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Introduction

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Engaging Women's Communities: Dilemmas and Contradictions in Feminist Research

This volume explores the problems, dilemmas, contradictions, and prospects for doing feminist research, not only *on* women or even *for* women, but also *with* women. In feminist research gender operates as a "basic organizing principle which profoundly shapes/mediates the concrete conditions of our lives. . . . Through the questions that feminism poses and the absences it locates, feminism argues the centrality of gender in shaping our consciousness, skills, and institutions as well as in the distribution of power and privilege" (Lather 1988:571). These essays form a collection of original research on the topic, in some cases presenting revised or revisited arguments made in previously published work.

Each chapter discusses principles of feminist inquiry, contains illustrative examples from each author's own research, and evaluates research practices for their potential to promote social change. Detailed case studies follow the actual research process, starting with the selection of questions for investigation, through the establishment of relationships with a community, to conducting research and dissemination of the research findings. By building a bridge between theory and practice, these chapters highlight the dilemmas and the possibilities for social change posed by different feminist research methodologies. The book serves as a practical guide to making connections with different women's communities, to representing different feminist (women's) voices, and to discussing possibilities and problems for the production of knowledge inherent in different research methods and theoretical strategies.

1. Feminismo
2. Metodologia

In contrast to other collections, this volume includes a broader range of social science methods, new contexts, and a selection of essays written about North American cases. While most texts focus on a single method, *Feminism and Social Change* covers a diversity of methodologies, including interpretive research strategies and techniques such as ethnographic study, in-depth interviewing, naming, and going public; consultative relationships between academic researchers and activist organizations; participatory and advocacy research processes; and coalition building. Contributors address a range of questions:

1. Who will have ultimate control over the results?
2. Should the results promote a specific set of policies or programs?
3. How best can an academic researcher represent women's voices in the results of research?
4. What methods best engage women in the process and products of research?
5. Is there a privileged standpoint from which to discover the real relations among human beings?
6. What is the relationship between standpoint and experience (theory and practice)?
7. Must we forsake scientific standards to conduct feminist research?
8. If science is always partial, situated, and a cultural product, then what standards (if any) could be applied?

To demonstrate how this volume adds to feminist research, I will review recent literature on the topic. The review of research on feminist methodology highlights the gaps and issues that are grist for continuing debate.

RESEARCH ON FEMINIST METHODOLOGY

The explosion of feminist research has produced several excellent anthologies on feminist methodology.[1] *Feminism and Methodology*, edited by Sandra Harding (1987), compiles an interdisciplinary selection of chapters addressing the unique feminist contributions to several disciplines. This collection directs attention to and makes theoretical claims about the existence of an uniquely feminist epistemology[2] and methodology. The interdisciplinary essays in Harding's book add a strong voice to a larger chorus of feminist challenges to the traditional canon. Unfortunately, research issues are posed at a very high level of abstraction. All too often, the authors resort to rarefied philosophical language to make their cases. In so doing, the book is most useful for constructing reasonable arguments in favor of conducting feminist

scholarship. The authors, however, tend to remain silent on research methods and rarely come down to the concrete level to discuss their own methods or the research methods implied by their arguments (as acknowledged by Harding in her introduction). As a consequence, one is left without practical knowledge about how to conduct research in accordance with the various theoretical positions.

At the other end of the spectrum stand *Doing Feminist Research*, edited by Helen Roberts (1981), and *Theories of Women's Studies*, edited by Gloria Bowles and Renate Duelli Klein (1983), much earlier endeavors to assert feminism as a method of conducting research. In contrast to *Feminism and Methodology*, these texts primarily rely on experiential-based research practices. The contributors celebrate a woman's experience or voice that can be apprehended by and informs the necessity for feminist research methods. They insist that gender is integral to research practices and grant priority to experience as the primary source of knowledge. Yet basing research claims on the authority of experience can be limiting and exclusionary. Since the publication of these books, feminists have turned away from a search for either a single feminist theory, methodology, or authentic woman's voice. As a consequence, research practices premised on experience represent only one out of many choices for doing feminist research.

Assembling a group of interdisciplinary scholars from diverse racial, national, and ethnic backgrounds, two recent collections, *Women's Words*, edited by Sherna Berger Gluck and Daphne Patai (1991), and *Feminist Praxis*, edited by Liz Stanley (1990), offer a much needed corrective to these earlier volumes. Although *Women's Words* purports to focus exclusively on the feminist practice of oral history, as evidenced in the subtitle *The Feminist Practice of Oral History*, the volume includes informative discussions about other interpretive methods, such as ethnography, life history, testimonials, and interviewing. The authors of the chapters attempt to problematize as well as identify the conditions of possibility for the production of cultural representation. Such a pursuit, while tightly focused, should not be mistakenly interpreted as a narrow one because it spans multiple disciplines and methodologies.

Feminist Praxis begins with excellent theoretical chapters on the production of knowledge within a specific academic mode of production and on method, methodology, and epistemology in feminist research, followed by several chapters of substantive feminist research processes. This exemplary text contains substantive essays about British cases. However, the case material is less readily accessible to a U.S. audience. Both volumes weave together theoretical insights with illustrative examples from feminist research practices.

Two other books warrant mention: *Beyond Methodology*, edited by Mary Margaret Fonow and Judith A. Cook (1991), and *(En)gendering Knowledge*, edited by Joan Hartman and Ellen Messer-Davidow (1991). Fonow and Cook bring together noted feminist scholars, primarily from sociology, who examine feminist epistemology and methodology through a sociology of knowledge perspective. The chapters reprise four major themes extant in feminist scholarship: reflexivity, action-orientation, attention to the affective components of research, and the use of the situation at hand.

One of the most interdisciplinary collections is the recently published *(En)gendering Knowledge*. Whereas social sciences or the philosophy of science dominate most other texts on feminist methodology, Hartman and Davidow add to these the classics, biology, physical sciences, literature, and art history.

The above-mentioned volumes remain limited in their representation of research strategies and of women's diverse communities. Much of this writing on feminist methodology remains highly abstract, and the applied/practical/political side still requires closer attention, both by way of more abstract reflection and especially in the context of specific research projects in varied settings. *Feminism and Social Change* incorporates these criticisms, builds on previous insights, and tries to fill in some of the gaps.

ENGAGING WOMEN'S COMMUNITIES

This book begins with a discussion of broad epistemological and methodological concerns and then moves to the nitty-gritty of actual research projects, proceeding in three parts: part 1 explores dilemmas and contradictions in feminist research; part 2 presents case studies of research practices developed while engaging different women's communities; and part 3 considers various strategies of activist research, including participatory research, advocacy research, and coalition building.

As Harding (1987) notes, debates about feminism often conflate method, epistemology, and methodology. Some argue that feminism is a mode of doing research (Stanley and Wise 1983); others that it offers a theory of research practice (McRobbie 1982); still others that it constitutes a frame through which inquiries about the world can be made (Hartsock 1987a).

Those who argue for feminism as method distinguish between feminist methods and those same methods practiced in accordance with alternative theories. For example, ethnography and interviewing have

been viewed as giving expression to women's experiences. As Judith Stacey notes in her essay in this volume, "In ethnography, the researcher is the primary medium, draws on those resources of empathy, connection and concern that many feminists consider to be a woman's special strength." Joan Acker, Kate Barry, and Joke Esseveld, along with Judith Stacey, caution that empathy is not always possible, nor is it a defining quality of feminist research. Feminist researchers may work on topics that require interviews with women and men who have more power or different political aims than the researcher (see, for example, Kaufman 1991). Feminism as method sees the representation of women's experience as the beginning and often the end of the production of knowledge claims. Sophie Laws (1986 as cited in Kelly 1988) argues that sociological literature on feminist research has favored women interviewing women as a research strategy because of its parallel to consciousness-raising. Consciousness-raising as feminist method privileges experiential knowledge.

Granting priority to experience as the primary source of knowledge can lead to a "politics of identity." A politics of identity bases research claims on the authority of experience. In so doing, a politics of identity can be both limiting and exclusionary, since no personal experience is inclusive enough to encompass all human experience. Further, premising insights into oppression on the verities of personal experience can be atomistic and relativistic.[3] "Naming" one's own experience is only the first step toward collective self-liberation (Combahee River Collective 1984; hooks 1984).[4]

Yet, insight into and appropriation of experience is critical to feminist projects of social transformation. It is theory that facilitates the mutual understandings among diverse experiences upon which collective action can be taken. Theory makes understanding another's point of view possible (Kelly 1988), but theory is not a substitute for political action. Alison Jaggar observes that "theory alone will not liberate women. But women's liberation seems equally unlikely to result from simple activism, not grounded in systematic understanding of women's situation" (1983:289).

The recognition of a bond between theory, research, and experience denies the conventional social science attitudes of disinterest and disembodiment. For example, the researcher attempts to narrow the distance between herself and other subjects and engages with them—a process Helen Roberts (1981) calls "reflexivity." Angela McRobbie (1982) situates the feminist researcher both personally and politically; she contends that feminism forces the researcher to locate her own autobiography and experience inside the questions asked. Similarly,

Sandra Harding (1987) and Dorothy Smith (1987b) claim that in the best feminist research, the inquirer places herself on the same critical plane as the other subjects. "Thus the researcher appears to us not as an invisible, anonymous voice of authority, but as a real, historical individual with concrete, specific desires and interests" (Harding 1987:9). Feminism can situate the researcher within the problematics from which inquiry begins. The intimate connection between theory, research, and experience is realized in a mutual engagement among concrete, historical individuals.

The first section in this volume discusses these feminist debates over the relationship between theory, research, and experience. Dissatisfied with the equation of feminism and method, Sherry Gorelick reviews both feminist critiques of dominant methodological paradigms (especially positivism) and early feminist methodological claims. Feminist scholars have an important role to play beyond giving voice to our silent sisters. Simply letting women speak for themselves will neither address internalized oppression nor allow the subjects of research to uncover the hidden bases of gender, class, and race oppressions. Gorelick suggests that voice(s) offer the researcher raw materials, but that theory provides the interpretive frame to make visible the daily and concrete social relations through which men and women create their worlds. An emancipatory social science, then, should provide women with understandings of how their everyday worlds, their trials and troubles, were and are generated by the larger social structure (see also Acker, Barry, and Esseveld in this volume).

Still, voice remains a valuable concept in feminist research. Since the publication of Carol Gilligan's (1982) influential book *In a Different Voice*, the concept of voice has become ubiquitous in feminists' writings. Feminists have used "voice" to convey agency on the part of women speakers. However, the application of the concept has not always been consistent, shifting from literal to metaphoric meanings—sometimes in the same work. In its literal sense, voice purports to simply represent women in their own words, that is, to let women speak for themselves, a method particularly prevalent among feminist symbolic interactionists. The metaphoric meaning refers to the logic of the speech act. To say that women speak in a "different voice" is to suggest that frames of reference differ, as in Gilligan's example of women adopting a needs (caring) and men a rights (justice) discourse to ground moral claims. To escape both the empiricism implied by symbolic interactionists and the essentialism implied by Gilligan's formulation, Gorelick and Acker, Barry, and Esseveld advocate problematizing representation of women's voices and examining the connections between women's everyday experience and the relations that underlie them.

The remaining chapters in part 1 continue the focus on the politics of doing feminist research. As the contributors turn their gaze back to assess feminism's second wave, optimism gives way to ambivalence about the political possibilities for feminist scholarship to help transform the oppressive conditions affecting women's lives. Some attribute partial blame to the retreat of feminism into the academy, arguing that academic feminism may be the victim of its own past successes. Dorothy E. Smith offers a trenchant critique of sociology as a disciplining discourse. She uncovers the hidden political ground of the academy that deprives feminist social scientists of perspectives other than those that sediment in our disciplines. As an alternative mode of inquiry, Smith proposes an engaged sociology constructed from standpoints of women both inside and outside of the academy. Joan Acker, Kate Barry, and Joke Esseveld agree that once the production of knowledge for women became fully embedded in the academy, the ties linking it to the concerns of women outside became attenuated.

Qualitative research methods are discussed in the final chapter of part 1. Judith Stacey extends the discussion by formulating a question, "Can There Be a Feminist Ethnography?" In conducting fieldwork on family and gender relationships in California's Silicon Valley, she found herself wondering whether the appearance of greater respect for and equality with research subjects in the ethnographic approach masked a deeper, more dangerous form of exploitation. There are no easy answers to the question she poses. Through a feminist dialogue with the "new," "postmodern," or "reflexive" ethnography,[5] she puts to rest any quest for an unproblematic feminist methodology. She concludes: "A fruitful dialogue between feminism and critical ethnography would continue to address their complementary sensitivities and naivetés about the inherent inequalities and the possibilities for relationships in the definition, study, and representation of the Other."

The volume as a whole moves beyond the old log jam of qualitative (supposedly more feminist) versus quantitative methods, what Geoff Skoll (1993) calls a false divide. Roberta Spalter-Roth and Heidi Hartmann and Ronnie J. Steinberg show that quantitative techniques can serve social change purposes and that such techniques can be innovatively fashioned to produce outcomes favorable to women. For example, to make quantitative data more accessible to a lay audience, Spalter-Roth and Hartmann have fashioned new techniques such as "reconstituted diaries." Yet going beyond this false divide is only the

first step. We face other complex issues of how to choose the most appropriate method(s) for what we are studying, how to be more reflective and innovative in handling the drawbacks of each method, and how to be more creative in reshaping methods to more fully account for women's varied lives and experiences.

ENGAGING WOMEN: VARIED SETTINGS AND DIFFERENT COMMUNITIES

Chapters in part 2 detail the actual process of doing feminist research. As several of the essays illustrate, feminist scholars seek to transgress the boundaries of insider/outsider, observed/observer, and subject/object. Personal narratives chronicle the authors' attempts at negotiating these relationships.

Pierrette Hondagneu-Sotelo addresses the melding of research and activism in a project called "Immigrant Women and Paid Domestic Work," based on eighteen months of participant observation and in-depth interviews. In spite of many handicaps (legal, class, race, and gender), the research reveals that undocumented immigrant women workers construct and utilize informal social networks to stabilize their employment and to enhance their working conditions and pay. One of the more innovative aspects of the project involves its intimate connection to an ongoing advocacy project, the Coalition for Humane Immigrant Rights in Los Angeles. The coalition utilized Hondagneu-Sotelo's findings to construct *novelas*, didactic informational handouts using caricature scenarios and text. These handouts are now being distributed to domestic workers who ride Los Angeles public buses and to Latina nannies who congregate with their young charges in public parks.

Linda Carty discusses feminist research projects involving Caribbean women who were either current or former domestic workers and documented or undocumented immigrants. Being from the Caribbean and understanding the culture and some Creole dialects afforded her the privilege of initially approaching these women, but this insider status did not allow her total acceptance. The women's suspicion was predicated on Carty's positional difference as an academic and her political self-identification as a feminist. The research revealed the contested and politically charged nature of feminism and the implications of using the concept when trying to solidify participatory relationships and to achieve feminist and antiracist goals.

Further pursuing the theme of insider/outsider, Verta Taylor and Leila J. Rupp reflect on and present richly descriptive case material to

illustrate the problems and advantages involved in the process of making lesbian existence central to their research. In assessing their research on the women's movement, they discovered that many of the women lived together in marriagelike relationships and formed communities with similar couples. This led them to question how to discuss lesbianism in cases where the subjects themselves did not self-identify as lesbians. Taylor and Rupp "believe that it is important to pay attention to women's relationships, to describe carefully and sensitively what we do know about them, keeping in mind both the historical development of a lesbian identity and the individual process that we now identify as 'coming out.'" Their next project focused on women's cultures produced in lesbian communities. As insiders they gained access unavailable to outsiders: "It is not that insider status gave us a privileged vantage point on some 'true' story of the community, but rather that we had knowledge of ephemeral developments that might not appear in any written sources or oral histories and were able to interview women who were willing to speak with us because they knew that we were lesbians and trusted that we would generate our analysis from a lesbian feminist standpoint."

Nancy A. Naples and Emily Clark add a new voice to the literature on participatory research, mixing personal narrative and theoretical explication. Their narrative emerges out of the reflexive bond between theory, research, and practical action as they "go public" with their experiences of child sexual abuse. Naples explores the role feminism plays as an alternative discourse in shaping the storytelling process and as implicated in the articulation of survivors' discourses. The unfolding research process comes to life in the dialogue between Nancy and Emily, who bring different, albeit overlapping, interests (e.g., healing, consciousness-raising, career building) to the project.

FEMINIST ACTIVIST RESEARCH: MULTIPLE STRATEGIES

The chapters in part 3, taken together, provide entre into feminist activist research, including participatory, advocacy, and coalition-building types. Many styles of activist research have existed both within and outside of the academy as alternatives to conventional social science methods. Activist researchers share in common a commitment to social change and to empowerment of the subjects of research but use different methods to realize these goals.

The starting point of participatory research is the "problem-posing" educational program popularized by Paulo Freire (1972) and extended to feminism most notably by Patricia Maguire (1987) and most re-

cently by Francesca M. Cancian (in this volume). Participatory research exemplifies one of the most radical and activist elements of feminist methodology by enlisting a community's participation and collaboration in social change projects. Participation and collaboration are more than terminological hallmarks of participatory research: they specify the practices, subjects, and possibilities for such research projects (Hall 1981; Brown 1985; Brown and Tandon 1983). Cancian describes the components and process of conducting participatory research and analyses and identifies the strengths and weaknesses of several feminist and nonfeminist participatory research projects based on interviews with activist researchers. Various forces within and outside the university (e.g., careerism and the academic power structure) conspire to suppress participatory research methods.

In contrast to participatory research with its emphasis on grass-roots participation and collaboration, advocacy researchers operate in a policy context, often use available data sets, and stress the use of numbers as an important code in policy debates, that is, "counting is as important as naming." Unlike participatory researchers who engage directly with a community, those who work with advocacy organizations implicitly assume a division of labor between research organizations (who do the research) and national advocacy groups (who do the advocacy). According to Roberta Spalter-Roth and Heidi Hartmann, the advocacy researcher relies on others to tell us "what is important to women, what needs to be researched, and how that research will be used" rather than working directly with constituencies of women and learning about their needs directly through our own experience. Thus the connection between theory (research) and practice (the grass roots) is attenuated at best.

Spalter-Roth and Hartmann frame these issues by situating the debate within the historical context of two generations of feminist advocacy research. The first group of women received training as social scientists in the new research universities of the late nineteenth-century United States, which were centered around the University of Chicago and Hull-House. This important but lost history provides a unique vantage point to compare the second generation that includes the Institute for Women's Policy Research. They contrast today's division of labor model with the "Hull-House model" of feminist policy research, a model in which data collection, research, and analysis; policy change and advocacy; service and education to the community; and politics were all done simultaneously by Hull-House members. Proposing "the dual vision of feminist policy research," Spalter-Roth and Hartmann attempt to synthesize the views of the two generations—to create research that meets both the standards of positivist social science and the feminist goals of doing research *for* rather than *on* women.

Reflecting on fifteen years as an advocacy researcher, Ronnie J. Steinberg assesses problems that have plagued policy-based researchers. Throughout her work on comparable worth and pay equity, she and others have utilized scientific credentials and social science methodologies as a "power resource" to further policy goals. Feminist advocacy research encompasses a variety of activities that bring scholarship to bear on social issues explicitly for the purpose of achieving change in power relations and in structural arrangements—in this case, on behalf of women. According to Steinberg, "for research to serve as a power resource, it is necessary that it be impeccably designed and carried out, given the ease with which any scientific study can be pulled apart by those of other ideological persuasions." The need for legitimacy in the face of hostile adversaries adds pressure on advocacy researchers to adopt methodological tools common in conventional social science. "While feminist advocacy researchers are critical of science and feel all too strongly the limitations of its claims to objectivity and universal truth at a practical level, we use [conventional] methods because they legitimate our expertise and because they legitimate the findings we introduce into the policy arena."

Steinberg and Spalter-Roth and Hartmann acknowledge that advocacy researchers purposively adopt conventional social science methodologies characteristic of academic research, but distinguish between conventional and advocacy research methods. Advocacy research differs in several aspects, as enumerated by Steinberg: in the context in which the research is conducted; in the need for the researcher to establish scientific credibility, in that, as an advocate, she is not assumed to be an objective scientist seeking one uncontested truth; and in the amount of control that the researcher can exercise in the design of the research and in the selection of the indicators. Steinberg concludes that the unique and combined characteristics of advocacy research make it more difficult to do effectively than conventional social science research.

Shifting the focus to the theoretical bases of coalition building, Nancy C. M. Hartsock discusses the challenge of postmodernism to feminism. In a political excavation of postmodernist thought (particularly of Richard Rorty and Michel Foucault), Hartsock reads postmodernism as the "destabilized voice of the powerful being forced to come to terms with the voice of the disenfranchised." In her chapter she argues for the utility, validity, and even necessity of political theory in developing feminist liberatory politics. It would be folly to follow post-structural-

ist's injunction for the abandonment of theory or the death of the subject. Those on the margins need to understand the world systematically in order to change it (Hartsock 1989:7). Feminist politics today should involve building new coalitions and strategic alliances; and in any effort at coalition building or alliance formation, close attention must be given to the specific situations (identities) of each group as defined by axes of gender, race, class, and sexuality. As Hartsock summarizes in this volume: "Recognizing our experiences in the lives of others can strengthen our resolve to do the difficult political work of coalition building."

