderance of literature treating “black” and “poor” as synonyms, as well as the doubt I encountered in talking to my dissertation committee about my data, I combated skepticism regarding the African American women’s wealth. In trying to *prove* that the African American women actually were elite, I described in my fieldnotes their clothing, education, cultural capital, even jewelry. I invoked my own cultural capital as evidence that “I know money when I see it.” I could not reveal too many details about the women or their community because of confidentiality. Yet, the particulars of these women’s lives drove my analysis: their lineage, their portfolios, etc. I did not focus on the material spoils of the white women because (a) more of them were middle class than wealthy, and (b) I took for granted a greater degree of economic diversity among whites than blacks in the U.S. Here lies my primary analytical sin:

writing for the skeptics helped reproduce stereotypes linking poverty to African Americans. In my efforts to substantiate their wealth, I implied that “these are exceptional blacks”—clearly a racist claim in itself. I sighed with relief when I finally read Zweigenhaft and Domhoff (1999) because they provided legitimate, “objective” evidence of the African American WAVers’ class privilege. I backed off from the copious descriptors and relied on the literature instead.

Second, my standpoint raised political issues. I felt as if I had made a dirty discovery. I was wary of concluding that black WAVers’ wealth altered their racial alliances. I did not want to appear naïve about the power of racism in the U.S. Despite WAVers’ economic gains, they still were seen as black first in the eyes of their community. It was problematic for me to liken African Americans to the white power structure, regardless of wealth, given the visceral history of American racism. I doubted my own analysis. Using Collins’ (2000) concept of intersectionality helped make sense of the contradictions—power and oppression may exist simultaneously. Again, I relied on the literature to alleviate tensions. I hid behind this cloak of legitimacy and tried to cover my concerns.

The third problem has been harder to remedy. My standpoint affected the reception of my work. Because of points one and two above, reviewers have expressed concern that I am judging the women in my study. I am a politically dangerous messenger; my message is easy to reject. On some level, their assertion is true. I did hold high hopes for WAVers, and they let me down. I surreptitiously tried to ignore the seeds of truth, inserting more quotes in my papers to verify my claims. Yet, my judgment continued to seep through to the surface. In response, I openly included my standpoint in my writing. By revealing my personal struggles as a researcher, I hoped to assuage apprehensions. Surprisingly, this actually exacerbated reviewers’ concerns about validity. It gave reviewers a hook on which to hang their misgivings—bringing into focus what had been quietly nagging them as they read the paper. Being honest can backfire. It goes against all of our methodological training. Being dishonest is easier, but it is a greater sin in terms of the integrity of the research endeavor.

Finally, WAVers’ wealth created a real power differential between me and them that sometimes overshadowed my power as a white researcher. A book editor recently rejected my prospectus, arguing that there would be no way to maintain WAVers’ confidentiality. He said that he did not want to be sued. I, too, have feared lawsuits. My analysis is not slanderous, but it is critical. I worried about damaging the social and political networks of my informants. My trepidation has prevented me from sharing my work with the respondents, despite encouragement from feminist scholars to do so (see Fonow and Cook 1991). Recently, for example, WAV’s current (white) president contacted me. Frustrated with WAV’s old guard and their resistance to change, she wanted to read my work to gain insight. In her query, she assured me that she would not share my work with the founder and the board. Her unsolicited assurances brought back old fears: without even reading my

work, she was already entertaining its ramifications. In the end, I referred her to my published pieces, but I did not provide her with any works in progress. Taken together, then, my standpoint has both enhanced and hurt this project. It opened doors and prepared me for the gendered expectations of the site. However, it also inhibited my ability to give the women voice.

CONCLUSION: FIELDING PRIVILEGE

This project began with a question: What difference does difference make in sociological research? The question is a compelling one because, as a number of standpoint theorists have noted, the manufacture of research occurs within a wider field of social relations. Unlike many other forms of representation, scientific inquiry is practiced almost exclusively by members of dominant groups in ways that reflect and enhance their situated interests. In this article, we have sought to address questions and concerns about identity and difference by scrutinizing the front and backstages of our own research projects. In so doing, we present a unique opportunity to glimpse how position and privilege influence the production and performance of research.

Although differences in design and setting make immediate comparisons between each of our projects difficult, there are a number of key parallels in terms of our assumptions, practices, and relationships that allow us to identify several of the ways in which positionality affects research process and outcome. First, positionality confers distinct sets of narratives that we use to make sense of the world. Master narratives originate from dominant groups and operate to legitimize and naturalize the order of things. They become part of how we see the world and, by implication, our research subjects. The manner of their influence on each of us was subtle and indirect. McCorkel, for example, rejected certain constructions of women inmates (e.g., dangerous) while embracing others (e.g., out of control). Indeed, master narratives remained influential despite our access to various counter discourses that challenge the legitimacy of existing race, class, and gender arrangements. Subsequently, while the content of our research proposals and preliminary write-ups would suggest that master narratives were not “in play,” it is clear from our analyses here that these narratives influenced the ways we approached, reacted to, and interpreted our relations with respondents. This, in turn, shaped the questions we asked and the analyses we produced.

The influence of these narratives cannot be directly attributed to our competence (or lack thereof) as social scientists. As Harding (1991) makes clear in her critique of positivist standards of objectivity, values and assumptions are products of a researcher’s positionality and, as such, are not eradicated through the methodological procedures that positivists use to test hypotheses. The link between positionality and master narrative is evidenced in Myers’ struggle to understand her

poor interview rapport with African American women. Compared to the easy rapport she enjoys with many of her white respondents, she has difficulty interviewing African American women and spends much time in her fieldnotes examining her feelings about what she interprets as their hostility. Although she is knowledgeable about black feminist literature, she initially fails to identify whiteness as a factor in the different levels of rapport she experiences with white and black respondents. This lapse makes sense only when we consider Myers' positionality—as a white, middle-class academic she moves through racially segregated spheres (e.g., housing, education) wherein her opportunities to interact with black women who talk back are extremely limited. By focusing in her fieldnotes on the physical appearance and apparent hostility of her African American respondents, she downplays their responses to her questions and thereby mutes the challenges they present to her beliefs and assumptions about her own identity as well as theirs.