Activist researchers appear to be more self-conscious about the impact that the choice of methods have on their intended audiences and try to address different, and sometimes conflicting, audiences, such as academics, policy-makers, and grass-roots groups. Cancian, Smith, and Acker, Barry, and Esseveld discuss how academic audiences and academic structures, including career building in academic women's studies, affect our work, sometimes in cooptive and depoliticizing ways. Spalter-Roth and Hartmann and Steinberg argue for the legitimacy of numbers and scientific rhetoric and the dilemmas this poses for altering research to speak to the experiences of the less powerful and to appeal to and serve other, also politically important audiences instead of just policy-makers. Grass-roots activists are emphasized by Cancian, Taylor and Rupp, Hondagneu-Sotelo, and Naples and Clark, while Hartsock uses a more direct political-organizing and theoretical focus.

FROM SINGULARITY TO PLURALITY

A summary of feminist theories and methods suggests that the terrain has moved from singularity to plurality.[6] Feminists no longer argue for a single procedure that produces one true story. Similarly, the privileging of experience has given way to accounts premised on different feminisms and on differences between women.[7] This current plurality of feminisms was most notably "motivated by the enormous and continuing political impact of black women's critique of the racist and ethnocentric assumptions of white feminists" (Barrett and Phillips 1992:4; see also Collins 1990). Invoking the abstraction "woman" tended to privilege gender over other oppressions and failed to represent adequately the diversity of women's lived experiences, legacies, and possibilities (Spelman 1988). Most feminists now acknowledge the importance of difference(s); at stake is whether or not some differences matter more than others (di Stefano 1990; Bordo 1990, 1992; Hartsock, this volume) and how various axes of difference relate to each other (Fraser and Nicholson 1990).

This focus on diversity and difference has led many (Harding 1989; Collins 1990; as well as many in this volume) to question whether a feminist standpoint or even a hierarchy of standpoints can adequately capture multiply configured positions and identities of women. For example, Acker, Barry, and Esseveld are "critical of theory developed from a universalistic male perspective, [and they would] counterpose to this a women's perspective that also is universal." In their afterword, they point out that their earlier formulation assumed a universal, mostly heterosexual, woman's standpoint. Any new research would recognize the diversity of women's experiences.

Feminists have proposed more contingent and less deterministic theories and concepts to ground a critique of phallocratic, racist, homophobic, and capitalist institutions. The concept of "situated knowledges," developed by Donna Haraway (1988) and used by both Hartsock and Cancian, offers a way to incorporate diversity of women's lived experiences. Situated knowledges imply qualities of multiplicity, are locatable in time and space and particular cultures, are embodied in specific ways, and operate as social and collective points of view. As an aspect of being situated, these knowledges represent a response to and an expression of a specific embodiment.

By extension Cancian draws the conclusion that a partial and situated science implies limits to the achievement of science. Despite these limits, she argues that feminist researchers can build minimal standards of science. This would include a commitment to good evidence and a consideration of alternatives—i.e., the openness to debate, allowing for the play of competing explanations. She suggests feminist standards of evidence, starting with what's useful in positivism, while recognizing that positivism favors the dominant.

Gorelick's notion of a "complex of many determinations" opposes either "academic feminist pluralism" or a fragmentary science and a simple hierarchy of standpoints: "A methodology based on a 'complex of many determinations' implies a cumulative social science that is not merely additive. The visions of each subgroup of women must refocus or revision the knowledge of all."

Harding (1989:22) advocates a "perverse" research stance that constantly seeks to reveal and examine the privileged identities and the unequal power relations that facilitate the collusion of research in the perpetuation of conditions of domination, oppression, and privilege and corrode the interpersonal dynamics of research relationships. Feminist researchers who seek relations of mutuality must continually invent

"monstrous" identities out of step with the conventions and comforts of acceptable selves; it is a painful process that involves an ongoing struggle against social and psychic complacency and comfort (Harding 1989:27). Research should not just reproduce experience but should develop procedures that allow for the production of contrary readings, which can play an active role in social debate.

Feminists' contingent (partial) view of science emphasizes multiplicity and diversity and criticizes deterministic accounts as well as accounts premised on indeterminacy. Concepts such as situated knowledges oppose, on the one hand, modernists' uncritical faith in reason and rationality and, on the other hand, postmodernists' rejection of the possibility of knowledge. It is neither a call for the abandonment of theory nor for a ceding of the possibility of knowing the Other.[8] To reject a universalizing and totalizing voice, that of the transcendental scientist, does not necessitate a displacement or negation of theory. By using concepts like situated knowledges, we can begin to counter essentialism inherent to standpoint theories, to recognize that the knowledge we claim is conditioned by the locations we occupy (Hartsock 1987a:32), to take as reference points positions other than white, industrialized world, and heterosexuality, and to allow for multiple and shifting perspectives that enable us to learn about ourselves from the experiences and knowledges of others (Martin and Mohanty 1986).

RESEARCH AS POLITICAL PRACTICE: PROBLEMS AND FUTURE POSSIBILITIES

> How we can use our educational apparatuses and institutions to make social change—how we can reinvigorate "our capacity as agents to act as well as to know otherwise, to intervene in the world as well as the academy, to have an effect."
>
> —Gayle Greene

This book highlights research as political practice, examining both the possibilities and contradictions posed by feminist research at the conjuncture of a third wave of feminism. Research is inherently political, structured in hierarchies of power among researchers, between sponsors of research and researchers, and between researchers and the subjects of their research (Bell and Roberts 1984). These political relations present feminist researchers with an array of counterforces to the development of liberatory research practices, including the insinuation of relations of control and dependency, the material privileges of researchers, and the influence of institutional interests on research conduct.

First, the reinscription of the researcher into a position of power and privilege opens possibilities for exploitation that subvert empowerment and mutuality. While there may be an effort toward the collaborative, reciprocal quest for knowledge, Stacey cautions that the research project ultimately is that of the researcher—it is the researcher who narrates and "authors" the final text.

Second, writing is always an act of translation rather than simply a transcription of women's words. It is neither desirable nor possible to simply represent the "voices" of women. Women's voices are always mediated through the filter of the researcher and conventions of language. Ultimately the researcher must objectify the experience of the researched and must translate that experience into more abstract and general terms if an analysis that links the individual to processes outside her immediate social world is to be achieved.

Third, the researcher intervenes into a system of power relations that she is free to leave upon completion of a research project. By disengaging, the researcher leaves the subjects on their own to negotiate the power dynamics they mutually have uncovered and often abdicates responsibility for the political implications of the research results.

Fourth, the interests of the academic researcher may conflict with those of the community. In the process of doing research, a researcher may come to learn what Steinberg has called "guilty knowledge," any information that potentially compromises an individual's or a group's interest. Stacey faced the decision of whether or not to reveal the sexual orientation of one of her respondents. To "out" a "closeted" lesbian would have had political repercussions for the woman, yet to suppress the knowledge about sexual orientation would distort Stacey's research. Do we have an obligation to disclose information uncovered during the process of research even if it could damage community efforts toward social change? Does failure to report guilty knowledge violate tenets of good research practice? The authors offer different strategies for dealing with these dilemmas and contradictions.

Research involves power relations and the conduct of research is embedded in the hierarchies and constraints of academic life. Doing research in academic institutions raises difficult questions: How can the material privileges rooted in academic positions be used in the service of oppressed women rather than in the interests that such privileged academic positions represent, i.e., those of the academy and the powerful groups in society that support it? How can disciplinary protocol and the conventional standards of social science research be confronted in ways that assert rather than accede the legitimacy of liberatory research practices? How can universities be sites of struggles while serv-

ing as sites for the reproduction of power and privilege (Bannerji et al. 1991:5)?

These constraints cannot be overcome by simply altering the methodology or applying a particular theory. Methodological change ultimately depends on changing the structure of the university. "The task of dismantling the master's house with the master's tools, always problematic, has become more vexed the more institutionalized we've become; our dilemma is now, . . . how to dismantle the masters' houses while we are trying to get computers for their offices we have set up inside them" (Bammer 1991 as quoted in Greene 1992:25). Possibilities do exist for reforming parts of the university by taking advantage of feminist spaces such as feminist journals or women's studies programs. These programs create spaces in which debates over power and the production of knowledge can take place (Lather 1988:569). If, as Kelly (1988) suggests, one of the basic principles of feminist practice is to challenge social relations based on power, then feminist research must critically confront the power dynamics of academic expectations, conventions, and traditions in order to establish an alternative practice. Research itself can be a vehicle for consciousness-raising.

However, to remain cloistered behind the walls of the university, safely entrenched in the day-to-day battles that preoccupy us, will serve to attenuate feminism's base in broader communities of women and to sever feminism's connection to wider women's movements. Acker, Barry, and Esseveld sound a cautionary note in their observation that "as feminist scholarship feminism has become institutionalized within the academy, increasingly abstract theoretical concerns seem to have less and less to do with practical problems of women's everyday lives." Feminist researchers thus must renew and maintain links with various communities of women in order to ground their research in women's everyday lives.

None of the problems discussed in this book come with easy answers. The authors have raised questions and have provided answers to a wide range of vexing research problems. Hopefully, readers will come away with a renewed spirit to carry on feminist research both inside and outside of the university. Future generations of feminist researchers can reflect on our triumphs and defeats in order to fashion new feminist practices aimed at promoting social change.

NOTES

Many of the ideas for this chapter were developed in "Research for Women: Notes toward the Development of a Liberatory Research Project," coauthored with Patty Sotirin, and expanded upon in "Notes towards the Development of a Liberatory Research Project," in *Trade Unions and Social Research*, edited by Keith Forrester and Colin Thorne (Aldershot, England: Avebury, 1993).

1. Methodology is the study of actual techniques and practices used in the research process.

2. Harding states that "an epistemology is a theory of knowledge, it answers questions about who can be a 'knower' (can women?); what tests beliefs must pass in order to be legitimated as knowledge (only tests against men's experiences and observations?); what kinds of things can be known (can 'subjective truths' count as knowledge?), and so forth. . . . Feminists have argued that traditional epistemologies, whether intentionally or unintentionally, systematically exclude the possibility that women could be 'knowers' or agents of knowledge; they claim that the voice of science is a masculine one; that history is written from only the point of view of men (of the dominant class and race); that the subject of a traditional sociological sentence is always assumed to be a man" (1987:3).

3. Relativism assumes that all explanations are subjectively grounded and therefore have equal weight. When all accounts are equally valid, the search for "how it actually works" becomes meaningless (see Acker, Barry, and Esseveld in this volume).

4. Spender advocates naming as "the means whereby we attempt to order and structure the chaos and flux of existence which would otherwise be an undifferentiated mass. By assigning names, we impose a pattern of meaning which allows us to manipulate the world" (1987:163).

5. "The favored postmodern solution to the reflexive anthropological predicament has been to fully acknowledge the dialogic and discursive character of the ethnographic process and product and to deconstruct their own claims to ethnographic authority" (Stacey, this volume).

6. Barrett and Phillips (1992:4) find that early formulations in feminist theory instantiated a "modernist" impulse whose starting point assumed that one could specify a cause of women's oppression; feminists differed on what this cause might be, alternatively stressing either male control of women's fertility, a patriarchal system of inheritance, or capitalists' need for a docile labor force.

7. Multiplicity and diversity of approaches resist simple dichotomies such as emancipatory versus subordinative research. The idolization of experience as the beginning and end of research is seen as a romantic fallacy that denies the specificity of social and scientific practices. These ideas benefited from the comments by Erik Kats and Folks Galstra in personal correspondence.

8. Symbolic interactionists and postmodernists converge in their arguments when they contend that we can never truly know the Other and refuse to speak for or about the Other.

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PART I

Doing Feminist Research: Dilemmas and Contradictions

CHAPTER 1

SHERRY GORELICK

Contradictions of Feminist Methodology

Feminist methodology grows out of an important qualitative leap in the feminist critique of the social sciences: the leap from a critique of the invisibility of women, both as objects of study and as social scientists, to the critique of the method and purpose of social science itself. This is the leap from a sociology *about* women to a sociology *for* women, as Dorothy Smith (1974) put it. Smith argued that male-dominated science objectifies, but something very fundamental happens when both the knower and the known are women. When the pronoun applied to the knower is *she*, rather than the *seemingly* impersonal *he*, the knower is changed immediately from The Scientist to a person with a gender. And when this scientist with a female personal pronoun studies women, she is apt to feel a different relationship with her subjects, because she is subject to finding herself mirrored in them, a fact with revolutionary implications for the relationships among observer and observed, theory and experience, science, politics, race, and class. In the past two decades, however, we have learned that this mirroring process has its own limits, reflecting divisions based on race, class, and other forms of oppression and requiring that we push the methodological revolution even further.

FEMINIST CRITIQUES OF THE DOMINANT METHODOLOGICAL PARADIGMS

Feminist methodological critiques have been made on several interrelated levels: philosophical, moral, and practical. The *philosophical* level has involved a critique of positivism: the pretense of value-free science and the presumption of objectivity conceived of as a set of procedures or an achievement, rather than a process (Gorelick 1989; Keller 1980).

On a *moral* level, feminists have criticized the objectification of subjects and their exploitation by researchers using the dominant methods.[1] Objectification rests on positing a radical difference between the roles of scientist and subject in which, in the most extreme positivist approaches, studying human beings is, in principle, no different from studying things. George Lundberg wrote that there is, for the social scientist, no difference between a paper flying before the wind and a man flying before an angry crowd (1963:45–46).

In contrast to this reduction of human beings to social facts, feminism, building on the interpretative approach in sociology, emphasizes the human agency and subjectivity of the people studied. The production of science is not an operation (or indeed an autopsy); it is a relationship. That relationship is exploitative when a researcher studies people for the benefit of the researcher's career or of the sponsors of the research without regard for any positive or negative effect on the people being studied. Feminists have also criticized the entire structure of inequality in the conduct of research, especially the hierarchical structure of large-scale research projects.

This moral critique of research hierarchy is directly related to feminists' *practical* critique of the dominant methodologies. The opposed interests of researcher and researched in the dominant, hierarchical methodological approaches lead to distortions, lying, even farcical results (Gorz 1972; Klein 1983:91). A subject population does not tell the truth to those in power. Not only that, large-scale research projects generate *two* subject populations: the people being studied and the people doing the routine labor involved in studying them. In "hired hand research" (Roth 1966; Reinharz 1983:171), low-level research staff may find myriad ways of cutting short their work, constituting a "labor problem" in the truth factory. Between the creative dissimulation of the objectified research subjects and the subversive creativity of the research workers, each responding to their different modes of exploitation, the results are often not science, but science fiction.

Two feminist methodological alternatives emerged: the Marxist-feminist and the experiential-inductionist, often affiliated with ethnomethodological and interactionist scholarly traditions and with the new social history emphasizing "the view from the bottom." Some feminists have attempted to integrate these two approaches (e.g., Acker, Barry, and Esseveld 1983; Smith 1987). After reviewing this literature, I propose to explore some contradictions involved in the development of a Marxist-feminist-interactionist methodology. These contradictions concern the role of theory and "false consciousness" in the relationship between researcher and participant; the implications of race, class, and other inequalities for feminist research; and the implications of the social biography of the researcher for the development of a liberatory feminist standpoint. I argue that the feminist inductionists fail to take account of the hidden structure of oppression (the research participant is not omniscient) and the hidden relations of oppression (the participant may be ignorant of her relative privilege over and difference from other women). Women's oppression is a complex of many contradictions and requires a new standpoint-based methodology created by researchers and participants of diverse race, class, and other oppressed groups, refocusing and revisioning knowledge based on theory, action, and experience.

EARLY FEMINIST METHODOLOGICAL MANIFESTOS

In 1978 Maria Mies set forth methodological guidelines for feminist research that proposed that the hypocritical "postulate of *value-free research*, of neutrality and indifference toward the research objects, has to be replaced by *conscious partiality*" toward the oppressed, engagement in their struggles for change, and the creation of a form of research that fosters *conscientization* of both the researcher and the researched (1983:122–26). The guidelines set the dominant formula for research practice on its head. The dogmas of positivism—its hands-off approach, its clinical fastidiousness about mutual contamination, its insistence that research must precede change, that indeed change is the business of politicians and not scientists—were overturned. For feminist methodologists, as for the Marxist and interpretive sociologists on whose work they built, social science is much more profound than the mere collection of "facts." Said Mies: "Most empirical research on women has concentrated so far on the study of superficial or surface phenomena such as women's attitudes toward housework, career, . . . etc. Such attitude or opinion surveys give very little information about women's true consciousness. Only when there is a rupture in the 'normal' life of a woman, i.e. a crisis such as divorce, end of a relationship etc., is there a chance for her to become conscious of her true conditions" (1983:125). As Judith A. Cook and Mary Margaret Fonow put it much later: "Feminism is a vision of freedom as future intention and this vision must indicate which facts from the present are necessary knowledge for liberation. Description without an eye for transformation is inherently conservative and portrays the subject as acted-upon rather than as an actor or potential actor" (1986:12). The implication is quite clear: Merely collecting descriptive statistics or experiential data about women does not constitute feminist research. Feminist research must

be part of a process by which women's oppression is not only described, but challenged. Similarly, beginning in 1974, Dorothy Smith argued that sociology as currently practiced expressed unreflectively the distortions of a male, ruling-class standpoint. She urged that instead research must be done "from the standpoint of women," taking "the everyday world as problematic" and beginning from women's ordinary, everyday experience (1974, 1979). Smith specifically cautioned against *confining* the inquiry to the world of experience (1974:12, 1979:174). Some later feminist methodologists, however, have argued for a social science that is "inductive rather than deductive," that "focuses on processes rather than structures," and that is "interested in generating concepts *in vivo,* in the field itself" rather than using "predefined concepts" (Reinharz 1983:172, 168). According to Shulamit Reinharz, whereas the validity criteria of "conventional or patriarchal" science are "proof, evidence, statistical significance [and] replicability," the validity criteria of feminist science are "completeness, plausibility, . . . understanding, [and] responsiveness to readers' or subjects' experience; [the] study cannot, however, be replicated" (1983:171). The role of the researcher is to "give voice" to hitherto silenced groups and facilitate their own discoveries (Kasper 1986).

GIVING VOICE IS NOT ENOUGH: THE LIMITS OF FEMINIST EMPIRICISM

"Giving voice" was a progressive development in the history of feminist theory. It went beyond criticism of the use of "mainstream" social science as a tool of oppression and began the quest for a liberatory social science. But the more radically empiricist forms of the feminist critique have their own limitations that threaten to encapsulate feminist social science within each specific milieu being studied and even preclude understanding the very milieu being examined. For example, use of such techniques as interviews, participant observation, and oral history helps to describe the world as perceived by the persons studied, but it may remain confined within their perceptions and thus not be able to provide them with much that they do not already know.

The agonizing and cumulative process of feminist discovery over the years has revealed how much of sexism is deeply internalized and therefore buried beneath the conscious level (MacKinnon 1987). Consciousness-raising as a technique of research and political action may enable women to "give voice" to knowledge that they did not know they had. But this knowledge, too, is limited to what each group of women is able to discover anew. Maria Mies's emphasis on the impor-

tance of crises or ruptures in the pattern of normality, so that the pathology of the normal may be perceived, is of crucial importance. Even so, giving voice is not enough. Women know much and may learn more about their own pain, but some of the underlying causes of that pain may be very well hidden from them (cf. Maguire 1987:37).

THE HIDDEN DETERMINANTS OF OPPRESSION

In *Capital,* Marx showed that the most fundamental social relations occur "behind the backs" of the actors. That is, much of the underlying structure of oppression is hidden, not only by means of ideology, but also by means of a contradictory daily life. Appearance contradicts reality: Workers feel dependent on capitalists for employment and wages, yet in reality they produce daily, in surplus value, the wages with which they are paid and the wealth that permits their continued subjugation. The "developing" world appears dependent on the "industrializing" world for technology and investment, yet in reality the imperial world is dependent on the colonized world for raw materials, markets, and cheap labor. Wives appear to be dependent on husbands for support and protection, yet in reality it is husbands that are dependent on wives for their unpaid labor, emotion management, and much else. Suniti Namjoshi's amusing but chilling fable "The Monkey and the Crocodiles" (1981:26) shows women's need for male physical protection to be little more than a protection racket. In reality each of these dependencies is substantially reversed, yet none of these realities is immediately apparent to those most oppressed by them.

In "Women's Perspective as a Radical Critique of Sociology," Dorothy Smith described the dependence of professional and managerial men practicing "the abstracted conceptual mode of ruling" on the concrete invisible labor of women as computer specialists, secretaries, administrative assistants, wives, and so on. Their own social determinants are invisible to the men (1974:10), yet the importance of their own role may be invisible to the women themselves, for two reasons. First, the dominant ideology obscures their role: "ideas and social forms of consciousness may originate outside experience, coming from an external source and becoming a forced set of categories into which we must stuff the awkward and resistant actualities of our worlds" (1987:55). Second, women's vision of their own oppression is masked by the development of corporate capitalism, in which local events are determined by social forces far from the site (1979:161). In short, although oppression can *only* be understood from the standpoint and experience of the oppressed, the very organization of the everyday

world of oppression in modern capitalism obscures the structure of oppression: "The everyday world is not fully understandable within its own scope. It is organized by social relations not fully apparent in it nor contained in it" (1979:176; 1987:92).

In contrast to the reified conceptualizations of social structure produced by functionalist (and radical functionalist) social scientists (Gorelick 1977), the feminist concept of social relations does not connote a rigid and reified social structure impervious to human action. Rather, social relations are relatively enduring relationships among people, relationships that embody contradiction and change (Acker, Barry, and Esseveld 1983:425). Some of the methodological implications of the structure of social relations were developed in Nancy Hartsock's pivotal "The Feminist Standpoint: Developing the Ground for a Specifically Feminist Historical Materialism" (1984). According to Hartsock, "if material life is structured in fundamentally opposing ways for two different groups, . . . the vision of each will represent an inverse of the other, and in systems of domination the vision available to the rulers will be both partial and perverse" (232). It is perverse because it enforces and justifies oppression, even including murder. Both the partiality and perversity of this view undermine the claims of objectivity made by those who practice establishment science. Yet although the standpoint of the ruling group is perverse and self-serving, it cannot be dismissed as simply false, because "the vision of the ruling class (or gender) structures the material relations in which all parties are forced to participate" (232). If the ruling class and gender have the power to structure ideology, reality, and perception, then everyday material reality will obscure the causes of oppression. "In consequence, the vision available to the oppressed group must be struggled for and represents an achievement which requires both science to see beneath the surface of the social relations in which all are forced to participate and the education which can only grow from struggle to change those relations" (232).

To some extent these hidden relationships can be discovered (and are discovered) by the oppressed themselves as they begin to interact, collectivize their experience (for example, through consciousness-raising), and start to change their situation. For the very act of trying to change the structure tends to bring the nature of the system of oppression into bolder relief (Mies 1983). To some extent, the hidden structure of oppression must be discovered anew by each group of women because of the great educative power of direct experience and because each concrete situation of oppression has its own historical specificity, its own specific lessons.

Direct experience has its limitations, however. Besides the lack of cumulative knowledge, there are some hidden aspects of oppression that no amount of direct struggle will reveal. In view of these limitations, the researcher may play a role that is quite different from that of the participants. For example, in their study of industrial homework in Mexico City, Lourdes Benería and Martha Roldán (1987) not only interviewed homeworkers but they also traced the subcontracting links from those homeworkers on up through major corporations. Decisions of managers to employ women rather than men, and to employ them directly in factories or to subcontract out the work, had a major impact on the lives of the women who were assembling parts, polishing plastics, sorting pieces, and finishing textiles in their homes. Yet the women themselves would never have been privy to these decisions if Benería and Roldán had not had the institutional resources giving them access to these managers and the theory leading them to seek that access. Because the structure of oppression is often hidden, a feminist standpoint "is achieved rather than obvious, a mediated rather than immediate understanding" (Hartsock 1984:234).

Hartsock was not proposing an abstract, ivory-tower science; she did insist, however, on the necessity of scientific analysis. It would be extremely difficult, if not impossible, to derive that analysis purely inductively. In her study of lacemakers in India, Mies (1982:2–3) criticized the androcentric conceptualizations of Marxism: "Labor," "productive labor," and other concepts have been defined with inappropriate biologistic assumptions. She forcefully rejected abandoning these concepts altogether, however. It would be foolish, she said, not to reclaim, reform, and use them. The concepts—which are, after all, the essential links between theory and method—must be redefined "from below." The social scientist can, in collaboration with research participants, provide, question, and test theoretical understandings that reveal the hidden underlying structure of oppression.

FALSE CONSCIOUSNESS AND THE SOCIAL RELATIONS OF RESEARCH

The notion of hidden determinants—the determination of women's oppression by factors beyond their immediate experience—raises the issue of false consciousness, an idea that exposes some of the contradictions in Marxism. If social relations occur "behind the backs of the actors," how can the researcher know them, unless she claims a source of knowledge or understanding beyond that of her respondents? If she makes that claim, doesn't she run the risk of elitism? But if she does

not attempt to uncover social relations and structures of oppression that may be hidden from her respondents' view, is she not limiting her contribution to them and to feminist science and political practice? If we reject the solipsism of feminist empiricism, from what standpoint does the scientist know the "reality" masked by appearances? If structural conflict produces opposing worldviews, then the social biography of Marxist theorists becomes problematic. In conceptualizing the false consciousness of a group, its imperfect comprehension of its own interests, what is the theorist's relation to the multifaceted structures of oppression?

In her excellent study *Life and Health in Three Palestinian Villages*, Rita Giacaman describes how her team of health scientists discovered their own class and urban bias and the limitations of their attempt to apply an unalloyed Marxist-feminist analysis:

> The women interviewed had their own agendas, and we were incessantly grilled with such questions as "How many children do you have?" "Why aren't you married?" "Where are your parents?" As we were being interviewed we would try to slip in a question or two in the midst of the confusion. The experience slowly led us away from the stereotyped images we had of "poor, weak and obedient" peasant women. . . . We had begun by looking at the women condescendingly: We were there to help them, to "raise their consciousness." But these women did not necessarily need their consciousness raised. They knew what was going on and . . . how to solve their problems. What they needed was the power and authority to change their lives. (1988:37)

The concept of false consciousness has been passionately criticized by many feminists, most notably Liz Stanley and Sue Wise:

> We reject the idea that scientists, or feminists, can become experts in other people's lives. . . . Feminism's present renaissance has come about precisely because many women have rejected other people's (men's) interpretations of our lives. Feminism insists that women should define and interpret our own experiences. . . . Feminists must attempt to reject the scientist/person dichotomy and, in doing so, must endeavor to dismantle the power relationship which exists between researcher and researched. (1983a:194–95)

Stanley and Wise took pains to state that they were "in no way opposed to theorizing as such": Instead they espoused symbolic interactionism because it "adopts a non-deterministic attitude towards social life and interaction, . . . [and] insists that structures are to be found *within* [the] processes [of interaction] (1983a:201–2). They also embraced ethnomethodology because it "accords well with the egalitarian ethos of feminism" (204).

The Struggle for Egalitarian Feminist Methodologies

If it is true that women's oppression is created entirely within the process of social interaction, then women can come to understand their oppression themselves, through ethnomethodological and symbolic interactionist techniques. The researcher's role would be limited to facilitating that process of discovery "from the ground up." In *Street Corner Society*, one of the original, paradigm-founding exemplars of symbolic interactionism, William Foote Whyte (1943) showed that the "corner boys" in a Boston slum created, through their patterns of interaction, the social and symbolic hierarchies in their gangs, their religion, and their politics. The structure he analyzed could have been made visible by the "members," since to a great extent, it was already known by them. But why were thirty-year-old men hanging out like "boys" on street corners? Because it was the Great Depression and they were unemployed. The depression was certainly not a result of their patterns of interaction. Looking at their own patterns of interaction, they would only have been able to blame themselves, each other, and the people they knew.

If women make their own history, they can uncover the roots of their oppression in the patterns of their own making. But if women "make their own history, but not under conditions of their own choosing" (Marx 1963;[2] Personal Narratives Group 1989:13), then women must be able to examine those conditions as well as their own patterns of interaction and understanding. While it may be more egalitarian to reject the notion of outside determination, that does not stop the president of Ingersoll Rand from making decisions in his New Jersey office that affect the work lives, choices, and susceptibility to cancer of women in Singapore (Fuentes and Ehrenreich 1984). Nor does it prevent those decisions from being influenced by the investment climate in Brazil. Understanding the implications for Singapore women of those international investment patterns and capital flows, understanding the location of Mexican homeworkers in the labor process requires theories that generalize from realities outside of the immediate experiential frame of the Singapore and Mexican women, theories more derived from Marxist-feminism than ethnomethodology (Benería and Roldán 1987).

The difficulty with the concept of false consciousness is not, in my view, that it asserts that people may have an imperfect understanding

of their own conditions. Nor does the solution lie in asserting that their understanding is perfectly valid, as if the nature of the world were merely a matter of opinion (cf. Acker, Barry, and Esseveld 1983; Fisher 1984, on relativism). The difficulty with the concept of false consciousness lies in the implications that there is a true consciousness that is known and complete and that the researcher/activist knows it and the participant does not.

Joan Acker, Kate Barry, and Joke Esseveld began their research on women going out to work at midlife with a commitment to egalitarian relations; they discovered that the women demanded a more complex understanding of their respective roles:

> What they wanted, they said, was more of our own sociological analysis. They wanted us, the researchers, to interpret their experience to them. . . . If we were to fulfill the emancipatory aim for the people we were studying, we had to go beyond the faithful representation of their experience, beyond "letting them talk for themselves" and put those experiences into the theoretical framework with which we started the study, a framework that links women's oppression to the structure of Western capitalist society. (1983:429–30)

Exploring the "incompatibilities between various components of our feminist approach to social research," they conclude with a commitment to "reconstructing women's experience in a way that accounts for both their and our explanations of that experience and the relation between the two" (430). This reconstruction must include both the active voice of the subject and the researchers' own dialectical analysis (431). Similarly, the Personal Narratives Group concluded that the social context of the women they studied "had to be considered from the standpoint of the subject of the personal narrative, as well as from the standpoint of the interpreter's analysis of a particular cultural and social system" (1989:12).

Paradoxically, the ideology of complete equality between researcher and researched reintroduced the notion of value-free science in a new guise because it obscured the differences of their roles and the power complexities of their relationship (Stacey 1988; Personal Narratives Group 1989:13). The researcher is not a mere vessel of consciousness-raising or social action any more than a psychotherapist is merely a neutral facilitator of personal growth. The newer notion of research as a sort of dialogue or contrapuntal duet, while recognizing that the viewpoints of researcher and participants are not necessarily compatible (Personal Narratives Group 1989:264), remains somewhat problematic, however, as long as the vast majority of researchers (or "interpreters") remain predominantly white and privileged (Riessman 1987).

To her interaction with the participants, the researcher brings her social location, culture, motivations, limitations, ignorances, skills, education, resources, familiarity with theory and methodology, trained incapacities of socialization in dominant institutions, and an outside perspective that may be useful as well as troublesome (Acker, Barry, and Esseveld 1983; Riessman 1987; Gorelick 1989; Stanley and Wise 1983b). The researcher is transformed in the process of research—influenced and taught by her respondent-participants as she influences them. Theory and practice emerge from their interaction. The researcher is ultimately responsible for the final version, however. She cannot avoid this responsibility (Acker, Barry, and Esseveld 1983:428–29; Benería and Roldán 1987:27–28; Gorelick 1989:352; Mbilinyi 1989:224–25; Sacks 1989; Stacey 1988).

Stanley and Wise's critique of the researcher-respondent relationship was similar to the critiques many of us made, during the 1960s and 1970s, of the elitism involved in teacher-student, psychologist-patient relationships. In no way do I wish to associate myself with the reactionary arrogance, the suffocating smugness, with which social pundits of the eighties look back (and down) on the radical and creative spirit of the sixties and seventies. We have learned from our experience of living and struggling in a backlash era, however, that these relationships are a set of contradictory interactions, and our successes and limitations in resolving them are historically determined. Teachers cannot *alone* undermine their own oppressive power over students, nor can researchers in relation to respondents. Even the possibility of their working together to overcome these oppressive relations is shaped by outside forces (Sacks 1989).

Even in the worst of times, however, we must not simply succumb to the institutional forces recreating hierarchy. We must always push at the margins, push at the limits, push at ourselves. In the worst of times, we must be most on guard against the hierarch within ourselves. But we must, collectively, try to understand the times and how they frame our possibilities of transcendence.

THE HIDDEN RELATIONS OF OPPRESSION

A purely inductive research project such as that advocated by the feminist empiricists can generate only those progressive understandings available to the women studied. If the participants are white, heterosexual, middle class, or North American, they are likely to generate a stand-

point that is on the wrong side of racial, sexual, class, and imperial oppression. If they are Christian, they may not be able to find within their milieu the basis for understanding their own anti-Semitism. Hartsock's observation regarding the ruling class and gender applies here: "There are some perspectives on society from which, however well-intentioned one may be, the real relations of humans with each other and with the natural world are not visible" (1984:232). If generalizations are not to be made from one field situation to another, none of these groups of women can learn from each other and all must remain mired in the ignorance of their various privileges.

Combining interviews, participant observation, and an extensive historical analysis of domestic work, Judith Rollins found that maids and their employers had very different views of themselves, each other, domestic work, wages, and their relationship:

> Domestics were able to describe in precise detail the personalities, habits, moods, and tastes of the women they had worked for. (The descriptions employers gave were, by comparison, less complex and insightful—not, it seemed to me, because employers were any less capable of analyzing personalities but rather because they had less need to study the nuances of their domestics.) . . . The domestics I interviewed knew the importance of knowledge of the powerful to those without power. (1985:213–16)

Rollins, a Black sociologist doing participant observation as a domestic, was able to reveal contradictions her white respondents could not see: "The middle-class women I interviewed were not demanding that their husbands play a greater role in housekeeping; they accepted the fact that responsibility for domestic maintenance was theirs, and they solved the problem of their dual responsibilities by hiring other women to assist" (104). Her work reveals the white employer as caught in a contradictory location: oppressed as a woman, oppressing another woman as her employee, under the particular conditions of race, gender, and political economy in the late twentieth century (cf. Fisher 1988:223–24). It is for this reason that a methodology based purely on induction, and on the conclusions that the participants are able to generate for themselves, cannot even help them to understand their own milieu completely. As Stanley and Wise put it, discussing Frye (1983), "maleness, heterosexuality and whiteness all 'work' . . . by being states of *unawareness* in which the key privilege of the privileged group is not to notice that they are such" (1990:33). "Feelings are useless with-out facts," said Adrienne Rich, and "all privilege is ignorant at the core" (1986:226).

To understand both the domestics and their employers, therefore, and for them to understand themselves, Rollins needed both perspectives, but they were not equal. The maids' perspective had primacy. Consistent with the insights of all of the feminist methodologists, *theirs* is the "view from below." This idea goes beyond "different perspectives" and "difference" to the nature of oppression as a multifaceted structure of unequal social relations.

In this sense, *Interpreting Women's Lives* (Personal Narratives Group 1989) is a way station along the road "from feminist empiricism to feminist standpoint epistemologies" (Harding 1986:136). Its authors recognize the necessity and inevitability of interpretation and theory and the likelihood that the perspectives and motivations of "narrator" and "interpreter" may differ (4–6). Yet the Personal Narratives Group does not adequately analyze the consequences of the interpreter's social biography (her race, class, nationality, sexuality) for her interpretation. Although the authors are excruciatingly, fascinatingly honest about their difficulties, ideological commitments, errors, and contradictions, they generally do not mention their own social characteristics, even when exploring race and class differences among their narrators. As Acker, Barry, and Esseveld pointed out in 1983, "the interpretation must locate the researcher in the social structure and also provide a reconstruction of the social relations that produce the research itself" (431; see also Riessman 1987).

The Personal Narrative Group's solution to the problem of different perspectives (between narrator and interpreter and among women of different race, class, and nationality) is limited to invoking the necessity of substituting "truths" for "Truth," and urging "a reconstruction of knowledge that admits the fact and value of difference into its definition" (1989:263). Understanding the necessity and problematics of interpretation, they have moved from "giving voice" to hearing voices. That is, they dissolve the structure of inequality into a cognitive pluralism supplanting standpoint (cf. Stanley 1987). To solve the problem of different conditions of oppression by focusing on different "truths," however, is to equalize what is not equal, to spread a patina of equivalence over brutal realities and their inverse insights.

STANDPOINT AND MOVEMENT: A COMPLEX OF MANY DETERMINATIONS

In *Feminism and Methodology,* Sandra Harding considers whether the critiques of science by both the "postmodernists" and by women of color mean that no unitary science is possible:

> For instance, Bell Hooks insists that what makes feminism possible is not that women share certain kinds of experiences, for women's experiences of patriarchal oppression differ by race, class, and culture. Instead, feminism names the fact that women can federate around their common resistance to all the different forms of male domination. Thus there could not be "a" feminist standpoint as the generator of true stories about social life. There could, presumably, only be feminist oppositions, and criticisms of false stories. (1987:188; she is referring to hooks 1984)

I believe that this is a misreading of hooks and of the implications of the works by women of color for the creation of a feminist standpoint (in contrast see Fisher 1989; Hartsock 1987; Smith 1987:121–22, 134). Hooks did not call her book "Another Country" or "A Different Voice." She called it *Feminist Theory: From Margin to Center.* She argued that as a result of the dominance of feminism by relatively privileged women, "feminist theory lacks wholeness, lacks the broad analysis that could encompass a wide variety of human experiences" (1983:x). To create such an analysis, the perspectives of women of color must move to the center of feminist theory and the feminist movement. White feminists' definitions of feminism must be overturned by the view from below or from "the margin."

The notion that there must be "many stories," that is, a fragmentary science, is similar to men's assumptions that the study of gender is only about women's worlds. On the contrary, difference of condition does not mean absence of relationship. Black women's experiences are relevant not only to other Black women but also to understanding the situation of white women, and indeed of Black and white *men.* It is only because Black women empty bedpans that white men can run hospitals. It is only because Native American women are poor that ruling-class men and women are rich. It is only because Guatemalan peasant women are oppressed that North American businessmen have power. And it is not only lesbians but all women who are oppressed by the compulsory heterosexuality that lies at the heart of sexism (Rich 1986).

Theory making, therefore, cannot be ghettoized because reality does not come in separate boxes. We must uncover not only the different experiences of diverse groups of women but also the processes creating these differences. We must trace how these processes of oppression—racist, imperialist, class, national, religious, and sexual—are connected to each other and determine, in very different patterns, the lives of all and each of us.

Within a feminist approach, we need an analysis of racism from the standpoint of women of color, national oppression from the standpoint of oppressed minorities, Christian chauvinism from the standpoint of Jews and other ethnoreligious minorities, class from the standpoint of working-class women, and heterosexism from the standpoint of lesbians. All of these systems (or axes) of oppression intersect and implicate virtually everyone (at least in U.S. society), since everyone stands on one or the other side of these axes of oppression and privilege. Therefore, every piece of research must include an analysis of the specific social location of the women involved in the study with respect to these various systems of oppression.

Such an analysis requires that someone be able to step back and do that analysis, or facilitate its emergence among the participants, raising again the questions of the segregation of milieux, the social biography of researchers, the researcher-participant relationship, and so on. Ultimately, what we can build toward is an understanding of the "complex of many determinations" as a set of dynamic interrelations (Marx 1970:206).

The notion of a "complex of many determinations" goes beyond "academic feminist pluralism" (Stanley and Wise 1990:47), beyond the notion of a fragmentary science, and beyond a simple hierarchy of standpoints (Stanley and Wise 1990:28; Harding 1986). Rather, a methodology based on a "complex of many determinations" implies a cumulative social science that is not merely additive. The visions of each subgroup of women must refocus or revision the knowledge of all. The field is continually decomposed and reconceptualized at deeper and more complex levels of understanding (Smith 1987:215–16, 222–23), always giving primacy to the vision of the oppressed.

Such a science may imply an amazing goodwill transcending opposing interests, for example, between white employers and "their" maids. That is, the idea of such a cumulative social science may seem to ignore conflict. The conflicts are real, however. The problem of creating a women's social science encompassing the diverse consciousness and conditions of different women is similar to and related to the problem of creating a nonoppressive women's movement. Are there any material supports for unity? Are there at least creative contradictions to counter the differences in material interests? Can we begin to analyze our present situation as a "complex of many contradictions?" And will "we" all want to do so?

We have learned this much: The old top-down methods of politics and science will no longer do. To end the oppression of women we need a political movement and a social science that "gives voice" to wom-

en. But because of the multifaceted structure of oppression, giving voice is not enough. To understand the different milieux in which women experience their oppression and to trace their connections with each other, we need a social science produced by women of various social conditions (race, class, sexual preference, nationality, ethnicity), a social science that reveals the commonalities and structured conflicts of the hidden structures of oppression, both as they are felt and as they are obscured. The quest for such a science confronts and comprises a dynamic tension among the researcher and the researched, struggle and science, action, experience, method, and theory.