In the course of doing this study, we discovered that as researchers we were not only susceptible to the influence of master narratives, we often reproduced them in the course of our fieldwork. McCorkel did so when a respondent challenged her claims that she shared a troubled past that was similar to the experiences of her respondents. Rather than acknowledge her own race and class privilege, McCorkel endeavored to account for differences in their social standing by drawing on popular accounts of drug-involved women as “out of control.” This influenced how she interpreted their activities (e.g., their nonviolence was passivity) and later became the source of a “problem” to be studied (e.g., why don't incarcerated women resist?). In this case as in the case of Myers' discussion of respondent hostility, master narratives came into play when aspects of our identities as researchers were directly or indirectly challenged by our respondents. In these instances, we worked to preserve our privileged status as knowers. Master narratives were the resource that we used to accomplish this.

Ironically, while we saw it as our job as feminist ethnographers to give voice to women whose views and experiences were overlooked in sociological and political discourses, our use of master narratives threatened to erase all but our own situated perspectives, especially in our early analyses. Subsequently, we ran the risk that our studies would do little more than repackage our earliest, self-interested assumptions about our respondents as scientific truth. It would seem that for knowledge production, difference makes the difference.

This brings us to a final question: How does a researcher attenuate the influence of the master narrative and, concomitantly, her positionality? Earlier, we suggested that feminist sociologists have essentially pursued one of three different strategies. One strategy involves elaborate efforts by the researcher to renounce privilege. McCorkel's early attempts to emphasize her supposed similarities with incarcerated women highlight the futility of such an approach. A number of respondents read such efforts as disingenuous and insulting. Further, such a strategy obscures crucial insight into the relationships between the more and the less

powerful. In this case, McCorkel was able to understand how incarcerated women were constructed by political and institutional elites only when she recognized how her own race and class privilege influenced her assumptions about their pre-prison lives.

Another strategy feminist researchers have pursued is to erase the negative effects of positionality by “studying up.” This was certainly part of the appeal of WAV. Here respondents are women who are considerably more wealthy and powerful than Myers. Their wealth and power does provide them with a greater level of control over the research process than respondents in PRW possessed. Myers worked largely around their schedules and demands and, beyond this, encountered obstacles in publishing a book because of the potential for a lawsuit. At that same time, Myers discovered that studying up did not erase her positionality, it recast it. The wealth and power of her respondents did not eliminate the significance of her racial privilege, nor did it facilitate a more accurate account of WAV and its members.

In doing these ethnographies, we discovered that the researcher’s positionality cannot be erased with either of these strategies or with scientific rigor and elaborate efforts to achieve value neutrality. What is necessary is a crisis point from which to apprehend, interrogate, and challenge the narratives that the researcher uses to make sense of the world, as well as her location in it. Crisis points often occur when the narratives we use to make sense of reality do not resonate with our experiences. Such is the case for persons who are marginalized within a particular institutional order (“outsiders within”) who experience a line of fault between experience and narrative. But what of persons who are advantaged within a particular institutional context? In both field settings, our positionality conferred privilege and from this location we initially theorized that the difference between ourselves and our subjects emanated within *them*. The source of their difference became the basis of our research agenda. We directed our gaze at them (e.g., their physical appearance, their activities, etc.) rather than toward ourselves or the relevant institutions.

However, this is not where our gaze remained fixed. Using standpoint epistemology we ultimately pursued a version of the third strategy. This strategy involves a recognition and analysis of how the researcher’s positionality facilitates specific forms of understanding and impedes others. We found that standpoint’s standards for strong objectivity and reflexivity are a crucial tool in this process because it not only forces researchers to redirect their gaze, it opens up space for critical dialogue with research subjects. This, in turn, creates possibilities for identifying and challenging master narratives. It does so by forcing researchers to confront themselves as socially situated. In examining the context of discovery, the researcher identifies how her motivations and assumptions give rise to a problematic to be studied. The researcher’s awareness of her own situatedness is further enhanced when she takes seriously the questions, concerns, and challenges that her subjects raise in response to the research process. In this way, the researcher views herself along the

same critical plane that she views her subjects. Indeed, our respondents reminded us in myriad ways that our problematics were not theirs and that our explanations did not resonate with their experiences. These relationships became crucial for rendering visible the context of discovery and ascertaining the legitimacy of our “scientific” claims.

The value of our work is not that it somehow manages to avoid problems associated with privilege and difference. Instead, it reveals them. This awareness led us to insights that we would not otherwise have had. We identified how master narratives worked their way into our “discoveries” about our respondents and how this partial and distorted view served to legitimate the social order and our (privileged) place within it. At the same time, power, privilege, and position remain salient even after our modifications. While we hope we have been fair to our respondents, we cannot claim to have been able to fully ground the research in their concerns. Indeed, we cannot even be sure that we have represented their concerns authentically. In the end, we still edited, silenced, evaluated, and categorized. Such practices are unavoidable in crafting sociological analyses. However, like Harding, we argue that truth claims are organized along a gradient of “better” and “worse.” In locating ourselves as agents of knowledge and scrutinizing the influence of our positionality on the practice of ethnography, we have sought to achieve a stronger form of objectivity—an objectivity that generates “better,” more realistic claims than those offered by positivism or earlier versions of standpoint methodology.

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