CONTRADICTIONS OF FEMINIST METHODOLOGY: A POSTSCRIPT

Unlike my earlier article "The Changer and the Changed: Methodological Reflections on Studying Jewish Feminists" (1989), this essay did not grow out of my own research. Rather, it was born of my strong reaction to reading what I call the "feminist empiricists." I originally called this essay "Giving Voice Is Not Enough." "Giving voice" is not enough because of the hidden determinants of oppression (the respondent may be unable to know the structure of her own situation) and the hidden relations of oppression (she may not be aware of her own implication in the oppression of others).

My current research focuses on Jewish feminists' responses to the Israeli-Palestinian conflict. In working with the transcripts of my interviews, I do not merely "give voice" to the views of my respondents. Dealing with as controversial an issue as the Israeli-Palestinian conflict and questions of Jewish identity, I realize that I cannot simply "give voice" to their views even if that were all that I wanted to do. After all, it is I who asked the questions, I who read the transcript, I who selected the materials to be placed in the text. It is when I am trying to be most faithful to their meaning, and particularly when I am trying to portray the political consciousness of women with whose politics I may disagree, that I am most painfully aware that simply "giving voice" is not so simple after all. It is fraught with interpretation.

Even if I simply published the transcripts verbatim, without editing, I would only be giving voice to those thoughts that the women were willing or able to express on the day of the interview. They might express themselves quite differently by the time their words are solidified into print.

But I do not simply reprint unedited transcripts. Their words, thoughts, and feelings are filtered through the selections that I, with

my own political views, my own (changing) convictions and contradictions, make. Nor do I think that simply giving my respondents voice would be enough of a contribution. I am attempting to analyze the way that Jewish feminists deal with being in a contradictory social location: the contradiction of being oppressed as women and as Jews (vis-à-vis anti-Semitism) while being in a structurally privileged position, at least by implication, with respect to the Palestinian question (and, of course, for most—but not all—North American Jews, with respect to race, although I am not studying this aspect currently). I am trying to analyze the interviews I held with them in historical perspective, in light of the historical events that have influenced their (and our) lives. In that sense too, I have the chutzpah to think that I may have something to say that is relevant and that comes from outside the immediate frame of the interview.

Yet although in my essay in this book I argue that the researcher *may* discover determinants of which the respondent is unaware, she may not always do so. In my current research, as I have read and reread my respondents' testimonies, over the years of this study, I have continued to see those testimonies differently, as I myself change, partly under their influence and partly under the influence of other events and forces in my own life, including historical events and outside circumstances. I am impressed more by what I learned from my respondents—not only about their own consciousness but about anti-Semitism and the Israeli-Palestinian conflict—than by what I have to offer them in insights and "hidden determinants."

That might be because I am myself very much like them and subject to some of the same social forces, some of the same distortions and limitations. There are hidden determinants in my life also, and I am both the worst and the best person to uncover them. Worst, because structural barriers mislead me too (in the way that Hartsock describes); best because only I am in my skin. I am closest to my own experience.

In that sense, being in the same "critical plane" as my respondents has both advantages and disadvantages. I may be better placed than another student to achieve an empathic understanding (*Verstehen*) of their situation and their contradictions; I may be less able to perceive our common "hidden determinants" and "hidden relations" of oppression, our common contradictions.

When Smith's feminist sociologist found herself mirrored in her respondents, social science took a great leap forward. She moved social science from a false universalism to a discovery of the particular characteristics and limitations of white, male scholars. Then, prodded

by women of color, lesbians, and others,[3] feminist scholars discovered the solipsism of the mirror. White, "middle-class" feminist scholars had mirrored *themselves* only *too* well—and yet still with a distorted lens.

Does the standpoint-based critique of feminist scholarship mean, therefore, that only Black lesbians can study Black lesbians, that only working class Jews can study working class Jews?

Yes and no.

I have argued that the structure of oppression hampers those who are in the dominant position, so that we are severely impaired in our ability to perceive the oppression in which we are implicated; privilege limits even our ability to understand our *own* circumstances, let alone exercise *Verstehen* toward the oppressed. Our privileges in the structure of oppression leave us severely hampered, but depending on the circumstances, not fully disqualified from collaboratively engaging in the work.

Furthermore, the implication of my critique of *both* feminist empiricism and standpoint theory is that although oppression gives the oppressed a vantage point, a standpoint, by which Black lesbians and working-class Jews have the potential to better understand our own conditions, the structure of oppression, and the complexities of multiple oppression, hamper our own search for clarity. Thus our relationship to oppression, as either privileged or oppressed, has implications for the quality of our research, but our relationship to it is contradictory, complex, and, to some degree, up to us. That is, in part, why I called this essay "*Contradictions* of Feminist Methodology."

Dialogue—multilogue—the noise of debate are absolutely essential correctives to the contradictory limitations of privilege and oppression. We will not be able to perceive our diverse, interlocking, structurally induced errors unless we can point them out to each other and learn from each other's work across the boundaries of race, class, and other conditions of oppression.

Nor are purely academic "discourses" sufficient. If we truly learn from Cook and Fonow (and from Marx before them) that the point is not merely to describe the world but also to change it, then it is in the crucible of activism, of coalition work to produce feminist change simultaneously confronting racism, heterosexism, and the other forms of oppression, that greater clarity may be forged. And it is in this sense that the separation over the past two decades of academic feminism from the active feminist movement has seriously handicapped both theory and practice.

Beyond a critique of feminist empiricism, the unique contribution

of this chapter is, I believe, my suggestion that "ultimately . . . we can build toward . . . an understanding of the 'complex of many determinations' as a set of dynamic interrelations." I introduced this model as a possible way of dealing with the problem created by the multiplicity of "standpoints" within multiple systems of oppression.

But what does the phrase "complex of many determinations" mean? What does it imply? It means that things are not unicausal or unidirectional, that each element of the complex (e.g., the subject being studied or an instance of oppression or society considered as a whole) affects and is affected by all of the other elements in multiple (specifiable) ways. It means that cause and effect are not a unilinear chain; rather, cause and effect are dialectically interrelated. It is our job to systematically trace these interrelationships.

The notion of creating (or analyzing) the "complex of many determinations" moves beyond the fragmentary "postmodern" description of "difference" in two ways. It is not *fragmentary*—rather, it conceptualizes difference as an expression of a relationship, not merely a separation. It moves beyond *description* to analysis of the forces producing those differences and relationships and the (dynamic) structure of which they are a (changing) part. When Adrienne Rich moved beyond complaining about the exclusion of lesbians from feminist writing to analyzing "compulsory heterosexuality," lesbians—and indeed feminism itself—moved beyond discussing "difference" to analyzing the determinants of lesbian existence. More than that: The concept of compulsory heterosexuality determines not only lesbianism but also heterosexuality as a set of institutions and ideological practices; compulsory heterosexuality shapes sexuality, economics, and gender inequality.

The task now is to find the comparable underlying elements with regard to race, nationality, and—more germane to my own work—anti-Semitism and Jewish existence. We can readily find in the works of people of color this comparable analysis at the core of racism and nationalism/nationality, for example.[4] Now we must further explicate the interrelations (in as painstaking and exhaustive a way as Marx did with Production-Distribution-Exchange-Consumption) between racism and compulsory heterosexuality and between each of these and social class. I have not even begun to make this type of analysis with respect to anti-Semitism and Jewish existence. I still consider the "complex of many determinations" to be a model for analyzing the (changing) system of interlocked and crosscutting oppressions in patriarchal, capitalist society. It is a model yet to be achieved, a methodological aspiration, a path, a promise, a task for our collective work.

NOTES

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1. I use the term *dominant* methodology, rather than *traditional* or *mainstream*, because *traditional* connotes a benign antiquity that modern-day social science does not have, and because *mainstream*, a rather bucolic metaphor, seems to imply that alternative methodologies are mere tributaries, rivulets of the mainstream, rather than real alternatives with opposing assumptions and consequences. The term *mainstream* also washes over the power structure that maintains the dominant methodologies in place and relegates alternative methodologies to the periphery.

2. Of course, Marx said, "*Men* make their own history, but . . . they do not make it under circumstances chosen by themselves" (1963:15).

3. See, for example, Collins (1990) and hooks (1984) among many, many others.

4. McCluskey (1994).

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CHAPTER 2

DOROTHY E. SMITH

Contradictions for Feminist Social Scientists

I am unreservedly committed to securing for women the resources institutionalized in the academy that create knowledge, build and transmit intellectual tradition, house and foster debate, and sustain continuities across generations. While we have lacked these, in the past twenty years or so we have begun to secure them but at a cost. Twentieth-century North American universities have never been directly subjected to state control. But buried historically in their foundations is a powerful class politics. It is a politics that, at crucial periods during the thirties, fifties, and again in the nineties, has sought to isolate the university bases of the intelligentsia from local and regional connections, from linkages with the working class and the activism of the trade union movement, and from organizations identified with oppressed and marginalized groups. This politics is institutionalized in the university system. Most of the time it is taken for granted. It only comes into view when it has to be enforced. Freedom of thought, speech, and the pursuit of knowledge, however imperfectly realized in the university system, create an endemic pressure threatening to authority. When it breaks out of bounds, as it did during the early fifties, the sixties, and the seventies, it is actively repressed. Discourse, the across-time-and-space conversation of the intelligentsia, builds in the boundaries of the institutional order, adopting its standpoint and incorporating its relevances and interests. So long as it observes the class boundaries imposed by that order and does not serve the need to know of people subordinated to it, discourse is not exposed to repressive political pressure.

By relations of ruling I mean that complex of extra locally organized relations that are specialized *as* forms of organizing, regulating, communicating, and so forth. They are text-based and entirely reliant on

technologies of the text, including, of course, computers. We know them variously as text-mediated discourse, professional organization, formal organization, management, administration, bureaucracy, the state, at all levels, and so forth. The categories naming them overlap because they have been evoked by the theories, issues, and topics of different disciplines, but they identify a variety of institutional forms that act in and regulate the local actualities of people's lives. The relations of ruling rely on and generate specialized systems of concepts, theories, categories, technical languages, and so forth that define their objects and situations as actionable on their terms. These specialized language systems generalize the distinctive organizational competence of each institutional form across the many and various sites of people's actual living.

The work of the social sciences sediments the logic of these controls. The sociology we have adopts the boundaries of its terrain; it is hooked up dialogically to its relations at multiple points, often incorporating their categories directly and always preserving their standpoint of ruling in its conceptual and theoretical order; it knows how to harvest information, data, and knowledge from people and to bring it home to the text-based discourses that have universities as their local bases. Social scientific knowledge represents the world from a standpoint in the relations of ruling, not from the standpoint of those who are ruled.

The relations of ruling are far from monolithic. Multiple and diverse interests and voices operate within it. Nonetheless, generally its terrain is never conceded to or opened up to use and influence by people who are exploited, marginalized, or subordinated by the relations of contemporary capitalism. As a system of control it has been particularly effective in ensuring that the knowledge produced is not oriented toward the needs and interests of the mass of people, but to the needs and interests of ruling. When loopholes are found and leakages occur, they are stopped; when those privileged with access desert and go over to the other side, their access is cut off. Breached during the sixties, this system of control has been reformed and tightened.

The women's movement has its distinctive history and its distinctive struggle against and within the relations of ruling because these relations encysted the gender hierarchy we call "patriarchy." Its critique of patriarchy has often been also a critique of the relations of ruling, proposing radically alternative forms of organizing the social relations of knowledge and communication. It revealed, for many women, the taken-for-granted class, gender, and racial subtexts of academic institutions—the hidden boundaries, exclusions, and positionings on which the texts and practices of ruling rely.

The women's movement has struggled to make women's voices heard in universities and colleges and within academic disciplines. Those of us who were active in universities and colleges were, in the early stages of this struggle, activists in the women's movement outside as well; what we worked for in the academy was inseparable from what we were working for outside it. We wanted the immense resources vested in the university and college systems to sustain the development of thought, knowledge, and culture by women and for women. We had discovered, were and are discovering an intellectual and political world to which women were marginal if present at all. The intellectual, cultural, and political achievements of our foremothers had been for their own time only—if at all. The academy has never vested its resources in preserving and advancing their thought and work. If there was no ongoing intellectual tradition among women, no conversation extending from the past into the present, it was in part because the resources of the academy were never dedicated to this project.

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To remind us that the women's movement didn't begin with us, we have called this a new phase or wave of the North American and European women's movement. But this phase has been and is distinctive in making us conscious of the complex of power relations we have named "patriarchy" and in the development of organization aimed at transforming it. Whatever that term might mean when subjected to refined definition, it showed us the institutionalized barriers and exclusions that we had for so many hundreds of years taken for granted. And though women are everywhere in the society, established channels of communication—academic discourses and universities, book publishing, the mass media of television, newspapers, even women's magazines—were not for us, as women, to use in organizing among women, speaking and writing as women for women, developing issues, innovating expression, and remaking academic and professional discourses from women's standpoint.

A radical critique speaking from the experience of women has been integral to the politics of the women's movement. Recognizing women's right to speak from the actualities of our experience is always potentially disruptive; there is always something new to be heard; there is always rethinking to be done. The "we" of the women's movement has been open; hence settled positions in it are always subject to challenge. The perspectives and relevances of white, heterosexual, middle-class women built into the definition of women's issues have been disrupted by working-class, lesbian, and nonwhite women opposing

that hegemony. Elsewhere women of "developing" countries were and are evolving a women's movement or women's movements independent of and in many respects more radical than those of North America and Europe.

New bases of organization have been emerging constantly; rifts and rows resulted in new activism, realigning and expanding the women's movement's system of communication. We published newspapers and newsletters; created new publishing houses, established bookstores. And of course we also tried to convert the established structures for women, most often by creating within them a shell, such as a women's caucus or committee, or in universities and colleges, a women's studies course, even a program. We took seriously in practice and in theory the universality lent our project by the category "women." Though established exclusions and barriers of race, class, politics, and imperialism were implicit in women's movement practice, they were, and are, always subject to confrontation and disruption. The very claim to speak *as women* and *for women* as the speaker creates its own instability as the speaker is found not to be speaking for me, for you, for her, for us.

Today women like myself working in the academy most often learn of other women's experiences in the disembodiments of the text wherein they appear only as "voices." But in the early days of the women's movement, we were also connected in multiple ways through organization and activism. Of course these may have been the peculiarities of my own experience growing up as a feminist in the women's movement in Vancouver, Canada, a city then of some one and a half million. The women's movement of those times was a many-headed organization, a hydra of contending groups. Yet despite contention, or perhaps because of it, there was an interlacing of multiple relationships crosscutting factional differences.

Our challenges to the ruling relations weren't only voices, they were challenges created by activism and experience acquired in activism; issues of gender, sexuality, and class were raised not as a matter of theory but as a matter of political practice (much more rarely at that time they were challenges raising issues of race). The activism and debates of the women's movement were embedded in and responsive to other forms of activism and organization of the time—the openings created by the movements of the sixties, the idealization of the Chinese revolution, the renewal of Marxism-Leninism. The dogmatisms and sectarian forms of organization were hooked in to a footloose women's movement whose participants moved in and out of organizations, quarreled, made friends and enemies, debated positions, and

created new organization out of dissatisfaction with what was already in existence or recognizing newly a gap where action was called for.

The splitting, trashing, passionate quarrels, debates out of which opposing sides came, resolving never to speak again to former friends and allies, were the dynamic of a movement that was grounded in multiple ways in the society. What had seemed at first a simplicity of our sisterhood, what we confronted as patriarchy, what we found as the bases of our oppression in the control of our bodies, for example, turned out to be magnificent but untenable simplifications. Because women were everywhere in the society and because the forms of what we named patriarchy turned out to be multiple and various, the women's movement came to take up issues not as a generality crosscutting such divisions but as based in particularities of experiences in paid employment, in the home, in the community, in relation to children, in political organization, in sexual relationships, of racism—wherever our daily/nightly lives located us.

Differentiation did not mean separation. Arguments and debates were intense and passionate; they engaged others in opposition as well as in agreement. In them, positions became defined, were given theoretical formulation, were dissolved, and were reformed. The debates, alignments, conflicts, shared experiences, and issues linked women across institutional barriers. The connections were *active*, partly in and through the media of the women's movement, partly in organization around issues of shared concern, and partly just in informal support and discussion.

Women taking up the women's movement in the academy were part of this connectedness. We participated with women outside the university attempting to create linkages that broke with its traditional isolation and its traditional claims to authority. Certainly this was my experience. And I found that the institutional barriers that detach the university from the local community had two sides: on the one hand, there was nothing in the university that supported making connections outside. If anything it made difficulties; and, on the other, as we became active in off-campus organizations, we found that the women we worked with were antagonistic to the implied superiority of knowledge derived from the institutional dominance of the academy. We were under constant critical pressure in this encounter, constantly challenged by thinking and theories originating outside academic discourse, and by being confronted with bases of knowing grounded in experiences other and beyond our own.[1]

In universities and colleges, we sought alternatives to institutional connections and to use the skills and resources the academy commands

for women. The women's studies courses that we established also relied on experience and understandings grounded outside the academy. Since there was little or nothing in the way of books or articles that we could teach from in more orthodox styles, we didn't speak in our classes from an established discourse that we were trying to pass on to our students. Rather, we encouraged and evoked their and our speaking from our experience as women, beyond the comprehension of the academic discourses as they were then.

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In the years since that time, we have achieved extraordinary things in the academy. Of course, we do well to take nothing for granted, but women's studies are now part of the normal course of business in many if not most universities and colleges of English-speaking North America. We now enjoy resources that we did not have before. We made rich and brilliant achievements. The contrast between the early days of teaching women's studies and today is very marked. Then we had extraordinarily little material to learn and teach from. Now the wealth of women's scholarly, cultural, and political writing is vast. It is powerfully enriched by the progressive displacement of white women from the center and the advent of the authoritative presences of Latina, Black, Asian, and native women. Whereas once each individual could think she could know it all—read a little later at night, get up a little earlier in the morning—we now know that is impossible. And for better or for worse, we now have specializations, subdisciplines, schools, academic factions, hierarchies.

We have, I do believe, been successful in vesting at least some of the institutional resources of the academy in preserving, transmitting, and advancing knowledge of and for women. We have also been somewhat successful in breaking down the radical one-sidedness of the male-dominated discourses of the disciplines and sciences, particularly in the humanities and social sciences. If we have not succeeded altogether in overturning the claims to generality based on gender partiality, we have at least succeeded in creating a richness of critique and alternatives that is astonishing given the relatively brief period of our "renaissance."

But there is a cost. There are powers operating at a less-visible level in the university that pull our feminist work in unseen ways. In establishing ourselves in the academy, in making a place for women and women's experience in social science and the humanities, to a modest degree in law and the life sciences, and at least marginally in medicine, we have also become increasingly detached from our former

linkages with activism and organization outside the academy. This is partly due, no doubt, to the changing organization of the women's movement itself, to growth and advances that have meant more specialization and less place for the multiconnected activism of the earlier movement. In the academy, it has meant a progressive conforming of the discourses of women's studies and feminist theorizing to the institutional boundaries of the university. Once the production of knowledge for women became fully embedded in the academy, the ties that hooked its characteristic forms and directions of development with the concerns of women outside the academy became increasingly attenuated. Not that such knowledge production has not continued; this is particularly true for Black and Latina women with ongoing activist ties outside the academy. European-American women have also continued to do work that serves women, but the pull these earlier forms of activism and organization exercised over our minds, imaginations, energies, and loyalties has been attenuated. Increasingly feminists working in the academy with ties to activism beyond it are hooked into the relations of ruling—professions, public service, political life, scholarly careers, and so on. As our own thinking becomes more articulated to disciplines sedimenting the hidden political ground of the academy, we become increasingly detached from independent sources of resistance and from the profoundly different take on the world they represent. Our feminism becomes professionalized.

And there *is* a hidden political ground. It has a history that is part of the developing organization of class in North America. The recurrent politicization of university campuses stimulating alternative approaches to teaching and research have encountered, sooner or later, repressive political responses. The pressure may come via boards of governors representing very directly the interests of a regional ruling class or from university administrators and organizations of administrators or from within the academic community itself or from external and indirect pressures from the media or politicians. Clyde Barrow's study of the "reconstruction" of U.S. higher education during the early years of the twentieth century describes a major political shift from faculty to administrative and board of governors control of universities. He details the specific practices, now institutionalized in universities, by which that control was secured, in particular over the freedom of faculty to develop knowledge independent of the interests of business (Barrow 1990:250–59). Capacities established then have come into play during the recurrent waves of campus political activism since that time.

The public target of McCarthyism was the Communist party, but its

effective, perhaps intended, aim was the broader-based political activism of students and faculty after World War II (Schrecker 1986:84–85). Student organizations created linkages with labor, took up civil rights issues, proposed foreign relations radically opposed to "cold war" thinking, and were involved with left-wing presidential politics. Earlier campus radicalism in the forties had been repressed. The attack on radicalism in the fifties, now identified with McCarthyism, confronted and repressed a new and more general political activism on campus. The attack on communism has been called a witch-hunt because merely to come under suspicion was to be damned. Faculty members were frightened, particularly liberal faculty. They might not have changed their views, but they were afraid to act on them.[2] In some instances, political activism on campus was prohibited altogether—the issue that eventually, in the sixties, sparked the free speech movement on the Berkeley campus of the University of California.

Emphasis on the ideological dimensions of campus radicalism tends to obscure the extent to which it breached a class-enclosure institutionalized as the effect of various repressive moves. I've sometimes thought that faculty members could be as radical as they wished in their writing and in the classroom so long as radicalism did not lead to an activism that built relationships between a university intelligentsia and a society's marginalized and exploited people. Writing of the early decades of this century, Barrow describes repression directed against individual faculty members who undertook research in the interests of local communities and against those of "big business." An agricultural chemist, for example, was penalized for bringing evidence to a state legislature about the damage done to local crops by copper smelting (1990:241). The notion of the scholarly and academic as detached from concerns and interests external to discourse itself is constructed and reconstructed recurrently in the context of an endemic campus activism. Writing of the period following the seventies, Paul Diesing describes how administrative and budgetary powers of university administrations and boards were used to tame radical faculty: "We found faculty resolutions ignored by the university administration and tenure recommendations overruled; library budgets cut and class sizes increased; tenured professors fired with three months' notice or less; educational policy and resources shifted without consulting us. . . . Our illusions crumbled; we began soberly to face our real conditions of life" (1982:262).

Political repression had its effects in social scientific thinking. The McCarthy period of sharp divorce between intellectual activity and what it might imply in terms of the society at large can be traced in

sociology's shifts from the influences of Marxism or native North American traditions of radicalism, such as those embodied in C. Wright Mills's work. The "systems" thinking of Talcott Parsons, in which issues of class and racial oppression disappear, assumed a dominant role (Diesing 1982). "Mass society" theory was briefly popular, perhaps because it offered a striking redefinition of the intelligentsia's relationship to the masses, proposing that an elite's ability to sustain democratic values could only be effective if it preserved its detachment from the people. Marxist conceptions of class and class struggle were displaced by and sublated in the new politically purified notion of "social stratification." Durkheim's devices for converting the world of actual people into a subjectless phenomenal universe became standard practice (Smith 1989). The popular methodological artifice of the Archimedian point constituted a discursive space for sociology outside class relations: race was an object but lacked subjects; and gender was not yet even a whisper. The artifices of objectified discourse concealed the real subtexts of race, class, and gender oppressions. In the contemporary context, postmodernism has written the constitution that eliminates from the phenomenal domain of social and cultural thinking the bases of oppression that Marxism had brought into view, replacing it with a self-reflexive critique of discourse within discourse.

Feminist postmodernism is feminism's own variant of the post-McCarthy redesigning of sociological discourse that stripped social science of its relation to political activism beyond the academy. It insists on the subject as existing only in discourse, creating a discursive seclusion that restricts speakers and speech, writers and writing, to discourse's objects and conventions (Butler and Scott 1992; Smith 1993). It repudiates the speaking from experience that was so powerful in the beginnings of this phase of our women's movement and that has been so powerful in the disruptions and displacements of white, middle-class, heterosexual hegemony in feminism. At an earlier period, women would speak up from and for the margins that had been created by the focus of a conference or meeting. They could speak from their own realities and that had its own authority; others might not agree or believe, but had to attend to what was said and take it seriously. But the conventions of postmodernist feminism, for all its denial of the unitary subject of modernism, set up barriers to such speech. Take this striking tale by Susannah Radstone of her experience at a conference held at Glasgow University in Scotland in 1991. She is describing her uneasiness with the kinds of exclusion that the theoretical commitments of the conference set up.

> My first, and perhaps keenest intimation of unease came . . . when . . . Glasgow's Lord Provost—a woman—delivered a heart-rending speech of welcome. She felt, she told us all, an abiding sense of gratitude for and commitment to the struggles which had enshrined women's—and especially working-class women's—right to education, since, as a child of the Glasgow tenements, it was via education that she had accomplished her undoubtedly tough journey from the Gorbals to the civic hall. This was a story that moved me, a story told bravely, and a story told from the heart—a story, though, which appeared destined to fall into the void. For, unlike at Ruskin [a women's movement conference twenty-one years before], at Glasgow there remained no place, apparently, for the questions implicitly raised by the Lord Provost's welcoming address—questions about women and class, women and education, and women and the welfare state, to name but a few. (Radstone 1992:87)

The effect of "there remaining no place" points to how feminist theory organized the discursive enclave on that occasion, what could be said and what could be admitted to the talk of women there. The ongoing discussions were impenetrable to speech that would address what might be actually going on in the lives of women in Glasgow or the lives of women anywhere.

I emphasize that this isn't a matter of individual responsibility or guilt. It is a matter of the social relations in which our work comes to be embedded, who its readers are, how it is funded (and let's not pretend that we can go on forever doing work on shoestrings and night oil), and how it is recognized for purposes of publication and hence serves not only our survival in the academy (and I don't trivialize the issue of survival) but also our means of reaching others through our work. The political ordering of the academy is less significant in our choice of subject matter than it is in ensuring that we write from a standpoint that fits our work within the dialogic parameters of the relations of ruling.

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Here is our difficulty, our problem, and our problematic. On one side, there's the problem of how to write a sociology that speaks in and of the world as it is in women's, in people's, actual experience. If we are to be writing a sociology that serves people, we have to create knowledge from their standpoint that provides maps or diagrams of the dynamic of macrosocial powers and processes that shapes their/our lives.

We want to create a systematically extended consciousness of society from women's standpoint and therefore we want more than short-run applications of our sociological skills. We also need to advance the technicalities of such a knowledge so that our research can be responsible in terms of "truth," accuracy, and relevance. And we have to contend with the jungle created by the in-text organization of sociology's object world, so that we can put in place a sociology or sociologies oriented to exploring the extended social relations of people's lives. Such a sociology or sociologies would recognize that, as Marx saw, the social comes into being only as the doings of actual people under definite material conditions and that we enter into social relations that our own activities bring into being but we do not control them. Thus our own powers contribute to powers that stand over against us and "overpower our lives" (Marx and Engels 1973:90).

On the other side is the problem of connecting such a sociology to those for whom it might be useful and who might use it. There must be real and equal interchange. Solving this problem is essential to how such a knowledge can be developed; knowledge is not abstract but is embedded in a discourse or discourses. How could a knowledge of the kind sketched in the previous paragraph develop if it were embedded in discourses wholly within the academic circuits of sociological and feminist theory? My own experience has been telling in this respect. For I started work on a sociology for women in the very practical contexts of a women's research center that was oriented, outside the university context, to working *for* women in the community and my thinking was pulled by the exigencies of doing this kind of research properly and finding that the sociology I knew would not do. But as my thinking on a sociology for women became known in academic contexts, I was invited to speak in (usually U.S.) universities to audiences of women scholars who were interested in issues of theory and epistemology and I became a participant in discourses of quite a different order. These were discourses that were framed within the academy and were "determined" largely by a feminist dialogue with established disciplines rather than with activists. My thinking began to orient toward these debates, and in consequence, more than functionally toward the theoretical and epistemological frames of academic discourses. My work was certainly strengthened in this process, but if the other term of the relationship had continued to be as strongly present as it was when I started on this line of inquiry, my work could only have benefited from being pulled more immediately and vitally into relevance to women outside the academy. In its absence, the effect was to redirect my energies and thinking—and time—toward a theoretical discourse that it

had not intended originally. I could feel how my focus began to shift, a process that was progressive as the great political impetus of the sixties faltered and was repressed.

Any work and its development becomes what it is and will do in discursive settings. Discursive settings that are insulated from activism produce research and theorizing that is oriented elsewhere than activism. The theory may still be "radical" but nonetheless it is withdrawn from its anchorage with the actualities of people's lives, as we see with contemporary Marxist theorizing.

There are no easy solutions here. Since it's not a matter of an individual's intentions, or individual guilt, it is also not something that can be changed simply by an act of will. The implicit political organization of the ivory-tower university is still effective. It is not easy to go against it, though we can sometimes get things done within its scope. Indeed the recent attack on what is described by its opponents as the "political correctness" movement on North American campuses suggests that we have been more successful than perhaps we'd realized.

But this isn't the only isolation. The disciplinary norms to which we are subject sediment the politics of the ivory tower. They were put into place during the fifties and sixties in the process of moving sociology away from its earlier dubious connections with class struggle and the suspect ties of the North American intelligentsia of the forties and fifties to the working class. The academy creeps up on us. How do we get to keep our jobs? By writing papers that will be published in academic journals. To get our papers published we have to conform to procedural and methodological canons that have no relationship to what might be conceived of as the canons of relevance of women activists. Of course we have made and preserve our openings into the relationships in which so much of what we have done as feminists originate; we are ingenious. But it becomes increasingly difficult, and increasingly difficult as new feminist theories are established, even popularized, that conform to the class contours of the academy, the more so now that universities themselves are being increasingly pulled into direct subservience to the requirements of a global capitalism.

The academy is not seamless. I've shown earlier how repressions have followed on initiatives among students and faculty opposing injustices, oppressions, suffering in the society. Universities bear, I believe, the hidden radicalism of the Enlightenment and in teaching, in talk, and in the access to stored knowledges become, every now and again, reconnected to this long historical project. Here, for feminism, is our distinctive project in the academy. It is one in which, in fact, many of us are involved. I propose that we become more conscious of it and of each other.

The idea of developing a social science *for* women, which has been my own project, is not, of course, exclusively mine. There are women in the academy all over North America who in different ways are also engaged. We can change the way sociology knows the world because it is still a site of debate; because the academy is changing; because we know that there's desperation in society and our social sciences don't know how to know this new and frightening world; and because we know as women that we have the power and capacity to change and to create a social science that serves people rather than servicing their ruling. But the problem of how to create and preserve linkages with women working in sites outside the academy remains and hence the problem of how to create intersections between the in- and extra-academy discourses of feminism remains as well.

No intellectual enterprise can subsist in a social vacuum. Every such enterprise participates in something we could call a community. Because we are working with texts, reading and writing texts, the existence and significance of that community is often invisible to us. Yet it is always implicit in what we write. How then are we to defend and intensify connections beyond the academy against the multiple ways it inhibits them? In the session at the annual meetings of the Society for the Study of Social Problems at which the ideas of this essay were originally presented, a woman activist spoke of her dismay in finding how distant the sociology she had been hearing at the meetings was from the concerns and interests of activists and the women they represented. We had no answer then. This essay has no answer. But it has a diagnosis. It does not locate the issue in the individual intentions of women sociologists or in "careerism." The issue is not a matter of individual guilt. Rather it is to be located in the social relations embedding a politics at a level of the organization of the academy where it is not visible as such. Making it visible is a first step in addressing how we can overcome, bypass, and, as a minimum, avoid consciously replicating and reaffirming a politics that is neither for women nor for people in general.

NOTES

1. Of course, women are not the only group that has developed these relations. Here is a more current instance of exactly the kinds of relationships that the regulators of the academy fear: "Throughout this protracted struggle [for a Chicana/o Studies Department], a student-community alliance matured and campus mobilizations at UCLA began to take the shape of a crucial community struggle. Equally important, as the movement gained concessions from a resistant administration, Chicana/o student-faculty relations were strengthened by the presence of seasoned community organizers who mediated disputes and challenged all parties to keep the goal of the department at the heart of each action. This resulted in several key philosophical victories. Chief among these was a faculty pledge to include students and community people in the governance of Chicana/o Studies. Faculty were also pushed to develop curriculum focusing on current problems of the Latina/o community. Finally, faculty members were challenged to reaffirm through concrete action the basic aims of Chicana/o Studies—the development of an informed and community-minded leadership among our college youth" (Lizardo 1993:13).

Though unusually well-organized, struggles of this kind aimed at connecting the intellectual resources of the university with the needs of people in a local community are an endemic feature of campus activism and I suspect that it is this kind of activism that is most likely to invite repressive moves. An activism dedicated to causes elsewhere in the world is much less threatening. The latter does not jeopardize the monopoly control of intellectual resources and "production" by the class or classes that dominate in contemporary capitalism. Hence the carefully crafted controls that have been built into the academy and the discourses it sustains and the progressive shift of intellectual resources into organizations such as "think tanks" that can be more directly and consistently regulated.

2. Reported in Lazarsfeld (1958) according to Schrecker (1986:309).

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CHAPTER 3

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AND JOKE ESSEVELD

Objectivity and Truth: Problems in Doing Feminist Research

What methods should be used in a feminist[1] analysis of society? Are there modes of thinking, data collection, and analysis that are more appropriate than others for studying the situation of women from a feminist perspective? These questions were raised early in the contemporary feminist critique of the social sciences (Bart 1971; Bernard 1973; McCormack 1975; Smith 1974) and are still being explored and developed. Feminist scholars have analyzed the male bias in the social sciences (see, e.g., Sherman and Beck 1979) and are beginning to make a distinctive contribution to long-standing debates about theory and method (Smith 1977, 1979, 1980; Westkott 1979), sharing the concern of others with basic and enduring controversies such as the nature of science, its epistemological foundation, the possibility of a science of society, and the role of science in maintaining or undermining systems of power (see, e.g., Blumer 1969; Berstein 1978; Hughes 1980). In addition, we are beginning to consider how these debates become translated into problematic methodological issues for those doing empirical studies within a feminist perspective (e.g., Roberts 1981).

The goals of feminist social science have developed in the context of the criticism of the established natural science model of sociology and related disciplines (Bernard 1973). Extending that critique, some feminist perspectives share the critical view of the Marxist and interpretative traditions within the social sciences, while adding their own emphasis and content (Smith 1974, 1977). These feminists have argued that the traditional approach to social science is compatible with the aims of those in particular locations or positions of management and control in society (Smith 1977) whose goals include such things as

managing workers more effectively, dealing with civil disorder, and encouraging women to enter or leave the work force in accord with changing economic conditions; thus, what is taken as problematic in much of social science has also been what is problematic for those who control and manage the society. Moreover, in addition to problem definition, the concepts, frames of reference, and perspectives that define traditional sociology express the interests of and arise out of particular social institutions in which the governing and organizing of society takes place (Smith 1979). Almost all those who rule and manage are male; interesting and important phenomena are identified from a male perspective as well as from the perspective of those who manage and control. Women are largely absent from this world; the female domain of production and reproduction that provides the necessary infrastructure for the male world is, despite its importance, invisible, uninteresting to many social scientists, and largely unconceptualized. Thus, in the history of sociology, the development of an approach to knowledge with the goal of control has contributed to the failure to study the situation of women, as well as to a conceptualization of women that is consistent with continuing male dominance (Acker 1973). Beginning around 1970, attempts to deal with the exclusion, distortion, and neglect of women have produced many useful theoretical and empirical studies. One significant result of this research has been the identification of many regularities and correlations that describe women's situation. However, this has limitations for building a tradition of research for women because it leaves largely unexamined the social processes lying behind the correlations. Understanding the processes that result in inequalities is a necessary step toward changing women's position. For us this understanding comes from a theoretical perspective that has its roots in feminism, Marxism, and critical theory. This means a commitment to a social science that can help change the world as well as describe it. "Women's devaluation—and the consequences of this devaluation are reinforced by a social science which records these conditions while systematically ignoring alternative possibilities" (Westkott 1979:428).

The goals of a sociology for women, one that is in the interests of women rather than only about women, must be emancipatory (Esseveld 1980; Hartsock 1979; Westkott 1979). *Emancipation*, as we use the term, means the eventual end of social and economic conditions that oppress women and the achievement of a free society. The ideal is that women should be self-emancipating and our conviction is that social scientists can contribute to this process (Karabel 1976) by analyzing how the personal is political and by pushing that analysis beyond in-

dividual experience to comprehension of "its determination in the larger socio-economic structure" (Smith 1977:22). An emancipatory social science would provide women with understandings of how their everyday worlds, their trials and troubles, were and are generated by the larger social structure. The emancipatory aim of a women's sociology derives from its close connections with the contemporary women's movement as well as from our particular position as women researchers. Women's research is intimately connected with the political aims of the women's movement in a number of ways. The movement provided the necessary social basis for legitimation and political support that allowed women researchers to start publicly asking some of the questions they had long been asking privately. Moreover, the women's movement outside of academia posed new questions and new formulations of women's situation that then could be taken up in the academic setting. Women researchers, in addition, were usually members of the women's movement and had, and still have, a political commitment to ending women's oppression. This commitment supplied a general standard against which to assess the kinds of questions and problems that should be dealt with. At the same time women researchers were developing analyses of their own locations in the larger socioeconomic structure, for in some fundamental ways their positions were and are similar to those of their subjects. As women they too may have husbands and children, they too keep house as well as work, they too have to cope with sexism in their daily lives. Thus, a sociology for women has emancipatory possibilities for the researchers as well as the researched, for as women researchers we also have been absent and unheard within the male sociological traditions.

Having accepted the above critique of traditional social science and recognizing that in all social science women have been peripheral and their lives misrepresented, it is clear that a radical rebeginning is needed in feminist research.

For us, a radical rebeginning has meant understanding gender as central in constructing all social relations and taking individual women's lives as a problematic (Hartsock 1979; Smith 1980). What is to be explained is what actually happens in women's everyday world and how these events are experienced. We begin, then, with the ordinary life of women but neither stop there nor move into a search for individual psychological sources of feelings, actions, and events. Although we view people as active agents in their own lives and as such constructors of their social worlds, we do not see that activity as isolated and subjective. Rather, we locate individual experience in society and history, embedded within a set of social relations[2] that produce both

the possibilities and limitations of that experience. What is at issue is not just everyday experience but the relations that underlie it and the connections between the two. In this analysis, we use a dialectical method in order to arrive "at adequate description and analysis of how it actually works. Our methods cannot rest in procedures for deciding among different formalized 'opinions' about the world" (Smith 1977:26). Rather, this is a method of exploration and discovery, a way to begin to search for understandings that may contribute to the goals of liberation. Exploration, in our usage, means an open and critical process in which all the intellectual tools we have inherited from a male-dominated intellectual tradition are brought into question, including ideas about the basic nature of human beings, the nature of social life, the taken-for-granted worldview of traditional science, what concepts and questions might help to illuminate our shared condition, and how we should go about developing such knowledge.

In developing this knowledge we also try to maintain a critical perspective toward some of the assumptions made within the social sciences. For example, the assumption that the researcher must and can strive to be a neutral observer standing outside the social realities being studied is made by many who use quantitative and qualitative methods in a natural science model. This assumption is challenged by the feminist critique of social science that documents the male bias of theory and research that has previously been taken as a neutral account of human society. A feminist methodology must, therefore, deal with the issues of objectivity in social science and, in the process, deal also with the issue of the relationship between the researcher and the researched. As researchers, we must not impose definitions of reality on those researched, for to do so would undermine our intention to work toward a sociology for women. Our intention is to minimize the tendency in all research to transform those researched into objects of scrutiny and manipulation. In an ideal case, we would create conditions in which the object of research enters into the process as an active subject.

Recognizing the objects of the research as subjects in their own right suggests that researchers must take care not to make the research relationship an exploitative one. This has been a concern at least since the 1960s, when New Left criticism of the subtle and obvious repressions of bureaucratic society included an evaluation of the research process as oppressive. It becomes a critical issue for feminist researchers who themselves might be cast in the role of the research object and who, as women, have experienced the objectification of women in society. Perhaps more important, research that aims to be liberating

should not in the process become only another mode of oppression. But, this aim poses an ongoing contradiction; ultimately the researcher must objectify the experience of the researched, must translate that experience into more abstract and general terms if an analysis that links the individual to processes outside her immediate social world is to be achieved. Objectification would be minimized and the emancipatory goal furthered if both researcher and researched could participate in the process of analysis. But this is not always possible because the preconditions of such participation, some similarity of interest, ideology, and language between researcher and researched, are sometimes absent. Even with a similarity of interest, there are still problems of a practical nature. The impossibility of eliminating all objectification exists in all social research, and the problem cannot be solved by creating the illusion that no relationship exists between the researcher and the research object.

In summary, the following are some of the principles of feminist research with which we began this project:

1. Our goal was to contribute to women's liberation through producing knowledge that can be used by women themselves.
2. The methods of gaining this knowledge should not be oppressive.
3. We should continually develop the feminist critical perspective that questions both the dominant intellectual traditions and reflects on its own development.

In the pages that follow we begin to develop a methodology for doing feminist research based on the view of a social science outlined above.

WOMEN IN TRANSITION: AN EXAMPLE OF AN ATTEMPT TO DO FEMINIST RESEARCH

We, the authors of this essay, started a research project in 1976 with the intention of doing a study that might contribute to the liberation of women. We tried to apply the principles of feminist research discussed above and, in the process, learned about some of the difficulties with this approach. The following is an account of our research process and the problems we encountered.

The feminist critique of social science and our own commitment to the women's movement led us to select a particular problem. The choice of the problem, together with the critique, dictated a qualitative method of investigation. We chose repeated, unstructured, individual interviews as well as some group interviews. Although this

proved to be difficult and we are critical of our work at many points, the choice of a qualitative approach also produced new insights, new for us at least, about some of the issues raised above. As the project is concluding with the writing of a report, we are still convinced of the value of the method.

The Problem and the Method

The problem we chose was the relation between changes in the structural situation of women and changes in consciousness. We decided to look at the experiences of women who had been primarily mothers and wives and were attempting to move into the labor market. This group has participated in one of the major demographic changes in women's lives, their increasing entry into the paid labor force. We believed that these women, involved in a process of changing life circumstances, would come to see themselves differently as women and would reinterpret their problems, particularly in a social context that includes a widely discussed feminist movement.

The question of consciousness was important to us from a political point of view: consciousness-raising is an essential component of the feminist movement and a necessary part of feminist action (Bartky 1975; Westkott 1979). An understanding of how women's consciousness changes or doesn't change might be helpful to other women. Consciousness is important in a framework that views people as actors who intentionally try to affect their own situations. The oppression of women has limited our ability to actively intervene in working out our own destiny, but changing work opportunities and the feminist challenge to a whole range of barriers should have increased the possibilities for purposive action. An examination of whether or not this was occurring was thus relevant to our theories about the relation between individuals and social structure.

We were convinced that middle-aged women who had spent most of their lives as wives and mothers had been ignored by much of the movement and we hoped that we might give voice to some of their perspectives. We were also interested in this group because their long commitment to being housewives and mothers might make them resistant to change in a feminist direction. We also had a theoretical concern about adult life. At the time of the beginning of the research, very little had been written on middle-aged women; collectively as social scientists we knew next to nothing about the middle years of adult life. We were critical of what little literature existed and were skeptical of widely held assumptions about women of this age. For example, we

questioned the idea that women suffered from having an "empty nest" syndrome, an assumption that has since been discounted by a number of other researchers such as Lillian Rubin (1979). This was our general theoretical orientation, but consistent with the feminist critique that we, along with others, were working out, we decided not to structure interviews with predetermined definitions of consciousness. Rather, we entered our interviews in an unstructured way, getting women to talk about the changes occurring in their lives, leaving the definition of consciousness as an emergent knowledge that would come out of the discussions. This would allow us to develop a more thorough understanding of the women's own perspectives as well as get unanticipated information about events and problems. The women were interviewed in their own homes by one of the three of us as investigators. We had interviews with sixty-five women and followed a subgroup of thirty women for four to five years. We tried not to impose our ideas about what was important; our intention was to let the concepts, explanations, and interpretations of those participating in the study become the data we would analyze (Glaser and Strauss 1973). While we tried to avoid determining what was to be considered in the content of consciousness, we were still aware of our own theoretical ideas. In our continual process of analysis we had to confront discrepancies between our ideas and interpretations and those of the women we interviewed. As the interview process proceeded, we decided to bring up certain questions if they did not emerge in the interviews. The areas most likely to be unmentioned were the women's movement, feelings about aging, and sexuality. However, in most of the related interviews, the topics that we thought would be important came up spontaneously. Sometimes we did direct the interview. For example, after discussing present life situations and challenges, we asked about past history beginning with adolescence unless the interviewee herself initiated the subject of earlier experiences.

We got accounts of significant childhood experiences, as the women perceived them in the present. We also gathered information on education and work experience; on relationships with parents, husbands, children, and friends; and on aspirations and hopes for the future.

In second and subsequent interviews, we filled out areas not touched on before, but in particular focused on the changes that had taken place since the previous interviews, as well as on the issues that seemed to be paramount at the time of the interview. During the whole period of interviewing, which for some of the participants extended over five years, we in the research group had extensive discussions of the interview process. In these discussions we were in an ongoing process of reformulating our ideas and examining the validity of our assumptions about the change process; about how to conceptualize consciousness; about the connections between changing life circumstances and changing views of self, others, and the larger world; and about how to link analytically these individual lives with the structure of industrial capitalism in the United States in the seventies. Each of us had both formal training and considerable past experience in interviewing. Consequently, although we discussed the interview process, our main focus was on the analysis and integration of the data.

The initial interviews and many of the second interviews were taped and transcribed. Later interviews were treated differently—we took notes during the interview and then wrote, immediately afterward, to the best of our ability, a process account of the interview. Some of the taped interviews were not transcribed. Instead, we listened to the interviews, perhaps several times, noting topic areas and their locations on the tapes so that they could be listened to again when we were working on a particular theme. We then made detailed summaries of each woman's situation that included the main facts about her current life (marital status, number of children, work status, class, age) her perception of her problems, her goals, her consciousness of the women's movement, and the dilemmas or contradictions that we saw in her life. We made similar summaries at later interviews. At the same time, we were trying to identify common themes and also differences in experience. This analysis went on during the whole period of interviewing. We will return to the analysis process later, but here we want to discuss some other issues related to the interview process, in particular the influence of our relationship with our study participants.

The Research Relationship

The idea of neutrality and objectivity in the social sciences has been extensively criticized by those working within the interpretive traditions (Blumer 1969; Hughes 1980) and by some Marxists and critical theorists of the Frankfurt School (Habermas 1971; Bernstein 1978). Taking a women's perspective adds to that critique in some important ways. The ideal of objectivity is to remove the particular point of view of the observer from the research process so that the results will not be biased by the researcher's subjectivity. "Recent versions of this ideal of objectivity have emphasized the importance of the universal application of social science methods as the best guarantee against the bias or subjectivity" (Westkott 1979:425). These methods are designed to separate the knower from the object of study. Rejecting the notion

that such a separation is possible. Dorothy Smith (1977) argues that the illusion of this separation can be maintained so long as the knower can be posited as an abstract being and the object can be posited as the Other who cannot reflect back on and affect the knower. "Once women are inserted into sociological sentences as their subjects, however, the appearance of impersonality goes. The knower turns out not to be the 'abstract knower' after all, but a member of a definite social category occupying definite positions within the society" (Smith 1974:16–17). It also turns out that research is embedded in a definite social relationship in which there is a power differential in favor of the knower, who assumes the power to define in the process of the research. Research reports reflect only one side of this social relationship—that of the more powerful "knower."

That there is a relationship between the subject and object of study is more easily made visible when women are researching women. "Women studying women reveals the complex way in which women as objects of knowledge reflect back upon women as subjects of knowledge. Knowledge of the other and knowledge of the self are mutually informing because self and other share a common condition of being women" (Westkott 1979:426). The research process becomes a dialogue between the researcher and the researched, an effort to explore and clarify the topic under discussion, an attempt to clarify and expand understandings; the researcher and the researched are assumed to be individuals who reflect upon their experience and who can communicate those reflections. This is inherent in the situation; neither the subjectivity of the researcher nor the subjectivity of the researched can be eliminated in the process.

Our commitment to reducing so far as we could the unequal power in the research relationship and acknowledging the subjectivity of our study participants took a variety of forms. One strategy was encouraging the interviewee to take the lead in deciding what to talk about. This did not always work; people have ideas about what it is like to be interviewed and they want to be asked questions so that they can give the "right responses." Some women were uneasy with us because we were from the university. Others did not want to set the terms of the discussion because they felt that there couldn't be anything interesting about their lives. However, those with whom we had more than one interview increasingly took the lead in discussions and even took the initiative to get in touch with us to tell us what had been happening to them. Unstructured interviewing and letting the women take an important part in the discussion helped to counter some of the problems other researchers have confronted when using a more standard sociological methodology. Thus we did not have the problems encountered by Diana Woodward and Lynne Chisholm, who used more structured interviews and, as a result, enlarged the gap already existing between them and their subjects of study: "The very nature of our questions about employment and the domestic division of labor served to reveal our pre-occupation with work, marital conflict and women's oppression, rather then with the satisfaction of motherhood and housewifery" (1981:177).

Another part of the attempt to deal with the subject-object problem was to try to establish some reciprocity by offering, at the end of the first interview, to tell the women something about ourselves if we had not done so earlier. Often we didn't have to offer—it was a request made to us. We always responded as honestly as we could, talking about aspects of our lives that were similar to the things we had been discussing about the experience of the interviewee—our marriages, our children, our jobs, our parents. Often this meant also that our relationship was defined as something that existed beyond the limits of the interview situation. We formed friendships with many of the women in the study. We were offered hospitality and were asked to meet husbands, friends, and children. Sometimes we would provide help to one or another woman in the study. For example, one woman became depressed and called the interviewer, who then went to the interviewee's house and spent several hours with her while she talked about her troubles and gradually became less distressed. However, we recognized a usually unarticulated tension between friendships and the goal of research.[3] The researcher's goal is always to gather information; thus the danger always exists of manipulating friendships to that end. Given that the power differences between researcher and researched cannot be completely eliminated, attempting to create a more equal relationship can paradoxically become exploitation and use. We recognized this more as the research progressed and tried to avoid it.

During the interviews we were also often asked for information, which we provided. We viewed this as an additional opportunity to reciprocate for the help these women were giving us by participating in the study. Now, at this stage of the writing, we continue to have feelings of obligation to the women we interviewed—to finish the writing and find a way to publish our—their—material. If we do not do this, we will have failed on our part of our joint project.

A high degree of participation in the research was not established with all interviewees. As we noted above, repeated interviews resulted in more involvement. However, not only the number of interviews but also the experiences women were having at the particular time that

we first interviewed them influenced our contact. With those women who experienced this period as a critical period in their lives, we seem to have established the best rapport. Although our lives differed from most of the women we interviewed, with many we shared a sense of uneasiness, an experiencing of dilemmas and contradictions as well as a willingness to acknowledge them.

Another way that we tried to overcome the distance between researchers and researched was to show our written material to the women we wrote about. We did not do this with every woman in the study. We shared most of this material with the women with whom we had the most interviews, who were those who identified themselves as consciously trying to change. Since change was the central issue of the study, there was a theoretical rationale for spending more time with them. And, given the focus on change as well as our limited time and other resources, it made more sense to ask these women to reflect on our written material. They were, as we mentioned above, also women who most shared our worldview; a common frame of reference provided the grounds from which a dialogue could proceed. We have to admit to some reluctance to share our interpretations with those who, we expected, would be upset by them. There was a potential conflict between our feminist frame of reference and their interpretations of their own lives. Our solution to this conflict was not to include them as active participants in the analysis of our research. Whether or not to confront groups or individuals with interpretations of their lives that are radically different from their own is an ethical question faced by anyone attempting critical social research. This is particularly true when the researcher's interpretation is not only different but potentially threatening and disruptive to the subject's view of the world. For example, many of the women who were housewives defined themselves as very independent whereas our perspective defined the conditions of their lives as creating both a structural and personal dependence. These housewives had a stake in their own definition that was also a source of worth and dignity, while we as feminist researchers interpreted their situations differently. At that moment, we were dealing with a tension between the goal of reducing the power differences between the researcher and the researched and the difficulties of carrying this out when there was a lack of agreement on the meaning of experiences. We have not solved this problem; we believe that the solution lies in accepting the dilemmas and maintaining an awareness of when and why we are not able to make the research process a true dialogue, this giving full legitimacy to the subjectivity of the Other as well as to our own. At least then we can articulate the difficult bal-

ance between granting respect to the Other's interpretation of her reality, while going beyond that interpretation to comprehend its underlying relations.

Problems of Analysis

As we pointed out, our commitment to minimizing the power differentials of the relationship in the research was further confounded when it came to the analysis. We found that we had to assume the role of the people with the power to define. The act of looking at interviews, summarizing another's life, and placing it within a context is an act of objectification. Indeed, we the researchers took the position that some process of objectification of the self is a necessary part of coming to an awareness of one's own existence; it is not less a part of coming to an understanding of others. Acknowledging that a necessary part of understanding another's experience involves an act or moment of objectification poses further problems and contradictions. The question becomes how to produce an analysis that goes beyond the experience of the researched while still granting them full subjectivity. How do we explain the lives of others without violating their reality? This is part of a larger problem: a critique of objectivity which asserts that there can be no neutral observer who stands outside the social relations she observes can easily become a relativism in which all explanations are subjectively grounded and therefore have equal weight. When all accounts are equally valid, the search for "how it actually works" becomes meaningless. Though we don't claim to have resolved this problem, we tried to avoid it by claiming a validity for our analysis (see discussion below) while not in the process forcing that analysis into categories such as worker, housewife, or mother and divorced or married, which fracture women's experience.[4] However, in the actual task of analysis, we initially found ourselves moving back and forth between letting the data "speak for itself" and using abstracted categories.

Our feminist commitment had led us to collect data that were difficult to analyze and had provided us with so much information that it was difficult to choose what was "essential" at the same time that we tried to give a picture that provided a "totality." Our solution to this series of problems was to present a number of life histories, expressed largely in the women's own words, to typify what we thought were particular patterns of change. We based these patterns on apparently discrete categories such as whether change was occurring and how it was initiated.

This attempt to make sense out of our information by placing the

women into categories of "changers" and "nonchangers" obscured the complexities of women's lives. Although it was possible to categorize women using simple and rigid criteria, the boundaries between changers and nonchangers were not at all clear. We at first called all women who were enrolled in school or were looking for a job "changers." But interviews revealed that some of the nonchangers were going through an active process of rethinking their lives while some of the changers (a small minority, but nevertheless bothersome in terms of a neat analysis) were actively resisting all but very superficial changes.[5]

We were pushed to develop our analysis further by women in the study whom we asked to read the manuscript. They were hesitant about being negative, but were clearly critical. What they wanted, they said, was more of our own sociological analysis. They wanted us, the researchers, to interpret their experience to them. Here, once more, we faced incompatibilities between various components of our feminist approach to social research. If we were to fulfill the emancipatory aim for the people we were studying, we had to go beyond the faithful representation of their experience, beyond "letting them talk for themselves," and put those experiences into the theoretical framework with which we started the study, a framework that links women's oppression with the structure of Western capitalist society.

Both the ways in which we were categorizing experience and the kinds of categories we then developed were still somewhat antithetical to our theoretical position. We experimented with dividing our interviewees into housewives and workers for the purpose of analysis. We had tried to only recruit housewives for the study, but—not surprisingly—found that about half the women we interviewed had had considerable work experience. Almost all of them had continued to see themselves as housewives. How should we see them? What is the critical cutting point in work experience that can tell us how to differentiate? We came to the obvious conclusion (Acker 1978) that the stationary concepts of housewife and paid worker are problematic ones. Most women move from unpaid work to paid work during their lifetimes and only a few fit totally within the unpaid work/paid work dichotomy, as quantitative data clearly show (Maret-Havens 1977). Our concepts do not reflect the reality of women's lives; this was demonstrated to us again in our qualitative data. Our initial use of traditional categories, despite our own feminist critique of them, illustrates the power of conventional ways of thinking about the social world and the difficulty of breaking out of its boundaries.

Another difficulty we faced was the difficulty of conceptualizing process. We first tried to solve this and the categorizing problem together by thinking up categories of ways that women engaged in the change process. Thus, we tried talking about those who initiated change in an active way as contrasted with those who were forced into changes by outside events. We soon found that this categorization fell apart as we looked in-depth at the actual processes. We also rejected a life cycle perspective partly because of its biological determinist implications and partly because we could not find a common pattern among the women we interviewed in the time that change began in either the family or the individual life cycle.

We also attempted to categorize feminist consciousness and, to some extent, were successful.[6] However, again, the boundaries were unclear and we felt that the strategy of analysis was not productive. We were not gaining any new insights nor deepening our understanding of the relationship between the individual and social structure and it was in this part of the research process that these connections had to be made explicitly.

At the same time that we were trying to find some fruitful categories in which to group our interviewees, we were analyzing issues or themes in the interviews. The contradictions between our commitment to a dialectical analysis, our aim of reconstructing women's experience in a way that accounts for both their and our explanations of that experience and the relation between the two, and our actual use of rigid categories sent us back to our theoretical beginnings in Marxism, feminism, and critical theory. We saw that the themes of everyday life we were identifying could be understood as manifestations of contradictions or dilemmas inherent in the underlying social relations. We explore the nature of these relations in our account of the research itself (see, e.g., Acker, Barry, and Esseveld 1981). As the analysis proceeded we tried to understand what was changing in these women's lives and whether or not the underlying dilemmas we had identified were being resolved or were reappearing in new ways, as the specific conditions of their lives were altered in a society that was both changing and remaining static. In the process of analysis, we refined and reshaped our initial questions, trying to make the act of objectification analogous to a moment of critical reflection. The concepts and questions that are central in our final report are different from those with which we started. We know that this is the history of many other research projects, although usually it is an unwritten history. We expected to work in this way, but if we understood beforehand how long and difficult the process would become, we might have more consciously and more quickly worked out strategies of analysis.[7]

Problems of Validity

The research perspective outlined in the first section of this essay makes problematic the conventional way of evaluating the products of the research. How shall we decide whether what we have done—the knowledge we develop—is worthwhile? How shall we decide if what we say is true? The first question about the development of worthwhile knowledge has to be answered in terms of an emancipatory goal. We might ask whether our findings contribute to the women's movement in some way or whether they make the struggles of individual women more effective or easier by helping to reveal to them the conditions of their lives. We know that this is the case for some of the women in our study. This is also a historical question that can only be resolved in the future. An emancipatory intent is no guarantee of an emancipatory outcome. Perhaps the best we can do is to guard against our research being used against women, although that also may be difficult.

The second question, how to decide what is true or valid, is one we have in common with all social scientists. We differ, however, with many of them in how we conceive of this truth. We are not interested in prediction, but adequate reconstruction (Schutz 1963). We conceive of this at two levels. The first has to do with adequacy of interpretation and involves selection, organization, and interpretation of our findings with the help of our social theory. The other level of concern is with the adequacy of our findings. We want to know that our research results fairly and accurately reflect the aspects of social life that we claim they represent.

If validity is to be judged by the adequacy of interpretation, we must return to our theoretical orientation to determine the criteria of adequacy. This, as briefly discussed above, is a position that has its origins in feminism, Marxism, and critical theory and that is working toward a sociology *for* women. The first criterion of adequacy in this approach is that the active voice of the subject should be heard in the account. Our interpretations should avoid transforming the acting and thinking human being solely into an object of study, while recognizing that some objectification is inherent in the process of interpretation or reconstruction. Moreover, seeing persons as active agents in their own lives, we will not view them as totally determined or lacking in comprehension of the social world. For example, we consider the concept "false consciousness" inadequate as part of a valid interpretation.

A second criterion for adequacy is that the theoretical reconstruction must be able to account for the investigator as well as the investigated. The interpretation must locate the researcher in the social structure and also provide a reconstruction of the social relations that produce the research itself. For example, what are the social relations that produce this research situation and the enterprise of research itself? What makes it possible to raise the research problem at this time, in this place, in this society? What are the processes that have resulted in the researched and the researchers coming together in a particular kind of social relationship? Such a reconstruction should be possible, in principle, although we do not argue that every research report should spell it out in detail.

Our third criterion for adequacy is that the reconstruction should reveal the underlying social relations that eventuate in the daily lives we are studying. This is the heart of the idea of a sociology *for* women; we want to understand how the underlying organization of actions and practices results in the ordinary daily lives of women. This is a complex task, perhaps an impossible task. For example, to trace back from the daily experience of a working-class mother, getting three or four children ready for school in the morning, unassisted by another adult, packing lunches, buttoning coats, and so forth, to the arrangements and relations that put her there, would be to describe much of the organization of the society. Thus, we need to make decisions and choices; this is part of the process of analysis that we have discussed above.

The adequacy of the interpretation fundamentally depends on the accuracy of our descriptions of the experience we wish to locate within the social relations of the society. Have we told it the way that it actually happens? This is the question we turn to now.

Our research problem demanded that we try to understand reality from the perspective of the people experiencing it. Since we directly asked them about their experience we did not have the problem of developing indicators of concepts. Rather, we wanted to maximize direct communication in their terms. We assumed that our study participants would have a better chance of telling us about their worlds as they saw them if their active participation in defining the dialogue was encouraged. As we have indicated above, we are confident that in most of our interviews the interviewees felt comfortable about stating their own cases.

In qualitative work, the accuracy of listening and hearing may be as important as the openness of telling. The fact that we, the interviewers, were women who have been married, divorced, and had children (one of us had a baby after the study began) increased the validity of our data. We did not have to go through the process of getting to know the special perspectives and nuances of meaning of those we were

studying—a process that is often identified as necessary if the qualitative researcher wants to avoid errors that simply come from ignorance (see, e.g., Filstead 1970). We were studying people who had experiences very similar to ours, although of course there were important differences (the most important being our status as researchers) and we were, thus, sensitive to problems and issues that might otherwise have been invisible.[8]

We think that it was also important that we were feminists. Our feminist analysis of women's oppression, which constituted much of the theory informing our work, also increased our sensitivity and awareness in the interview process and contributed to the emergence of an empathetic atmosphere in the interaction process. A faithful account is best pursued, we are arguing, in research such as ours in which changing consciousness is the central question, through the close and sympathetic involvement with the informant rather than through distancing and objectifying. At the same time such closeness may create certain kinds of blindness in the researcher. One protection we developed against this was in the ongoing process of analysis in the research group. Our analytic discussions, of necessity, forced us to distance ourselves from our subjects.

We have confirmation of the accuracy of our findings from those women we interviewed. We received extensive feedback from many of them in both individual and group discussions. Some read their interviews or listened to their tapes. We also discussed our written material with many and in those discussions our findings and our interpretations were confirmed.

Much more difficult problems of validity began to emerge in the interviews that were continued over a period of four years. These problems have to do with how reality is constructed and reconstructed in the process of talking and thinking about it and how the process of research becomes part of the process of change. We will leave the psychological aspects of the reconstruction of reality to the psychotherapeutic professions. Here, we will limit ourselves to specific methodological issues and only discuss the content of the change process in relation to them. Our insights into these issues come from women in our study and in particular from one person whom we asked to participate in a workshop on developing a feminist methodology in social science.[9] Her comments on the experience of being the researched tell us a great deal about the validity of interview or questionnaire data. We will give a brief account of her experience as a background to further comments on validity.

Joanne began the first interview with a positive attitude toward the

research. She knew that the objective was to contribute to the goal of women's liberation; she herself had a feminist orientation and was anxious to cooperate. During the interview, she did her best to be open and honest. The interview lasted for three hours and both the interviewer and Joanne thought it was a good interview. However, reflecting on what she had said during the next few days, Joanne realized that she had omitted some very important aspects of her life and had unintentionally misrepresented others. She felt that the account she had given was chaotic, unclear, and disorganized. In the months between the first and second interviews, she thought about her life, tried to clarify events, relationships, and feelings; in the second interview she discussed herself within this altered point of view. Still, reviewing later what she said, she again was dissatisfied with the accuracy of her presentation of her current and past life. Once more, she went through a process of self-examination and rethinking. The third interview was somewhat better, but she was not yet satisfied. Only after the fourth interview did she begin to feel that she was portraying her life as she actually lived it. By the fifth interview she had arrived at a coherent explanation of her experiences. Joanne said that this was the first time in her life that she was able to put together a reasonable account for herself. She believes that her first accounts were chaotic and disorganized because that was the way that her life was, filled with multiple and conflicting demands from her husband, her five children, her jobs, her volunteer work in the community, and her friends. In the research process, between interviews, she spent long hours analyzing those relationships; her work on herself was part of the work on the research. She, as the researched, was constantly checking out the validity of the data she was giving us, the researchers. But, in the process, these data changed in some ways. The facts of the past were not altered, but they were elaborated and important omissions were filled in. Her own definition of what was important also changed in the process. Although it seems the best validity check is to have the study participant determine accuracy, one could also argue that the first interview might have reflected her conscious assessment of the reality of her life at that time, while the fifth interview reflected an equally valid picture at a later time. Are we, thus, getting a more and more valid account or are we getting several accounts that reflect the process of change? Certainly for Joanne, the interviews were part of a change process in which she was trying to deal with fundamental contradictions in her life situation. Her understanding of her present dilemmas became clearer too, clearer in that she was more satisfied with them.

Such problems have been discussed many times by others who have

argued that retrospective accounts are suspect in terms of validity. For example, Howard S. Becker and Blanche Geer stated: "Changes in the social environment and in the self inevitably produce transformations of perspectives, and it is characteristic of such transformations that the person finds it difficult or impossible to remember his [*sic*] former actions, outlook or feelings. Reinterpreting things from his [*sic*] new perspective, he [*sic*] cannot give an accurate account of the past, for the concepts in which he [*sic*] thinks about it have changed and with them his [*sic*] perceptions and memories" (1957:31). We take a different position, arguing that both the past and the present accounts are accurate. The first account was, we think, a true representation of Joanne's conscious thoughts about her life at that time, with all the things she forgot, held back, and interpreted in ways she thought would be acceptable. But now that we have her own analysis of the process we cannot take it as adequate "data" about her life history or her present situation. Her interpretations at the first interview were more narrow than the broader perspectives she had during the fifth interview, which was informed by social theory and by the interactions with the researchers. At this moment we have left our discussion of validity in a narrow sense and returned to our view of science in which an emancipatory goal is an essential part (Touraine, Hegedus, Dubet, and Wieviorka 1980). To return to our discussion: we should not take that first interview as filled with "error," although critical omissions may make our interpretations suspect. Especially painful memories or difficult experiences may be obscured—events such as the birth of an illegitimate child put up for adoption, abortion, rape, or an illicit love affair may be clouded over or simply seen as unimportant when from the point of view of the outsider, they are critical to understanding a life. These obscured experiences are central to the systematic devaluation of women in a male-dominated world. A feminist perspective redefines these experiences as part of women's oppression, helping women to see their feelings as legitimate and eroding the taboos against discussing such life events. Moreover, distance and some confidence in the interviewer that has been built up over time may make it possible to reveal such events while altering the ways that they are assessed by the person who experienced them.

Unless a relationship of trust is developed, we can have no confidence that our research on women's lives and consciousness accurately represents what is significant to them in their everyday lives and thus has validity in that sense. This is particularly true if we are trying to understand lives in their totality, as ongoing processes in which the person plays an active part. Certain survey data becomes, then, even more suspect. We have difficulty in assessing the validity of even the most factual data, to say nothing of data about opinions and attitudes. Even "in-depth" interviews present problems of interpretation, as the above discussion indicates. We are probably faced with another unresolvable dilemma: working from a perspective in which we are trained to want to give a reasoned and connected account, we face live material that is constantly in the process of transformation, that is not organized in the way of academic theories. Virginia Woolf, among other novelists, may give a better account of the conscious experiencing of life in all its episodic and unorganized ways than we sociologists can achieve. However, as sociologists we can find representations of such experiences that allow us to build a sociology *for* women, a sociology that connects experience at that level to its structural determination in the wider society. What distinguishes us from those who are not social scientists lies in our method of systematically attempting to reconstruct social reality and to put these systematic reconstructions into a social theory that we share with other social scientists. We are part of a group endeavor to understand society, even though the group is scattered and many of its members remain unknown to us.

SUMMARY

In this essay we have discussed our attempt to use principles of feminist research in a systematic way as we carried out a research project. For us, feminist research should contribute to the liberation of women. We chose our research problem with this goal in mind. The problem was the relation between changes in consciousness and changes in the structural situation of individuals. Women who were at the end of their period of intensive mothering were the ones we chose to study. Our problem dictated qualitative data gathering. This method of data collection forced us to confront issues about the research relationship and influenced our data analysis. It was extremely difficult to analyze process, even though we had at least some relevant data. We still tended to look at our participants at one interview and then at the next, observing the changes but unable to adequately account for the intervening process. Yet, that process may be most important to understand if we are to comprehend the ways the larger structure penetrates the life of individuals, as well as the ways that individuals in their daily lives both reproduce and undermine that structure.

Our commitment to bringing our subjects into the research process as active participants[10] influenced our rethinking of our original categories, strengthened our critique of research methods, and forced us

to realize that it is impossible to create a research process that completely erases the contradictions in the relation between researcher and researched.

In the relationship with those women who were actively changing both their life circumstances and their understandings of their lives, we were able to glimpse the research process as consciousness-raising or emancipatory. Many of them told us that they experienced the interviews in this way. However, the emancipatory potential could only be partially attained even with those who were most aware of subjective change. The limits were in the restricted possibilities for satisfying work and financial independence facing all the women in our study. The research process was not consciousness-raising for those whose life situation had not brought them into contact with the movement nor confronted them with the necessity to reflect upon their experiences. These were the women who were not in the process of trying to establish new forms of daily life and those whose interests seemed to be furthest from ours, the researchers. As we evaluate our experiences in interviewing these women, we are led to another dilemma of feminist research—should we do research that is not consciousness-raising for the participants? Is such research an oppressive process that of necessity exploits the subject? If our answers to these questions are yes, we are faced with the possibility of only doing research with people who are very much like us, eliminating most women from our view and limiting the usefulness of our projects. Perhaps this is another necessary tension in the ongoing project of feminist investigation.

We have not solved the problems of doing emancipatory research. By trying to make our hopes and failures explicit, perhaps we have made a contribution toward that end.

AFTERWORD

On rereading this essay, originally written in 1980, our reactions were mixed: much of it still represents our views on problems in doing feminist research. But, at the same time, on some issues our thinking has changed and our different experiences in the intervening years have led us to identify diverse aspects of our earlier argument that we would change if we were rewriting it today. Although we did not always agree with each other, we did agree that this piece should be understood as belonging to a particular historical time and place.

"Objectivity and Truth" was written when the women's movement was still in its exciting days of rising consciousness and expanding possibilities. Our essay reflects this optimistic mood in its emphasis on

the goal of emancipation, defined as the end of social and economic conditions that oppress women, and in its belief that a sociology *for* women could contribute to that goal. This essay was part of a process of creating a context of feminist practice and research and claiming legitimacy for such research. While we three have not abandoned these goals, we see the issues as much more complex and difficult to resolve. The years of promotion of competitive capitalism with its lack of concern for human costs and attacks on equality efforts, as well as the ensuing economic depression, have dampened optimism about change. At the same time, the clear relationship that we felt existed between feminist scholars within the university community and the women's movement has become attenuated and, in many ways, transformed. As feminist scholarship has become institutionalized within the academy, increasingly abstract theoretical concerns seem to have less and less to do with practical problems of women's everyday lives. Moreover, feminist arguments have become more complex as women from many different classes, races, and ethnic groups define their own feminist positions and issues. The women's movement is no longer cohesive and easily identified. Increased understanding of these complexities of social reality creates problems for theory and action. Consequently, today we would be more cautious about the possible role of sociology in helping women to change our situations.

We have two criticisms of our essay in this regard. First, although we think our understandings were more complex, our essay could be read as locating the major problem of theory in absence of women and gender from sociological thinking. Now we would emphasize that the issues are who is doing the theorizing, from what standpoint(s), and with what practices and procedures. Second, while in our essay we are critical of theory developed from a universalistic male perspective, we counterpose to this a woman's perspective that also is universal. Our assumptions of a universal, and mostly heterosexual, woman's standpoint pervade the essay. We also assume that we, the researchers, as well as the mostly middle-class, all white interviewees represent this universal perspective. Today, our theoretical beginnings would be much more complex and would attempt to recognize the multiple diversities of women's experience. This would influence our discussions of methodological issues as well, such as the possibilities for the establishment of mutual subjectivity and for erasure of power differences between researcher and researched.

Our discussion of the research relationship, then, would be more complex today. We focused on a relationship in which the researcher has more power than the researched. We wanted to make this rela-

tionship as nonexploitative as possible and tried to do this through creating conditions in "which the object of research enters into the process as an active subject." Although we discussed the dilemmas and contradictions in trying to do this, our different intervening experiences have led us to see even more difficulties in dealing with power in the research relationship. Differences in class, race, and age are even more difficult to transcend than we thought. In addition, the very techniques we used to increase the subjects' active participation may have worked in a way opposite to what we intended. For example, establishing reciprocity, even friendship, in the relationship may set up expectations on the part of the interviewee that the researcher cannot possibly meet.

Follow-up interviews conducted after this essay was published reveal another problem. Some of those we interviewed felt that we researchers had expectations of them that they had failed to meet. They had not been successful and they were disappointed. In contrast to them, one of us, Joan Acker, was about the same age as many of the respondents and was, by their definition, a success (a professor). We felt that, for some, reluctance to talk about reversals of fortune were linked with an implicit comparison of themselves with Joan. A more distant, traditional, objectifying research relationship might not have generated these difficulties.

Our essay implies that empathy is a necessary component of a feminist research relationship. Today we would say more emphatically that empathy is not always possible, nor is it a defining quality of feminist research. Feminist researchers may work on topics that require interviews with women and men who have more power or different political aims than the researcher. Such a relationship is different from the ones in our study because the powerful subject has no difficulty in being active and determining the parameters of the interview. Empathy might be impossible in such a relationship. However, a feminist researcher would still need to recognize the researched as an active subject and to comprehend the effects of the social locations of both the researcher and the researched on the process and content of the interview.

We also assumed in our essay that only women who are feminists can do emancipatory research. The three of us had some disagreements on this. Two of us maintained that while we do not propose as a universal principle that only women can interview women, for many feminist research issues women interviewers can establish more productive and less threatening research relationships than can men. There is a potential connection of experience between women and an ability to be self-revelatory about shared life events that is less likely to be

present with a male interviewer. One of us thought that, in principle, men can establish as nonexploitative and supportive research relationships as women and can elicit the same information.

We want to point to the parts of our discussion that are still central to all our thinking or that, we believe, are still important contributions to feminist research methodology. The idea of social relations and processes enunciated here still constitute the understanding of individual experience and social structure for all of us. We still "see individuals' activities in daily life as producing their social worlds; yet at the same time we recognize that there is an underlying organization of these activities that results in similar outcomes." We still think that feminist sociology is about explicating the interrelations between daily experience and the social relations that underlie it. This is one reason that the object of research must enter the process as an active subject, for this is the only way to discover what it is we need to understand.

In our view, probably the most substantial contribution of this essay is in our discussion of the problems of analysis and establishing validity. We tried to get away from the procedures of categorization and classification that are often the first moves in an analysis of sociological data. In addition, we continually attempted to reflect upon our own social locations and how those locations influenced the research relationship and the information we elicited.

We defined validity in terms of our feminist research perspective and our theoretical approach. This translated into assessing the adequacy of interpretation and the adequacy of our findings. We tried to spell out the criteria of adequacy in these two meanings, recognizing that the procedures for satisfying the criteria will vary with the research perspective and problem. Today, on the basis of further experience, we might modify what we had to say about procedures. We would not emphasize so much the contributions to accurate description of similarities between the life experiences of the researchers and the research subjects. While similar experience may minimize mistakes from ignorance, they may also lead to untenable assumptions of congruent experience. In addition, sometimes a sympathetic outsider can see patterns that are invisible to those whose daily lives are embedded in such patterns.

In our experience, the issues of how to do feminist research are far from solved. There is still the issue of how to do research that is not exploitative, how to establish research relationships in which there are two active subjects. Can that be done across lines of class, gender, and racial/ethnic differences and what procedures can help?

How we can establish validity is another unfinished issue. Accura-

cy of description can be partially checked by asking respondents to read their interviews and comment on them. But, as we found, we were not so comfortable asking those we interviewed to assess the adequacy of interpretation, particularly if they were likely to disagree with us. However, if we maintain our commitment to seeing the researched as active and competent subjects, we might ask about the meaning of such disagreement and the social practices that produce it, leading us to more adequate interpretation. As we move more and more from studying women's experiences as oppressed into studying the social relations that organize that oppression, we need to talk with those who have the power to shape some of those relations. Should we take such care with employers, welfare administrators, and the police? And how does gender influence the research relationship when the researcher is a relatively powerless woman and the researched a relatively powerful man? What are the possible effects of this power arrangement on the accuracy of description?

In conclusion, we would like to reiterate that, in our view, postmodernists' objections notwithstanding, our most distinctive contribution in this essay was our attempt to explicate feminist criteria for establishing validity or "truth." Many others are trying to deal with this issue, but it remains unfinished business.

NOTES

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1. The term *feminist* refers to diverse groups of people who take varying positions on particular issues and who identify with a range of political positions. In our usage here, *feminist* refers to a point of view that sees women as exploited, devalued, and often oppressed; is committed to changing the condition of women; and adopts a critical perspective toward dominant intellectual traditions that have ignored or justified women's oppression. Some people who identify themselves as feminists accept the natural science model of sociology.

2. The term *social relations* here signifies a particular epistemology derived from the Marxist tradition and is not equivalent to the notion of social relationships. We are not referring to interactions between individuals. Rather we see individuals' activities in daily life as producing their social worlds; yet at the same time we recognize that there is an underlying organization of these activities that results in similar outcomes. This organization is what sociologists call social structure, but this is usually conceptualized as a fixed determinate abstract category that is apart from or radically other than individual action. The term *social relation* is a way of overcoming this dichotomy: to give centrality to the organization of social life without positing either "the individual" or "the social structure" as separate and oppositional. See the work of Dorothy Smith (1977, 1979, 1980) for a feminist interpretation of this concept.

3. Daniels (1967) discusses a similar problem.

4. For a perceptive discussion of the need to reconceptualize social structure in ways that do not push women's experience into categories that are no longer reflective of that experience, see Kelly (1979).

5. This attempt to categorize was related also to our initial statement of our problem, the relationship between certain "exterior" changes—going to school or work—and certain "interior" changes—consciousness of self as a woman who exists in a particular world and the interpretations of that world.

6. We used two categories, personal feminism and political feminism. These are explained in Acker, Barry, and Esseveld (1981).

7. Our commitment to doing feminist research and thus the attempt to do away with the hierarchy so often present in research may have prolonged the time the research has taken us as well. We tried to work in a nonhierarchical way as a research team and also tried to do all the necessary work. This included transcribing interviews ourselves, with some positive and negative results.

We three researchers with different theoretical traditions and with specialization in different areas of sociology had the same interests and political goals. By working closely together we developed the central concerns of our research. During the discussions we also developed a common theoretical perspective in which no person attempted to dominate or impose her own views. Differences in interpretation could then be more democratically resolved. This way of working was often a long drawn-out process, but we believe a necessary one for working with the kind of questions in which we were interested. The research process was also prolonged by our decision to do all the work ourselves. This meant that we would do the transcribing, as we believed that to be one of the most oppressive tasks in the research. We did the transcribing during the few extra hours left us after we had taken care of teaching, work loads, and family responsibilities. It was a tedious process, especially since we were not trained transcribers. Eventually we decided to have some of the interviews transcribed or listened to the tapes and noted down topic areas and their locations on the tapes.

Looking back, we may have overemphasized the overcoming of hierarchy and may have lost some of the expert knowledge and differential experience in the group. It might have been better to include a transcriber in our project instead of trying to deal with the oppressiveness of transcribing by doing the work ourselves.

8. Taking the position that the idea of the neutral observer is a false assumption has implications for validity. The researcher does not stand outside social structure. Her location in society enters into the research relationship. To rec-

ognize and take this into account as we did contributes to a better understanding of reality and greater validity.

9. We wish to thank Joanne Ferrero for contributing her perceptive insights to our workshop on feminist methodology and to this essay.

10. The research process affected us as researchers and in our own lives. Our role as researchers was greatly changed because of the more active involvement of the women in our study, something that became especially clear during the analysis when our interpretations were being questioned. During the interviewing, we faced a tension between being expected to take the initiative and wanting more of a dialogue. Personally, it helped us to reflect on our own situations and influenced future personal choices.

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CHAPTER 4

JUDITH STACEY

Can There Be a Feminist Ethnography?

Most feminist researchers, committed, at a minimum, to redressing the sexist imbalances of masculinist scholarship, appear to select their research projects on substantive grounds. Personal interests and skills meld, often mysteriously, with collective feminist concerns to determine a particular topic of research, which, in turn, appears to guide the research methods employed in its service. Indeed, in such a fashion, I chose my dissertation project, a study of patriarchy and revolution in China designed to address major theoretical questions about Western feminism and socialism. The nature of this subject, compounded by limitations in my training, necessitated the macrostructural, abstract approach based almost exclusively on library research that I adopted. And, as a consequence, its textual product offered an analysis of socialism and patriarchy that, as several reviewers justly complained, left out stories about actual women or patriarchs (Stacey 1983). My dissatisfaction with that kind of research process and outcome led me to privilege methodological considerations over substantive interests when I selected my next research project, a fieldwork study of family and gender relationships in California's Silicon Valley. I was eager for a "hands on," face-to-face research experience, which I also believed was more compatible with feminist principles.

When I began my Silicon Valley research project in 1984, the dominant conception of feminist research among feminist scholars advocated research on, by, and especially *for* women and drew sharp distinctions between the goals and methods of mainstream and feminist scholarship.[1] Feminist scholars had begun to express widespread disenchantment with the dualisms, abstractions, and detachment of positivism, rejecting the separations between subject and object, thought and feeling, knower and known, and political and personal as well

as their reflections in the arbitrary boundaries of traditional academic disciplines. Instead most feminist scholars advocated an integrative, transdisciplinary approach to knowledge that grounds theory contextually in the concrete realm of women's everyday lives. The "actual experience and language of women is the central agenda for feminist social science and scholarship," asserted Barbara Du Bois (1983: 108) in an essay advocating "passionate scholarship," and only a minority of feminist scholars ventured a dissent. Indeed feminists were celebrating "feeling, belief, and experientially based knowledge," which draw upon such traditionally feminine capacities as intuition, empathy, and relationship (Stanley and Wise 1983a). Discussions of feminist methodology generally assaulted the hierarchical, exploitative relations of conventional research, urging feminist researchers to seek instead an egalitarian research process characterized by authenticity, reciprocity, and intersubjectivity between the researcher and her "subjects" (Klein 1983; Mies 1983; Du Bois 1983; Reinharz 1983; Stanley and Wise 1983a, 1983b). "A methodology that allows for women studying women in an interactive process," Renate Duelli Klein argued, "will end the exploitation of women as research objects" (1983:195).

Judged by such criteria, the ethnographic method, by which I mean an intensive participant-observation study that yields a synthetic cultural account, appears ideally suited to feminist research. That is why in "The Missing Feminist Revolution in Sociology," an essay reflecting on the limitations of feminist efforts to transform sociology, Barrie Thorne and I (1985) wondered with disappointment why so few feminist sociologists had turned to the ethnographic tradition of community studies within the discipline, a tradition that seemed to us far more compatible with feminist principles than are the more widely practiced positivist methods. Many other feminist scholars shared the view that ethnography is particularly appropriate to feminist research (Klein 1983; Mies 1983; Reinharz 1983; Stanley and Wise 1983b). Like a good deal of feminism, ethnography emphasizes the experiential. Its approach to knowledge is contextual and interpersonal, attentive like most women, therefore, to the concrete realm of everyday reality and human agency. Moreover, because in ethnographic studies the researcher herself is the primary medium, the "instrument" of research, this method draws on those resources of empathy, connection, and concern that many feminists consider to be women's special strengths and that they argue should be germinal in feminist research. Ethnographic method also appears to provide much greater respect for and power to one's research "subjects," who, some feminists propose, can

and should become full collaborators in feminist research (Klein 1983; Mies 1983; Reinharz 1983; Stanley and Wise 1983a).

This, at least, is how ethnography appeared to me as I found myself unintentionally but irresistibly drawn to it in a study originally intended to be based on more conventional interview methods. An ethnographic approach seemed to resolve the "contradiction in terms" involved in interviewing women that Anne Oakley (1981) had identified in her critique of classical sociological interview methods. Oakley rejected the hierarchical, objectifying, and falsely "objective" stance of the neutral, impersonal interviewer as neither possible nor desirable, arguing that meaningful and feminist research depends instead on empathy and mutuality. And I was reassured by Shulamit Reinharz's assertion that the problems of experiential fieldwork methodology "seem minor in comparison with the quality of relations that I develop with people involved in the study and the quality of the understanding that emerges from those relations" (1983:185).

But after two and one-half years of fieldwork experience, I became less sanguine and more focused on the difficult contradictions between feminist principles and ethnographic method I encountered than on their compatibility. Hence the question in my title, which is modeled (but with a twist) on the implicit question in Oakley's "Interviewing Women: A Contradiction in Terms." The twist is that, after conducting feminist ethnographic research, I came to perceive the opposite contradiction between feminist ethics and methods than the one that Oakley discusses. I found myself wondering whether the appearance of greater respect for and equality with research subjects in the ethnographic approach masks a deeper, more dangerous form of exploitation.

There were two major areas of contradiction my efforts at feminist fieldwork exposed. The first involves the ethnographic research process, the second its product. Precisely because ethnographic research depends upon human relationships, engagement, and attachment, it places research subjects at grave risk of manipulation and betrayal by the ethnographer, as the following vignette from my fieldwork illustrates. One of my key informants had been involved in a closeted lesbian relationship immediately prior to the period of my fieldwork research. I first learned of this relationship from her spurned lesbian lover, and this only six months after working in the field. Of course, this immediately placed me in an extremely awkward situation ethically, a situation of triangulation and potential betrayal in relation to these two women and of inauthenticity toward the more secretive one. Several months later (partly, I believe, in response to her perception of my inauthenticity) this informant "came out" to me about this af-

fair, but she asked me to respect the confidentiality of this knowledge when relating to her relatives, friends, and co-workers. Moreover, she and her rejected lover began to compete for my allegiance, sympathy, and ultimately for my view of their shared history.

I could give numerous other examples (such as the case of a secret of paternity, of an illicit affair, and of illicit activities). All placed me in situations of inauthenticity, dissimilitude, and potential, perhaps inevitable, betrayal, situations that I came to understand are inherent in fieldwork research. For no matter how welcome, even enjoyable the fieldworker's presence may appear to "natives," fieldwork represents an intrusion and intervention into a system of relationships, a system of relationships that the researcher is far freer than the researched to leave. The inequality and potential treacherousness of this relationship is inescapable.

So too does the exploitative aspect of ethnographic process seem unavoidable. The lives, loves, and tragedies that fieldwork informants share with a researcher are ultimately data, grist for the ethnographic mill, a mill that has a truly grinding power. More times than I would have liked, my Silicon Valley study placed me in a ghoulish and structurally conflictual relationship to tragedy, a feature of ethnographic process that became particularly graphic during the death of another one of my key "informants." My ethnographic role consigned me to experience this death both as friend and as researcher, and it presented me with numerous delicate, confusing dilemmas, such as whether or not, and to whom, to make a gift of the precious, but potentially hurtful tapes of an oral history I had once conducted with the deceased. I was confronted as well with the discomforting awareness that as researcher I stood to benefit from this tragedy. Not only did the funeral and family grieving process serve as further research "opportunity" but also the death freed me to include more of this family's "truths" in my ethnographic account than would have been possible had he lived. This and other fieldwork experiences forced my recognition that conflicts of interest and emotion between the ethnographer as an engaged, related person (i.e., participant) and as an exploiting researcher (i.e., observer) are also an inescapable feature of ethnographic method.

The second major area of contradiction between feminist principles and ethnographic method involves the dissonance between fieldwork practice and ethnographic product. Despite the aspects of intervention and exploitation I have described, ethnographic method appears to (and often does) place the researcher and her informants in a collaborative, reciprocal quest for understanding, but the research product is ultimately that of the researcher, however modified or influenced

by informants. With very rare exceptions it is the researcher who narrates, who "authors" the ethnography. In the last instance an ethnography is a written document structured primarily by a researcher's purposes, offering a researcher's interpretations, registered in a researcher's voice.[2]

Here too, therefore, elements of inequality, exploitation, and even betrayal are endemic to ethnography. Perhaps even more than the ethnographic process, the published ethnography represents an intervention into the lives and relationships of its subjects. As author an ethnographer cannot (and, I believe, should not) escape tasks of interpretation, evaluation, and judgment. It is possible (and most feminists might claim it is crucial) to discuss and negotiate one's final presentation of a narrative with informants, but this does not eliminate the problem of authority and it can raise a host of new contradictions for the feminist ethnographer.[3] For example, after several years involving scores of hours of mutual reflections on the meaning of the lesbian relationship mentioned above, this "research collaborator" asked me to leave this part of her history out of my ethnographic account. What feminist ethical principles could I invoke to guide me here? Principles of respect for research subjects and for a collaborative, egalitarian research relationship demand compliance, but this forced me to collude with the homophobic silencing of lesbian experience, as well as to consciously distort what I considered to be a crucial component of the ethnographic "truth" in my study. Whatever we decided, my ethnography was forced to betray a feminist principle.

Indeed the irony I came to perceive is that ethnographic method can expose subjects to far greater danger and exploitation than do more positivist, abstract, and "masculinist" research methods. And the greater the intimacy, the apparent mutuality of the researcher/researched relationship, the greater the danger.

The account I have just given of the paradoxes of feminist ethnography is falsely innocent. I have presented my methodological/ethical quandaries the way that I first conceptualized them as a feminist researcher, innocent as then I was of relevant literature by ethnographers who long have grappled with related concerns. I am no longer so innocent and ignorant, but I retained this construction to help underscore a curious fact. Until quite recently, there has been surprisingly little cross-fertilization between the discourses of feminist epistemology and methods and those of the critical traditions within anthropology and sociology.[4] Most pertinent has been the dearth of dialogue between feminist scholarship, particularly within feminist sociology, and contemporaneous developments in the literature referred to as the

"new" or "postmodern" or "reflexive" ethnography.[5] This is curious, because the new or postmodern ethnography is concerned with quite similar issues as those that concern feminist scholars and, at first glance, it offers a potential resolution to the feminist ethnographic paradox.[6]

Postmodern ethnography is critical and self-reflexive ethnography and a literature of meditation on the inherent, but often unacknowledged hierarchical and power-laden relations of ethnographic *writing*.[7] Like most feminist scholars, critical ethnographers tear the veil from scientific pretensions of neutral observation or description. They attempt to bring to their work an awareness that ethnographic writing is not cultural reportage, but cultural construction, and always a construction of the self as well as of the Other. In James Clifford's words, the "historical predicament of ethnography" is "the fact that it is always caught up in the invention, not the representation of cultures" (1986:2).

The favored postmodern solution to the reflexive anthropological predicament has been to fully acknowledge the dialogic and discursive character of the ethnographic process and product and to deconstruct their own claims to ethnographic authority. Like most feminists, critical ethnographers eschew a detached stance of neutrality, and they perceive their subjects as collaborators in a project the researcher can never control. Moreover, they acknowledge the indispensably intrusive and unequal nature of their participation in the studied culture.

Even more than most feminist scholars, I believe, critical ethnographers have been excruciatingly self-conscious about the distortions and limitations of the textual products of their studies. Here they have attempted first to fully acknowledge and own the interpretive authorial self and second to experiment with dialogic forms of ethnographic representation that place more of the unassimilated perspectives of the researched into the narrative and that more effectively reflect the dissonance and particularity of the ethnographic research process.

Finally, postmodern ethnographers, influenced by deconstructionist fashions, aim only for "partial truths," as James Clifford suggests in his introduction to *Writing Culture*, which, ironically enough, has become the canon-setting collection of this genre: "Ethnographic truths are thus inherently *partial*—committed and incomplete. This point is now widely asserted—and resisted at strategic points by those who fear the collapse of clear standards of verification. But once accepted and built into ethnographic art, a rigorous sense of partiality can be a source of representational tact" (1986:7).

Until quite recently, the reflexivity and self-critique of "postmodern" ethnographic literature seemed to parallel, while keeping surprising

distance from, feminist methodological reflections. It seems likely that it unwittingly exploited some of the latter as well, as feminist social scientists had published compatible reflections on matters of the self, commitment, and partiality in research.[8] In fact, there were even rare moments in *Writing Culture* when "postmodern" ethnographers incorporated feminist insights into their reflexive critiques. Vincent Crapanzano, for example, suggested that "interpretation has been understood as a phallic, a phallic-aggressive, a cruel and violent, a destructive act, and as a fertile, a fertilizing, a fruitful, and a creative one," and he self-consciously retained the male pronoun to refer to the ethnographer "despite his or her sexual identity, for I am writing of a stance and not of the person" (1986:52). Yet, few feminist authors or works appeared in the primary texts that came to define postmodern ethnography, and this too-familiar form of exclusion and marginality fueled considerable suspicion, anger, and defensiveness among feminist ethnographers, some of whom nearly advocated a rejection of "the postmodernist turn."[9]

Although there were legitimate grounds for feminist suspicions that many "representations" of postmodern ethnography could serve as coded alibis for the reassertion of male authority, this helped to establish a rivalrous and dichotomous formulation of the relationship between feminism and reflexive ethnography that I find unfortunate and unproductive. For example, the implication that feminist ethnographies have been less experimental than postmodernist texts has either been denied by feminists or defended on the political grounds that we have been too preoccupied with challenging the appalling conditions of women's subordination to indulge in the luxuries of textual innovation.[10]

While I find considerable insight in such feminist rejoinders, I worry that ultimately they both cede and shun more ground than is advisable. Instead of cementing a competitive relationship between feminism and postmodern ethnography, we gain more when we take more seriously the parallel insights from both discourses about partial truth, situated knowledge, and multivocality. All oppositional ethnographic projects could benefit from constructing multiple genealogies of radical reflexivity, rather than insisting upon the priority or superiority of their own. For example, ethnic studies, feminism, gay studies, American studies, and even ethnomethodology each can produce diverse, but overlapping, narratives about the genesis of contemporary interest in reflexive ethnography.[11] These might prepare the grounds for more fruitful dialogue and exchanges among these discourses than has been typical thus far.

The feminist anthropologist Marilyn Strathern (1987) also noted the surprising paucity of constructive engagement between feminism and the new ethnography and, in an important initiation of such dialogue, offered an analysis of the grounds for mutual resistance that undergird what she termed the "awkward relationship" between the two. Feminism and critical anthropology, Strathern claimed, are mutually "vulnerable on the ethical grounds they hold to be so important": "each has a potential for undermining the other" because they rest upon incompatible constructions of the relationship between self and Other (1987:289). Feminism, Strathern argued, presumes an antagonistic relationship to the male Other, a presumption that grounds its acute sensitivity to power inequalities and has the power to undermine those anthropological pretensions of alliance and collaboration with the Other upon which new ethnographic strategies for multiple authorship reside. Anthropology, in turn, from its cross-cultural vantage point, suggests the illusory nature of feminist pretensions of actual separation from men of their own culture.

I view the resistances somewhat differently. Feminism's keen sensitivity to structural inequalities in research and to the irreconcilability of Otherness applies primarily, I believe, to its critique of research by men, particularly to research *by* men but *about* women. The majority of feminist claims about *feminist* ethnographic and other forms of qualitative research, however, continue to presume that such research occurs almost exclusively woman-to-woman.[12] Thus feminist researchers are apt to suffer the delusion of alliance more than the delusion of separateness and to suffer it more, I believe, than do most reflexive ethnographers. Recall the claims about empathy and identification between feminist researchers and the women they study and the calls by feminist scholars for an egalitarian research process, full collaboration, and even multiple authorship with which this essay began. Less defensive engagement with critical ethnographic literature has begun to temper certain feminist celebrations of ethnographic methods with the same salutary note of humility about the limitations of cross-cultural and interpersonal understanding and representation that have been fostered by critiques of white feminist theory leveled by feminists of color.[13] A fruitful dialogue between feminism and critical ethnography would continue to address their complementary sensitivities and naivetés about the inherent inequalities and the possibilities for relationships in the definition, study, and representation of the Other.

With the hope of promoting such a dialogue, I have begun a collaborative project with Judith Newton, a feminist literary critic, that combines textual and ethnographic approaches to a study of the po-

litical and intellectual trajectories of male "tenured radicals," a group that includes numerous cultural critics of ethnography. This paradoxical attempt to partially "study up" (white, heterosexual females studying diversely situated males), at the same time we "study across" (as tenured radicals ourselves), is an attempt to conduct ethnography as a dialogic, political intervention. It represents one, unavoidably ambivalent, feminist response to the postmodern ethnographic impasse.[14]

Indeed, I agree with Strathern that the relationship between feminism and ethnography *is* unavoidably ambivalent. I am less convinced than she of the virtues of this awkwardness, but I concur that it can only be mitigated, not effaced. Even an exhaustive, mutually beneficial exchange cannot resolve the ethnographic impasse concerning study of the Other that postcolonial conditions and feminist politics have exposed. Reflexive strategies are inadequate responses to the ethical and political issues endemic to ethnographic process and product that I have encountered and described. They acknowledge, but do little to ameliorate, the problems of intervention, triangulation, or inherently unequal reciprocity with informants; nor can they resolve feminist reporting quandaries. For example, acknowledging partiality and taking responsibility for authorial construction could never reduce my handling of the lesbian affair into a matter of "representational tact."

My response to the question in my title is that while there cannot be a fully feminist ethnography, there can be (indeed there are) ethnographies that are partially feminist, in both senses of the term, that is, accounts of culture enhanced by the application of feminist investments and insights. There also can and should be feminist research that is rigorously, culturally self-aware and therefore humble about the partiality of its ethnographic vision and its capacity to represent self and Other.[15] Moreover, even after my loss of ethnographic innocence, I believe the potential benefits of "partially" feminist ethnography seem worth the serious moral costs involved.

Indeed, as Carole Joffe has suggested to me, my initial assault on the ethical foundations of fieldwork may have been unduly harsh, a fairer measure, perhaps, of my prior illusions about ethnographic virtue than of ethnographic vice.[16] Certainly, as she, Shulamit Reinharz, and many other feminists assert, fieldworkers can and do form valuable relationships with many of those we study, and some of our unsolicited interventions into the lives of our informants are constructive and deeply appreciated. For example, a daughter of the informant whose death I mentioned above later consoled me on the sudden death of my own father and thanked me for having allowed her to repair her hostile relationship with her father before he died by helping her to

perceive his pride in and identification with her. Often fieldwork research offers particular research subjects practical and emotional support and a form of loving attention, of comparatively nonjudgmental acceptance, that they come to value deeply.

But then again, beneficiaries of such attention may also come to depend upon it, and this signals another ethical quandary in fieldwork, the potential for, indeed the likelihood of, desertion by the researcher.[17] Although I have maintained contact with the "key informants" in my Silicon Valley study, several presciently feared that the publication of my ethnography about their lives would sunder my interest in its subjects (Stacey 1990). Indeed, they have been, and felt, increasingly displaced in my ethnographic commitments by the male cultural critics featured in my current project.

Of course, rigorous awareness of the ethical and political pitfalls in the method enables one to monitor and then to mitigate some of the dangers to which ethnographers expose their informants, but no research or rhetorical strategy can grant absolution from the power/knowledge nexus. I conclude in this Talmudic fashion to leave the dialogue open, believing that an uneasy fusion of feminist and critical ethnographic consciousness may allow us to construct cultural accounts that, however partial, can achieve the contextuality, nuance, and insight I consider to be unattainable through less dangerous, but more remote research methods.

NOTES

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1. Perhaps the most comprehensive summary of the characteristic distinctions between these approaches that feminists draw appears in several pages of tables detailing contrasts between the two in Reinharz (1983:168–72).

2. For just this reason Klein, Mies, and, to a lesser extent, Stanley and Wise argue against this approach and for fuller collaboration between researcher and subjects, particularly for activist research in the tradition of Paulo Freire generated by and accountable to grass-roots women's movement projects. But, as Smart (n.d.) as well as Stanley and Wise (1983a, 1983b) recognize, such an approach places severe restraints on who and what can be studied and on what could be written, restraints that could seriously harm feminist interests.

3. Smart (n.d.) offers important reflections on the adverse implications of this ethical principle when feminists study, as she believes we should, the powerful and the agents of social control rather than their targets.

4. Critical reflections on the ethics and politics of fieldwork have a long history in both disciplines, and by now the literature is vast. For important examples from the past two decades, see Asad (1973); Emerson (1983); Haan, Bellah, Rabinow, and Sullivan (1983); Hymes (1974); and Thorne (1978, [illegible]).

5. Becker (1987) makes a similar point about the unfortunate paucity of exchange between critical traditions in sociology and post-structuralist anthropology in a review of Clifford and Marcus (1986).

6. A few feminist essays published after I first published this essay indicate that feminists, at least, have begun to engage the postmodernist ethnographic literature. I discuss and cite these developments below. Thus far there is less evidence of engagement with feminist literature by male anthropologists engaged in the postmodernist discourse.

7. A good sampler and bibliography of postmodern ethnographic criticism appears in Clifford and Marcus (1985). Other important texts include Clifford (1983), Crapanzano (1977), Marcus and Cushman (1982), and Marcus and Fischer (1986).

8. For examples of parallel feminist works, see Krieger (1985), Mies (1983), Rosaldo (1983), Smith (1987), and Stanley and Wise (1983b).

9. For feminist critiques of the appropriation and exclusion of experimental feminist ethnographic literature by male critical ethnographers see Gordon (1988); Mascia-Lees, Sharpe, and Cohen (1989); R. Chabram (1990); and hooks (1990:123–33).

10. Mascia-Lees, Sharpe, and Cohen challenged the definition of *experimental* by which feminists were excluded from the Clifford and Marcus collection (1985) and from the Marcus and Fischer bibliography (1986). Abu-Lughod (1990) developed the analysis of feminist political priorities in an essay with the same title as this one.

11. See, for example, A. Chabram (1990), Newton and Stacey (1992–93), R. Chabram (1990), and Pollner (1991). For a particularly thorough, constructive exploration of the intersecting genealogies of feminism and reflexive ethnography, see especially Strathern (1991:part 1).

12. Recent examples of this premise appear in the title and many of the essays in Gluck and Patai (1991). See, for example, Anderson and Jack (1991) and Minister (1991).

13. Strathern (1991) is the most explicit analytical engagement of these discourses (Minh-ha 1989; Visveswaren 1994). Behar and Gordon (forthcoming) promises to advance this project further.

14. I have described the personal, political, and theoretical antecedents to this project (1993). Newton and Stacey (1992–93) is the first publication from this project. We discuss some of the paradoxical fieldwork dynamics (forthcoming).

15. Strathern provides a provocative discussion of an earlier version of this essay and helpfully advances my notion of a "partially feminist" ethnography (1987:34–36).

16. Personal communication with author, 1986.

17. In her inimicably witty style, Arlene Kaplan Daniels (1983) discusses the etiquette of abandoning one's research subjects as well as other ethical questions in fieldwork. Because of the intensity and depth of the relationships formed, the problem of desertion seems more serious in long-term ethnographic studies than in those based on the more limited contact characteristic of other forms of qualitative research.

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